Proceedings of the Institution of Electrical Engineers

Institution of Electrical Engineers

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The One Hundred and Seventy-first Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 12th, 1888—Sir CHARLES T. BRIGHT, late President, in the Chair.

The minutes of the Annual General Meeting of December 16th, 1887, were read and approved.

The names of new candidates for admission to the Society were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Ernest Danvers.	Charles Lever.
Basil Gee.	C. H. B. Patey, C.B.

From the class of Students to that of Associates—
C. S. Dalton Fisher.

Donations to the Library were announced as having been received since the last meeting from Professor E. J. Houston; the India-Rubber, Gutta-Percha, and Telegraph Works Company,

Limited; the Iron and Steel Institute; Mons. E. Jacquez; the Kew Observatory Committee; the Radcliffe Library, Oxford; Sir David Salomons, Bart.; Professor John Tyndall; and Dr. G. C. Wallich; to all of whom the thanks of the meeting were unanimously voted.

Sir CHARLES BRIGHT: Gentlemen,—Before I finally take leave of you as your President, which I shall do very shortly, I have one pleasing duty to discharge, and that is to distribute the premiums awarded for papers read during the twelve months ending 30th June last.

The first premium, being the "Society's Premium," value £10, was awarded to Mr. A. E. Kennelly, Associate, for his paper on the "Resistance of Faults in Submarine Cables," and he has chosen the pocket sextant and a prismatic compass which you see on the table ; but, as he is abroad, they cannot be presented to him personally, and will have to be forwarded.

The next, being the "Paris Electrical Exhibition Premium," value £5, was awarded to Mr. W. E. Sumpner, Associate, for his paper on the "Measurement of Self-Induction, Mutual Induction, "and Capacity ;" and I have very great pleasure indeed in now handing to him the books he has himself selected, and hope he will favour the Society with further papers of equal value to the one I have referred to.

The next, being the "Fahie Premium," value £5, was awarded to Mr. C. T. Fleetwood, Member, for his paper on "Underground "Telegraphs." Mr. Fleetwood has favoured us with a previous paper on this subject, and he has dealt with it most thoroughly. I have great pleasure in handing him the collection of books which he has selected.

Now, the remaining duty I have is also a very agreeable one to me, and that is to introduce your new President. I am sure that, although we have had many very eminent men as Presidents of this Society, you have never had one more practical or a better President than I am going to present to you. At the same time, it is hardly necessary for me to speak of the merits of a gentleman who has been our Honorary Treasurer, and has looked after our funds, disbursements, and accounts, for so long and with

such benefit to the Society, and who, besides that, must be well known to almost everybody here on account of the high position held by him as Engineeer-in-Chief of the Post Office Telegraphs. Therefore I think I need not go through the formality—which I believe is necessary at Cambridge or Oxford before you can even speak to a man—of having an introduction to him.   I therefore simply vacate my seat in favour of Mr. Edward Graves.

The President, Mr. Edward Graves, then took the chair.

Professor G. FORBES : Before proceeding to listen to the discourse of our President, I wish to propose that we give a hearty vote of thanks to our late President, Sir Charles Bright, for the manner in which he has conducted the duties of that office.   I am sure that everyone present will feel that the dignity of the Society is upheld by having on the roll of its Presidents the name of a man who has been so intimately connected, as Sir Charles Bright has been, with the practical application of our science from its infancy ; and also that everyone here must feel that we owe him great gratitude for the able way in which he has performed the duties of the Presidential chair during the past year.

Sir DAVID SALOMONS : It is my pleasant duty to second the resolution that Professor George Forbes has proposed.   You all know that the task of a President is very difficult ; and I think that last year has been especially so, because it has been our Jubilee year as well as that of Her Majesty.   I do not think that anyone present can have the slightest cause to complain, however par-ticular he may be in his ideas, of the way in which our late President, Sir Charles Bright, has acted ; and, as those of us know who are behind the scenes, where we meet an hour before this time, he has done his duty in the Council room.   I have very great pleasure in seconding the resolution of Professor Forbes, and I am quite sure you will carry it by accla-mation.

The PRESIDENT : Gentlemen, it has been proposed by Professor Forbes, and seconded by Sir David Salomons, that a hearty vote of thanks be presented to Sir Charles Bright for his conduct as President of the Society during the past year

I have great pleasure in performing my first duty, and putting that proposition to the meeting.

The resolution was unanimously carried.

Sir CHARLES T. BRIGHT, in reply, said: I am extremely and most sincerely obliged for the kindly manner in which my name has been mentioned by Professor Forbes and by Sir David Salomons. I can only say that everybody who is put in such an honourable position as being President of such a Society as this must always try, at all events, to do his best; and, I suppose like most of our Presidents, I have endeavoured to do the best possible for the benefit of the Society. I thank you most heartily for the kind manner in which you have received the observations that have been made; and, as we are all anxious to hear what our new President has to say in his inaugural address, I shall not detain you any longer.

The PRESIDENT: Gentlemen, before I commence my address, I ought to thank you for the honourable position in which you have placed me. I do not feel that I am the right man in the right place: I feel somewhat like a square peg in a round hole; but I must try to accommodate myself to the hole, do my duties as I best can, and trust, at the end of the year, that you will have been satisfied with my endeavours.

ADDRESS.

According to the poetic mythology of ancient Greece, Minerva, fully grown and armed, sprang from the brain of Jove: Wisdom was perfect at its birth; neither study, observation, nor experience were necessary to bring it to maturity.

But the wisdom of the early civilisations dealt only with the external aspect of material things. The hidden secrets of Nature were undisturbed. The philosophers of those days forbore to look below the surface, nor did they call into activity forces dormant until the moment that the researches of the investigator should stir them into life.

When that period arrived it was manifested by progress in a very different manner from the sudden growth symbolised by the

myth of Minerva.   First, the observance of strange phenomena, and of the fact that their reproduction, under similar or differing conditions, as the case might be, was possible.   Next, the discovery of distinct purposes that could be effected, by the utilisation of the same force that had given rise to the phenomena originally observed.   Then, the knowledge of the means by which this force could be produced, to whatever extent it might be required or be capable of, and that when produced it could be controlled ; and lastly, the combination, into a definite scheme, of the results of all these researches and discoveries, so that the principles regulating the new science might be formulated, and its leading laws made known—thus creating a basis upon which could be ultimately moulded a more or less valuable addition to the resources of the human race.

The various stages through which the growth of any original science must necessarily pass can be stated in a brief paragraph, but to realise them centuries are often required.

Electricity and magnetism (which may be regarded for our present purpose as varying expressions of the one original power) furnish a clear illustration of this fact.   Even now our knowledge, both of the agent itself and of the possible uses to which it may be applied, is very incomplete ; but being what it is we can clearly trace its evolution.

Neglecting the earlier observers, who merely noted the existence of startling facts which they neither endeavoured to reason upon nor explain, we may fairly say that from Gilbert, of Colchester, to Faraday, three complete centuries were occupied in nearly continuous investigation of the laws that govern the action of the mysterious agency, and of the methods by which its powers could be controlled and employed.   A long list of names of distinguished men in all civilised countries shows that many minds co-operated in the enquiry, and aided to piece together, observation by observation and discovery by discovery, that mighty outcome of scientific origin which, in many respects, has revolutionised the material world, and will still further—how much further one dares not say—influence its destiny.

Faraday and Joule may be regarded as the last workers and

discoverers—of English birth at least—in the field we are considering, who trod its path with absolute devotion to pure science alone.   They and their predecessors noted facts and deduced principles ; they stored up a mass of observations for the guidance of their successors, but they did not seek to apply them so as to turn the knowledge that had been gained to commercial account.

A different state of things now exists.   Long prior to Faraday's decease, other earnest students of electric science saw that, in realising the triumphs that it might attain, benefit might be secured to their race and to themselves alike.   The hour had come when the labourer could reap the reward of his efforts, and profit by the devotion of his life to the study of Nature's laws.   Since then every distinguished electrician has applied the science he loves to some practical purpose.   When success has been secured in one department it serves only as a spur to further exertions, seeing that success means reward, and thus the recurring stimulus produces greater and greater efforts, and wider and more far-reaching results.

The object of the compiler of this address has been, however imperfectly, to trace the nature of the benefits already conferred upon the world by the use of electricity in some of its varied expressions, and to gauge the extent of the employment it has given, by means of its operations, to at least the inhabitants of our own country.

It is almost impossible to express the variety of ways in which the action of electricity is utilised.   Their name is legion, and they are ever multiplying.   Communication between distant places was the first widely extended purpose to which it was practically put.   Originally its operations were confined to points separated by land only.   Then intervening rivers, channels, seas, and oceans were successively crossed, until now nearly the whole earth is bound together by submerged chains— a kindly bondage that is another name for union.   One ocean only—the Pacific—remains uncrossed, and the barren distinction will hardly be much longer preserved.   One country of importance only—the Empire of China—has hitherto been reluctant to

encourage the lightning messenger, but that reluctance appears to be on the verge of disappearing.

Another most prominent utilisation of electricity is for the purpose of illumination. First practically discovered by Humphrey Davy, its powers in this direction were successively made practical by Wylde and De Meritens, Pacinotti and Gramme, and by many eminent scientists of later date. Electric lighting has passed through many stages. Differing from tele-graphy, it needed a longer period of trial and experiment after its commercial application began ; and it is, of course, exposed to the competition of other illuminants ever seeking, by new methods, to lessen its superiority. Economy of production is, perhaps, the great necessity for its complete development. As the light of brilliance, health, and beauty, its claims are universally acknowledged; but the commercial problem can hardly be said to be yet completely solved, although the steps taken in that direction give promise of a satisfactory solution being not far off : the problem of distribution is becoming under-stood.

Telephony—in other words, telegraphing by means of the sonorous vibrations of the human voice, and not by means of signals controlled by the human hand—came into the field com-paratively recently, but has met with wide acceptance and has had a vigorous growth. It has been welcomed in all countries as a boon to humanity. The delicacy of the instruments by which it is worked is, perhaps, the source of greatest difficulty that its further development has to overcome : their sensitiveness is alike manifested to intended and unwished-for influences. Since their inception we have had means of observation far more delicate than any previously in general use, but we have also discovered that the influence of " induction " is apparent to a much greater degree than was before deemed probable as an interference with the practical working of electric communica-tions for commercial purposes.

In electro-metallurgy, electro-plating, electro-typing, and the like uses, electricity operates in many different directions. In some branches it is employed as a new and better way of

producing the same results that were previously produced by inferior means ; in some the results attained are novel, as well as the methods employed.

The substitution of electricity for steam, water, or gas, as a source of power, capable of driving mechanical engines, either stationary, locomotive, or marine, and the transmission of similar power to a distance from its source of origin, although successfully carried out in many important instances, may be considered as yet rather in the earlier stages of experiment and growth than of those of completion. There is a wide field yet to be explored in this branch of electric operations.

For medical purposes electricity is also extensively employed ; but I confess that I am unable to separate the wheat from the tares—the really curative agent from the imposture—and therefore can only say that it is evident that much may be done in this direction ; much is already done, but the extent of the area it will finally occupy cannot yet be perceived. When the real nature of the power employed is understood, then we can better appreciate the nature of its influence upon the ailments to which the human frame is subject.

As a warning against fires that may threaten our houses, an agent employed to summon helpers to our relief, the electric current renders great service, that will, it is to be hoped, be availed of more extensively in the future than it is at present.

Electricity is also utilised to an increasing extent as a safeguard to the lives of the toilers in our coal mines charged with explosive gases. As yet it is somewhat in an uncertain stage in this respect—its use is proved, the desirability of its employment recognised, and efficient instruments for the purpose devised, but practice is yet needed to remove the difficulties in the way of its general adoption, to overcome the inertia that blocks its path.

Like the steam hammer that can strike with the weight of many tons, or crack a nutshell with a gentle tap, electricity can unite sundered continents or serve as a household convenience. For ringing the bells of our houses it has practically superseded the apparatus of pulleys, cranks, and movable wires ; it can be operated over distances to which they were inapplicable. For

submarine mining, blasting rocks, firing discharges that cause mountains to crumble, and for the purposes of warfare, the heat-conveying powers of electricity are extensively employed.

The last of the uses of electricity that I will particularise was one of the earliest to be availed of: a species of telegraphing with a special object, guiding trains on their way, and guarding against the risk of collisions by night and by day. When steam had won its greatest victories, it was necessary that some swifter and more subtle power should be at hand to control and regulate its action. Such power was found in the use of electricity, by means of which signals are given that track and guide the moving train throughout its journey. The line of railway is divided into successive sections, into each of which a train is forbidden to enter until the electric indicator gives permission, such permission being withheld until it is announced, by similar means, that any preceding train has passed from the further end of the section, upon which its successor is allowed to enter.

I have so far specified a few of the leading purposes for which electricity is employed, but to enumerate them all is practically impossible ; the sphere of its influence is widening every day, and no one can attempt to say when its ultimate limits will be reached.

Only on Saturday last I saw an announcement in an evening journal to the effect that it had been discovered that the passage of a powerful stream of electricity through sewage water effected the division of the solid from the liquid constituents of the latter. No practical description of the *modus operandi* was given ; but the bare announcement, whatever the results, shows a new direction in which some minds, at least, hope that profitable employment for the electric agent may be found.

It is clear, however, that in the great majority of its applications electricity is used as an agent—a tool, so to speak—for producing effects hitherto obtained less perfectly and less extensively by other means. For illumination, the supply of power, for plating, and many other purposes to the production of which electricity is applied, means existed in one form or other before the use or value of the latter was known. The improve-

ment, however, effected by its use in some processes has been so great that it amounts almost to a discovery, and in almost all there is still great scope for increased progress.

One of these processes may be mentioned in the case of the electric light as applied to the illumination of ships—notably passenger steamers—at sea. The contrast between the comparatively dim oil lamps and the bright glow of the incandescent lights is so great as to make the latter really a new thing. Gas cannot, for self-evident reasons, be commercially applied to our floating palaces. Electricity has therein found a field in which it has no competitor.

Telegraphy and telephony, however, have introduced a really new thing into the world. True, the word "telegraph" was applied to an instrument—the semaphore—which preceded that which we mean when we use the same expression; but its operations were so limited—darkness or fog suspending them entirely—that no real comparison between the apparatus of Wheatstone and Morse and their so-called mechanical predecessor can be instituted. A few lines of gaunt-looking frameworks, on which hung pendant legs and arms of wood, or closing and opening shutters worked by levers, stood at considerable distances from each other. The signals expressed by the persons controlling the first machine in any such line were noted by an observer stationed at the next machine, through a powerful telescope; from the second they were repeated to the third; and so, when the light and weather permitted, they reached the final point communicated with, sometimes with very considerable rapidity. Nearly all such lines—and they were not many in England—were entirely for the Government service, chiefly that of the Admiralty. The only long private line of communication of the kind of which I am aware was that belonging to the Mersey Docks and Harbour Board—from Birkenhead to Holyhead—used for signalling the arrival or departure of vessels. Imperfect as such means of communication, often interrupted, were, when their operations *did* succeed they were enough to show how great the advantage would be to the world in general if they could be permanently depended upon. Hence arose the efforts of Ronalds, Sœmmering, and others; but the time

was not fully ripe.   The discoveries of Galvani, Volta, and
Œrsted, however, combined with the result of preceding experi-
ence, led to the construction of the telegraphs of Gauss, Weber,
Schilling, and Steinheil; and, finally, the endeavours of Cooke
and Wheatstone in England, and Morse in the United States,
resulted in the production and general use of a reliable means
of long-distance telegraphy, which has since been improved upon
in many ways, but which, in its essentials, remains the same.

Now let us see what is the outcome of all this thought,
inventive genius; and intellectual energy.   What has the thing
they produced done for us ?

Let us compare, say, 1837—when Cooke and Wheatstone first
demonstrated the practicability of their system—with the facts
of 1887.

In 1837, whenever a member of any family, however highly
placed, left the shores of his native country, he was lost to his
friends—he disappeared, as it were, from their lives.   He might
be heard of at more or less frequent intervals by correspondence
passing through a slow post, but in no emergency, however great,
could he be communicated with in time to admit of his taking
prompt action ; separation was realised and its real meaning felt.
For instance, in 1834—and the conditions were little changed in
1837—when the first Melbourne Administration was dismissed by
King William IV., a special messenger, despatched from Brighton
on the morning of November 15th to Sir Robert Peel, then at
Rome, could only carry to him the offer of the Premiership in
ten days' time ; making all possible speed he only arrived on
November 25th.

At the present day let a man travel throughout the world, he
holds in his hands, as it were, the strings communicating with
his own home.   Twenty-four hours, in practice, is an extreme
period to elapse between the despatch of an urgent summons, or
the communication of an important piece of intelligence, and its
acknowledgment by the recipient, even at the ends of the earth.
Last month Lord Hawke died at the St. Pancras Hotel.   His son,
and successor, was cricketing in Victoria, Australia, but in a few
hours he received the sad intelligence, and will be home long

before he could have known of the necessity for his return in the former state of things. Take the converse of this case. A criminal fleeing from justice wishes, of course, to cut off all traces of his connection with the place whence he flies. Fifty years ago he could do so easily; a few hours' law at the start would enable him to elude pursuit effectively. He could escape by sea and know that he could not be followed for a certain number of days, and that on his pursuers arriving at the foreign shore they would find him gone—vanished into the far-spreading regions of the West or South—where no clue could be found. Now let a man succeed in catching the fastest steamer that skims the ocean, and let him do so unobserved by his trackers here, he arrives at New York, Cape Town, Sydney, Melbourne, or Suez, only to find the police awaiting him, and ready to return him, by the quickest available means, to answer for his misdeeds.

In some quarters there is a tendency to question whether our modern improvements have really tended to increase the happiness of the world; whether there was not more real comfort when the pace was not so fast. A recent novelist has entitled one of his latest productions—the period of which is laid in the last century—" The World Went Very Well Then," implying, of course, that the present day is, at any rate, very little better: this is not the general feeling. There may be a germ of philosophic truth in it, but mankind does not, as a rule, govern itself by philosophic ideas. It may have been possible for a merchant of 50 years ago, who knew nothing of the changes of markets or of the political occurrences that had happened which might govern them, or affect speculative risks, save on the arrival of weekly, fortnightly, or monthly mails, to be content that he was spared the worry of concerning himself every day with such matters; but now the telegraph brings him daily or hourly news on all these points he has, at least, the advantage that no catastrophe can mature during his period of ignorance, and burst on him like a thunderstorm when the periodical notification arrives. Counting-house speaks to counting-house, not face to face, but in effect almost as if it were so. Orders have been despatched by post to be executed in London, and the goods sent to Calcutta: a turn of the market at the latter

place shows that the execution of the orders will be a misfortune ; the telegraph is employed and they are countermanded, no mischief being done. Shares that are held in various parts of the globe are suddenly greatly appreciated or depreciated at one of the great commercial centres : the facts are at once transmitted by wire to the others, and all markets are, at nearly the same time, placed on the same footing. The possibility of dealing rapidly in one centre, and of perhaps reversing the operation in another, is rendered, under ordinary conditions, impossible : no such *coup* as that by which Nathan Rothschild gained £2,000,000, by his energetic exertions to secure early intelligence of the result of Waterloo, is now possible. To a large extent individual chances are equalised, at any rate so far as they depend upon the securing of rapid knowledge of occurring events. It is argued that this is an evil to the smaller firms, that houses with a large command of means can bear the cost of communicating freely by wire, and thus the possession of capital tells more heavily than before in the race of competition against the lesser houses. There is some truth in the contention, but it is but one illustration of the general tendency of modern trade, which more and more shows that success in all the wider fields of commercial life is mainly secured by the operation of gigantic concerns working with vast resources, whether wielded by a single person or by an association. Individuals suffer in this struggle of competition, but the world at large profits by the increased magnitude of the aggregate business done ; and the facility of rapid communication is not the least potent factor in bringing about the results we are witnessing.

In political matters the lightning messenger is not less powerful for good. Formerly, intelligence of some disturbance or menacing event was received at headquarters on a certain day, and nothing could be heard in reply, nor any direction given, until many successive events had transpired, and perhaps irremediable mischief had been done. In its early days, also, to put the telegraph in motion was of but doubtful advantage ; the despatches were too brief, the orders were too curt—they were often misunderstood—rarely was their motive explained. In later

times it has been found possible, on sufficient occasion, to exchange by telegraph despatches as full and clear as could be transmitted by letter, and thus no ground is left for the growth of misunderstandings that might produce international evil.   Formerly irritated feelings grew up, and those whose duty and desire it was to soothe them were too late before they could apply any effective check to the popular passion that was urging either side into courses that could only culminate in war.   The excitement that convulsed this country when Commodore Wilkes forcibly took the Confederate envoys from the mail steamer "Trent," and very nearly brought two kindred nations into a miserable conflict—there being then no cable connecting England with the United States—and the speedy collapse of the fierce fever that overspread France this year, when the frontier squabbles with Germany culminated in the forcible arrest of Schnaebele, followed later by the unfortunate shooting of French sportsmen by Germans, are instances of the advantage of the telegraph being in existence.   The statesmen on either side were enabled to promptly calm the feelings that threatened much at first, but speedily collapsed when the trifling character of the unlucky accidents was understood.   Perhaps the ease with which directions can be transmitted from the centre of Government to colonial governors or military commanders should be admitted to have some drawbacks attaching to it.   Of course the full circumstances of any special situation can only be known to those absolutely on the spot, and it is an evil when the transmission of too detailed instructions to them, destroys the sense of individual responsibility in the officers commanding abroad. The telegraph makes much possible for the minister at home, but in some things discretion is best shown by abstinence from its too extensive use.   Again, to take another form of superfluous energy as an example, it was, to say the least, unfortunate that, when the British and French armies were engaged in a life-and-death struggle with the Russian foe in an inclement winter in the Crimea, Lord Panmure, the then Minister for War, found the single wire laid between Varna and Sebastopol convenient for desiring the English Commander-in-Chief to "take care of "Dowb"—his relative, Major Dowbiggin.

News, in its widest sense, is a very different thing now to what it was formerly. A newspaper of the earlier portion of this century contained intelligence from all quarters, no doubt, but it was not contemporary intelligence, nor at all regular in its appearance. From the nearer capitals of Europe it might perhaps be two or three days old only, but in the same journals that printed this news appeared articles from the more distant parts of the globe dated two, three, or even five months before. For instance, in 1837, in the *Times* of July 20th, there appeared news from Valencia, Spain, of Carlist operations on July 15th; the semaphore having been utilised for its transmission across France, but its news being broken off because darkness had come on. On July 26th, news was published from Rome dated July 16th. On the 28th of that month, the dates from Hamburg were July 23rd; but in the same issue Constantinople intelligence of July 7th was printed. From Quebec, news to the 3rd July did not appear until 3rd August; and from New York, May 4th is the latest date mentioned on May 29th. From Tasmania, news to January 27th was not made known until May 30th. From the Cape of Good Hope, intelligence to March 25th was not published until May 30th; and on the same day Calcutta advices to January 27th appeared. Sydney letters of December 31st, 1836, were made public on June 8th, 1837; and, in the same paper, Batavia reports of December 28th preceding appeared. Buenos Ayres news of March 19th saw the light on June 13th.

In an earlier issue of that journal, under date of December 10th, 1834, appears a paragraph calling attention to an instance of unexampled rapidity of communication between Liverpool and New York, letters having crossed the Atlantic three times within 65 days. The vessels conveying them made unusually quick passages, and each departure took place within a few hours of an arrival, the correspondents taking advantage of them to despatch their communications without loss of time.

Since that period an entire change has appeared on the face of journalistic matters. If a cricket match takes place at Sydney, New South Wales, on December 10th, the full score appears in the *Times* on the morning of December 12th, the intervening day

being Sunday, but 10 hours being represented by the difference of time between the longitudes of Sydney and London. On December 23rd, a resolution of the New Zealand House of Representatives, on the subject of Colonial Defences, dated December 22nd, appeared in the first edition of the same journal.   And on December 31st, a message appeared in the second edition of the *Globe*, printed at 12.30 p.m., dated the same day, giving the substance of Mr. Chamberlain's speech at a banquet at Toronto the previous night. If despatched at 6 a.m., the difference of time alone would make it 5 hours later before it could reach London.

Not only events of mighty import to the interests of nations, but records of comparatively trifling matters—the appearance of a popular actor, the result of a boat race, and the like—are flashed across the world, and published here as they would be had they transpired in some city of our own island. The world has indeed become very small, if its dimensions are to be measured by the rapidity of knowledge in one part, of the facts that occur in another.

Again, as regards our own country alone, a most remarkable change has occurred in the character of the press. Fifty years ago the most important speeches might be delivered a few hundred miles from London, and they appeared in the shape of a meagre, condensed summary the second or third day after delivery. They were no longer new when they could appear at all, and hence it was not worth while publishing them at length when they did appear; perhaps, however, this imperfect publication had the compensating advantage of lessening their frequency, so far as readers were concerned.   Now, if Mr. Gladstone speaks in Midlothian, Lord Hartington at Dublin, or Lord Salisbury at Liverpool—although it may be close on midnight before their oratorical efforts are completed, yet next morning the broad sheets of our daily press will give us, verbatim, all that their audiences have listened to; and, in fact, the readers of London papers often know much earlier than the residents of a village ten miles from the place where the discourses were delivered, what were the very expressions of the distinguished speakers.

It is not, at the present day, conditions of time or distance that limit the fulness with which an oration appears, the morning after delivery, in the newspapers, but only the amount of popular interest which the conductors of each particular journal believe will be taken by its readers in the speaker's words. Audiences are no longer confined to those who can be present in the largest halls; on the wings of the telegraph, and by the agency of the newspapers, the speaker addresses the whole world, or at least such portions of it as care to know his opinions on the subject of which he speaks.

The use of the telegraph is not at all in proportion to the population of the various localities it serves. Much depends upon the nature of such populations, upon the avocations in which they are mainly engaged, and upon the extent to which fluctuations are liable to occur in prices of the commodities in which they are interested. A certain constant quantity, as it may be considered, of telegraphic business arises from the needs of domestic life. This is common to all centres, but necessarily varies as the class of the inhabitants varies. From the east of London, for example, 500,000 residents have occasion to communicate by wire on their ordinary affairs much less frequently than a corresponding number of residents in the west. Speaking as a general rule, the operative classes of the community have less widespread interests than those placed higher in the social scale. They receive in relation to their numbers fewer letters per head, and send fewer telegrams. The employment by them of the speediest engine of communication is decidedly on the increase, but it has still great room for growth.

London is far and away the centre of greatest telegraphic activity; but the fact of its being the capital—the seat of the Legislature and of the Law Courts, the headquarters of the Stock Exchange, and so on—gives it so many special features, independent of its widespread commercial interests, that it can hardly be properly compared with any provincial town as regards the amount of business done. It may, however, be of some interest if I contrast a few of the leading provincial towns in this respect.

Taking the calculated population within the free town deliveries of letters up to the end of 1886, and the actual number

of telegrams originating within the same limits for the year
ending September 30th, 1887, the results are as follows :—

	Population.	Telegraph Messages.
Glasgow	740,000	1,444,741
Liverpool	730,000	2,268,062
Manchester	613,982	1,660,538

exclusive in all cases of the messages collected for America and
the Eastern Telegraph system, which are despatched by special
wires worked by the cable companies. Upon these figures it should
be remarked that the population attributed to Glasgow includes
the ring of minor boroughs excluded from the city in a municipal
sense.   In the case of Liverpool all north of the Mersey is
reckoned, but Birkenhead, largely peopled by persons interested
in Liverpool commerce, is excluded.   But the fact remains that
the seaport possessing the largest trade of any, out of London, in
the kingdom, does the largest telegraphic business.   Manchester
includes Salford in its number of residents, and, being a central
point for numerous surrounding towns who look to it for guid-
ance in commercial matters, ranks considerably above the larger
population of Glasgow as regards the employment of the tele-
graph.   Further—

	Population.	Telegraph Messages.
Birmingham	420,000	672,714
Edinburgh (with Leith) ...	319,030	667,758
Dublin	319,000	582,083
Newcastle-on-Tyne	151,500	546,294
Bristol	227,127	466,412
Hull	186,290	452,921

show the comparative telegraphic pre-eminence of the seaports
as compared with the subordinate capitals and the great Mid-
land manufacturing centre.   Edinburgh, including Leith in its
boundaries, adds thereby a place of material commercial activity
to its semi-metropolitan population.

Other cases strengthen the conclusion that the nature of the
trade rather than the absolute volume of business must be
considered in determining this comparison, thus—

			Population.	Telegraph Messages.
Leeds	...	... ...	296,108	337,998
Belfast	...	... ...	225,000	344,603
Sheffield	...	... .	305,000	257,056
Nottingham...	...	...	211,424	236,143
Leicester	...	... ...	127,500	188,890
Cardiff	...	... ...	115,000	394,891
Dundee	...	... ...	152,840	206,168
Aberdeen	...	... ...	113,212	206,680
Newport (Monmouth)		...	40,000	153,193

For most of these divergencies and discrepancies, persons familiar with the characteristics of the various towns will easily find reasons ; and I may add a few more instances : —

			Population.	Telegraph Messages.
Portsmouth ...	...	...	132,659	154,905
Plymouth (ex Devonport)		...	96,142	185,454
Southampton	...	...	61,000	158,069
York...	...	... ...	60,500	126,541
West Hartlepool	...	...	32,000	114,014
Oxford	...	... ...	44,000	86,524
Chester	...	... ...	40,340	98,721
Bath ...	...	... ...	57,000	81,126

Analysing the figures given above, we find that Liverpool, with rather more than 3 messages per annum per head, Newcastle-on-Tyne with rather more than 3½ messages per head, Cardiff with somewhat less, Newport (Monmouth) with an approach to 4 messages per head, and West Hartlepool with about 3½ messages per head per annum, rank highest among English towns as contributors to telegraphic employment.   It may be remarked that Oldham and Blackburn, both containing over 100,000 of population, do not figure in the list of the 52 towns whence the greatest number of messages are forwarded, although many much smaller places do so.

In some instances apparently extraordinary results are explained by local facts.   Thus, the 60,500 residents of York apparently do much more business than the 91,000 inhabitants

of Norwich, a city of the same general character (126,000 messages to 89,000); but probably the circumstance that two important race meetings are held at the former place, and that the great railway station there is far more important to the travelling community than any at Norwich, may account for the distinction. If it were possible to analyse the apparent discrepancies between the figures of other towns, no doubt there would be some similar means of explaining them.

This much may serve to illustrate the changes that the electric telegraph has wrought, and the benefits it has conferred.

The operations of the telephone as yet are of a more limited character, but even as at present developed are of little less interest. The telegraph may be regarded, in the main, as the rapid public transmitter of private or public correspondence ; the telephone as the instrument of personal, and therefore strictly private, communications between man and man. If the communication is perfect, and so arranged as to be free from interference, not only can the varying sounds of voices be recognised through the connecting wire, but the tones may be distinguished in many cases, and the speakers known. One individual can speak freely to another, obtain his answer, and make any rejoinder he thinks fit. An entire business transaction can be completed without any pre-arrangement, and confidential questions can be equally confidentially replied to. Reis advanced the discovery of the means of transmitting the effects of speech to the verge of success, but did not pass the verge. Bell completed what Reis had begun, and placed a workable instrument at the disposition of the world. Edison and Hughes strengthened the forces employed for the transmission of sound waves, and the telephone (as we know it) was completed. Founded upon the laws that guided the production of the telegraph, the generation of the telephone was especially rapid—but a brief space has elapsed between its inception and its realisation. The marvels it has wrought are less striking than those of the telegraph, but the processes by which they are produced appear more marvellous still. At present the operation of wide-stretching patents perhaps cramps, to some extent, the improvement of the mechanical

apparatus needed for telephonic expression, and time has not sufficed to admit of all difficulties in the way of its extended use being overcome; but seeing how few years have elapsed since the discovery of the thing itself, we may reasonably hope for its much greater development in the near future.

EMPLOYMENT ARISING OUT OF THE VARIED USES OF ELECTRICITY.

There are many indirect methods in which labour is employed, because electricity has been utilised, that do not admit of being ranked under any special head of its operations. Wire-drawers, and, in consequence, iron works, and copper smelting works, also copper-plate rollers, are largely engaged in the production of materials for the transmission of electric signals. Timber dealers and their workmen, creosoters, insulator makers and their *employés*, brass workers, spelter makers, engine builders, boiler makers, glass blowers, carbon plate and rod manufacturers, and very numerous other occupations, receive considerable employment by means of the demands that electricity has encouraged; but as the firms engaged do not generally devote themselves solely to meeting such demands, but exist also for wholly distinct purposes, it is impossible to arrive at any idea of the amount of labour on electrical account that they employ. It is equally impossible to enumerate all the persons engaged in absolutely electrical trades. In Sheffield there are 172 electro-plating and electro-gilding manufacturers, employing in all about 5,000 hands; and in Birmingham there are 99 manufacturers of the same class, two of whom employ 300 persons each, and others a large, though uncertain, number in the aggregate—one thing is certain, it must in the total be very large.

The electro-platers, electro-typers, and the like are so widely spread that it is impossible to collect reliable figures for them— the instances preceding must suffice; a similar conclusion to the former is only possible. It is the more difficult to arrive at any satisfactory determination because it is simply out of the question to ascertain how many persons were employed in dealing with the old methods of working in these directions.

In London alone there are, according to the "Post Office

" Directory" of 1888, 535 commercial firms, of greater or less magnitude, engaged in various operations of which electricity is the mainspring.   Some of these firms appear under several trade descriptions, and it is impracticable to assign any reliable figures to the number of their *employés*, but in the total it must be very considerable.   Each employer, although carrying on probably several distinct branches of trade, must engage the full time of various persons, in most cases, in each distinct branch. To the London employers must be added the numerous others existing in our provincial towns.   I have, however, obtained reliable figures wherever practicable.

I will first take the case of the British Inland Telegraphs.

The English Post Office employs, solely in the conduct of telegraphic operations, 18,303 persons.   These individuals give *all* their time to work in connection with the electric telegraphs ; many thousands of others are partially employed in the same work, but they discharge other duties also, and possibly would be required for those duties did not the telegraph exist.   I have, therefore, not included them as among those who have found their means of subsistence through the operations of electricity.

To this ascertained number must be added those who are employed by the various railway companies, either in the conduct of their general telegraphic business between station and station, in special duties in connection with the signalling of trains, or in upholding the telegraph lines existing on the different railways. I could not, in the time available, get the figures I wanted from every railway company.   I therefore contented myself with those of four great companies, whose entire systems extend over a length of 5,881 miles.   I found that they employed 1,627 persons solely on telegraph work, and, as the entire mileage of railways existing in 1887, as given in " Bradshaw's " hand-book, was 19,339 miles, I apply the rule of proportion, with the result that I find the total number of railway servants wholly engaged in electrical work to be 5,383.   Many of the railways which go to make up the gross total mileage are small concerns, many are large ones ; but as the infinite subdivision of lines multiplies the number of persons employed in carrying out the operations of

any branch of work, I do not think that I can be far wrong in applying the ascertained results of the figures I have reliably obtained to the average of the whole railway system.

But, like the Post Office, the railway companies have a very great number of persons partially though not wholly engaged in similar operations. The men at the numerous signal cabins not only work the levers controlling the fixed signals, but usually manipulate the block telegraph besides. At many small stations, where there is but a limited force engaged for the discharge of all duties, the booking-clerk works the instrument in addition to his other employments. I do not count either of these classes among the purely electrical *employés*.

The Exchange Telegraph Company, a licensee of the Post Office, engaged mainly in the supply of intelligence of special events to clubs, public institutions, and private persons, employs the entire time of 182 persons, besides the partial time of many others who supply the company with reports and information.

Thus the number of individuals engaged entirely in the transmission of business brought about by the existence of the electric telegraph in Great Britain and Ireland is, as nearly as I can ascertain, 23,868 persons; to which must be added an army of persons who probably find their incomes more or less considerably increased by the same cause, though not entirely dependent upon it, and a further uncertain number employed on wires belonging to private individuals.

I intended to confine myself to the economical figures of the British Islands only, but I may remark that if the rule of proportion holds good in this branch of the subject, and the entire mileage of public telegraph wires of all kinds in this country be, as is the case, 229,000 miles or thereabouts, while those of the world approximately amount to 1,800,000—omitting submarine cables—we have, as in the one case 23,868 persons are employed, a total number of 179,748 persons who find a field of labour in the working of telegraphic communication throughout the globe. It is impossible, seeing the varied circumstances existing in different countries—in some there are long mileages of wire connecting comparatively few offices or stations, in

others there are short mileages connecting many stations—to
say that this total is correctly arrived at, but it cannot fail to be
at least an approximation to the truth. In some countries the
available figures are indefinite as regards the differences between
Governmental lines and wires for railway purposes; in some,
telephone lines are mixed up with the figures for telegraph
lines. Thus the totals I have given are liable to error, but it
is, at any rate, error on the right side, and the number of
individuals engaged in their working in all probability will
rather exceed than fall short of the numbers I have given. The
facts as to the partial employment of individuals not enumerated
as telegraphic *employés* is common to all the world, and not by
any means limited to the United Kingdom. To the known and
fully occupied a very large addition must be made to represent
the unknown and partially occupied.

The Submarine Telegraph Cable Companies must next be
considered. From three of these—the Eastern, the Anglo-
American, and the Submarine—I have obtained the actual figures.
Their *employés* number in all 2,168. These three companies
possess about 36,697 miles of wire, and throughout the globe there
are about 107,000 miles of cabled wire (I deduct those cables
that are really integral portions of land circuits and which have
no separate organisation; the persons employed in working them
are treated as if engaged on land telegraphs). By proportion, this
total mileage of wire in cables should make the entire number of
persons employed, 6,315. Some time ago the Eastern Telegraph
Company endeavoured to get up a census of the number of persons
employed in submarine working throughout the world, and arrived
at a total of 4,844 persons; but I am not certain that all the lines
included in the above calculation were existing at the time such
census was compiled. I am pretty sure they were not. I take
the actual total of persons employed at 6,000.

These cable companies employ also cable ships in repairing and
maintaining the submarine lines. Of these I speak more fully
when dealing with cable manufacturing companies.

Next in order come the Telephone Companies. These, in
proportion to the number of their wires, of course employ com-

paratively few persons, as the renters of the lines are their own manipulators; they employ attendants only at Exchange switches; but, nevertheless, the information received from all the companies existing in Great Britain and the North of Ireland shows that their operations give employment to 2,386 persons. To this number must be added that of the company working in Dublin and the South of Ireland; thus the total is raised to certainly 2,500 individuals, probably more. I have no facts upon which I can extend the comparison to the world generally, but, seeing the universal adoption of this means of communication, and the extent of its development in the United States especially, there can be little doubt that the aggregate total of persons engaged in carrying out this form of electrical undertaking must at least reach 30,000.

The electric lighting industry then comes under consideration. From five of the largest concerns engaged in the business of either providing apparatus and mechanism for its production, or utilising such means for the actual installation of the illuminant, I have received returns of the average number of their *employés*. These amount to 1,828. The five concerns I applied to, although among the largest, by no means represent the greatest part of the entire industrial army occupied in the like pursuits. I believe that there need be no hesitation in putting down at least 5,000 as the total for our own country; and, keeping in mind the fact that all countries welcome and adopt the new light—some to an extent much exceeding our own—it is reasonably certain that twenty times this number, or 100,000, will not be too great a figure to represent the entire strength of the workers in this field. Along with the electric light companies and the employment they give, I comprise those of the makers and producers of secondary batteries or accumulators mainly occupied in operations auxiliary to the production of electric light.

There remains to be considered the number of persons employed by the submarine cable and insulated wire manufacturing companies, who most of them make also various instruments used in telegraph working, and in other electrical applications. The extent of the employment given by them varies considerably, as,

of course, cable-making is a somewhat fitful industry—the periods of active construction and of comparative slackness alternate with each other. From the three largest companies on the Thames, I learn that the average number of persons employed by them varies from 5,457 at a busy time to 1,803 at a dull one; this gives an average of 3,630. The trade, so to speak, is more concentrated than in the case of the electric light, and it is probable that about 5,000 persons will be a fair allowance for the entire number engaged in it in these realms.

In the foregoing totals are included the number of persons habitually employed on the cable-laying ships of the various manufacturing companies, as is also the case with the repairing ships belonging to the different cable companies. The vessels represent a kind of international fleet engaged solely in attendance upon the cables in which their owners are interested. They are of all sizes, from the gigantic "Silvertown," "Faraday," and "Scotia," to vessels of 300 tons and 400 tons burden. Our mercantile marine, therefore, must be counted among the occupations benefited by the advent of electricity.

We must not overlook the large body of persons engaged in teaching the aspirants to electrical knowledge. At universities, colleges, and technical schools a new branch of science has been added to the previous curriculum. Professors, demonstrators, and their assistants represent in the whole a very important and numerous class. Their pupils, in an appreciable measure, represent workers ranked in one or other of the occupations I have mentioned, while numerous others have as yet attached themselves to no special field of work, but are preparing to take part in some definite sphere of labour when their studies are sufficiently advanced. It is amongst these regularly trained for the conflict that we must look for our future victories over the inert forces of nature. It is they who will advance that which as yet is struggling with the difficulties that surround the unknown. It is to them that we must look for future discoveries which will make that plain and easy which now has to be sought for through much trial and many errors. In the early days men groped in the dark towards the light, as it were; but our present students

of electricity have at their disposal a clear *resumé* of all the achievements of the past to aid them as a torch on their way to fresh operations in the same field.

Finally must be mentioned the technical press. Independently of the papers devoted mainly to other interests, such as *The Engineer*, *Engineering*, and their like, which give large space to electrical subjects, we have in England, America, France, and Germany alone between twenty and thirty organs interested wholly or specially in the publication of facts or opinions concerning the operations of electricity. This department of the press is invaluable in rapidly spreading a knowledge of what is doing by various workers in the electrical field throughout the globe, and is itself a literary outgrowth of the marvellous agent we are regarding.

Considering the foregoing figures, the results come out as follows :—

Employed in connection with	Number of Persons.	Probable Number of Persons Employed throughout the world.
British Land Telegraphs ...	23,868	180,000
Submarine Cable Companies (mainly British)	6,000	—
Telephone Companies... ...	2,500	30,000
Electric Lighting	5,000	100,000
Cable-making, and allied businesses	5,000	—
	42,368	

Add to these the individuals that cannot be classified, who must amount in the aggregate to at least an equal number, we arrive at a total approaching 100,000, in round numbers, of persons dependent for their employment on the various operations directly connected with electricity in these islands alone, besides a very large addition to the output of various trades, caused by its consumption of the materials they produce. The employment of 100,000 persons means the support of at least 300,000 of the community. An unusually large proportion of young, or at least

unmarried, people of both sexes minister to the calls of electrical work, and hence I take the average family at three instead of the usual calculation of four and a half or five.

If these figures represent even an approach to the truth, it is evident that throughout the earth there must be 5,000,000 of people, at least, who would have to seek for other means of subsistence if electricity and its commercial applications had not been made known to man. Thus it is evident that a double blessing has been conferred by the discovery of its potent force—a blessing to the inhabitants of the world at large, who profit by its operations, and a further blessing to the toiling myriads whose field of labour lies in carrying them out. How great will be the numbers similarly occupied and similarly dependent when a century more has rolled away!

Thus far I have sought to sketch, although briefly and imperfectly, the various modes in which electricity is utilised, and to arrive at some idea of the extent of employment derived by the workers who strive to secure such utilisation. But this is at best an approximation to the truth at the present moment. It is little more than fifty years since the powers of the new agent were first turned to practical account, and since that time the record has been one of continuous and unabated progress. Discovery has succeeded discovery, and invention has followed upon invention. At present many fields are entered upon but scarcely trodden. Electro-motors and their like have clearly an unknown, an immeasurable field before them. What is done is as nothing to what will be done. The fusion of the influences of sound and light, the laws of heat in relation to the power we are considering, and its equally important bearing upon those of acoustics, open up so many directions in which we may look for future researches to make much that now shows, as it were, through a glass darkly, clear and distinct, if not to ourselves, at least to future generations. The Leyden jar and the frictional machine taught us the true resemblance between the force they made manifest and the awe-striking lightning discharge from the clouds. Later, more tractable and more easily controlled manifestations of the same agent have been

discovered, but in their obedience to control they enlarge greatly the sphere of its uses.

As a stander-by, watching the progress that has been made, and is still making, by the earnest workers who have given to the world so much—marking the difference between the knowledge of the present day contrasted with that which existed over thirty-six years ago, when I first knew the difference between a battery and an insulator, and conceived some vague idea of their respective functions—I am struck with astonishment. In 1852 the Rubicon had been crossed, the theorist had given place to the practical worker, but as yet the latter had not advanced very far from the lines laid down by his predecessors; the spirit of the originators was as yet the controlling influence, but the mind of the student was set free, and each month showed the results of unshackled enquiry. New fields, undreamed of then, have been opened; new applications have been discovered by which the powers of electricity have been utilised; known applications have been greatly advanced and extended. In the time of which I speak, comparatively few of the *employés* in telegraph working—and that was, speaking broadly, the sole demonstration of electricity that could be commercially cultivated—were attracted thereto by any knowledge of the science upon which it was founded, or interested in the progress it might make; most looked to it as something in the operations of which they could contribute with profit to themselves, remaining ignorant of the laws that governed it, or of the reasons why certain apparatus was employed to produce its effects. To them any knowledge they might glean was empirical, the pure result of experience in a contracted form. At the present day an entire change has taken place. Into none of the advanced operations of electricity does a youth engage himself without having some taste or liking for investigation and the acquisition of real knowledge of the subject in which he is interested. Societies such as this best justify their existence in lending aid to those who seek to elevate themselves and to know what can be learned from their predecessors in kindred labours.

A distinguished scientific statesman once described the mere *workers* of electric telegraph machines as " ambidextrous

"monkeys"—they knew only that moving a certain handle to the right or to the left, or a key up and down, produced signals or marks that were legible at the further end of a wire; but how they produced them, or *why*, they were supremely ignorant. It is not so with the student of modern days. The man who devotes his life to the electric light, motive power, or other allied branches, knows that he must prepare himself by a careful course of theoretical study before he undertakes practical work. He must know the reasons for all that he does.

Thus it is clear to me that in the facts detailed, lies the strongest justification for the proposed alteration in the name of our Society. In the summary of electrical operations which I have put before you I have given the largest space to the electric telegraph and its results. I am justified in so doing, because the telegraph is the most nearly completed of all the branches into which the laws of electricity are directed. No doubt there is still room to improve its processes, but its pioneer work is done, and it is scarcely possible that the future can yield more than progress, however important, in details. When the Society was first founded, "Telegraph Engineers" was a fitting name, as it was only in telegraph engineering that any considerable number of the followers of the science we are considering were actively engaged. A great change has taken place: positively even more important than it was then, it is now relatively less so. The field has been so carefully reaped that it offers little temptation to the average gleaner. Speaking generally, men's thoughts are directed to other channels, where they may seek, in partially explored or unknown regions, the fame and profit that may accrue to those who win victories therein. "Telegraph "Engineers" are still the most numerous units in our body, but from the force of circumstances they can no longer be said to be the special representatives of its character. "The Institution of "Electrical Engineers" is a title comprehensive enough to include all devotees of the science; no class of workers is singled out for undue prominence, no class is by implication excluded.

The President of the Royal Society, in his recent address, told the members of that learned body that the world wanted a great

discovery—that it was most desirable that the nature of electricity should be made known and defined. We may be on the eve of such a discovery, we may be far from it. The problem to be solved—that of the true character of the mysterious agent that seems to pervade all nature—ranks apparently with the equally puzzling one, "What is the soul?" Philosophical and theological students have quarrelled over the latter question for many generations without advancing nearer to a solution. Let us trust that "What is electricity?" may, in the fulness of time, receive an answer which will extend the boundaries of its use to purposes as yet undreamed of, and that the benefits it confers on man may be thereby multiplied many fold.

Major-General WEBBER: It is to me a double pleasure to be asked to move—"That the thanks of the Society are due "to our President for the most interesting Address which he "has just delivered to us, and that he be requested to permit "its publication in the Journal of the Society." It is not usual on these occasions to make remarks as to the substance of the address that is delivered by our President annually, or to deal with the matter contained in it; but, having passed through the ordeal myself once, and having had to prepare an address, I cannot help feeling that every year its preparation must be a matter of greater difficulty to each successive gentleman who does us the honour to fill that post. When the future President looks about him for material to put into his address, he must, I think, when he wishes to deal with that part which is sometimes incumbent upon him—viz., the retrospective part—rather hate the other past Presidents who have taken up that ground, and more or less fully occupied it; but on this occasion I think that we may congratulate Mr. Graves in his retrospect, because he has dealt with it from a point of view that I do not think has been followed by any of his predecessors.

There is always one feature in Addresses coming from men who have filled distinguished positions in the past, or who are now (as Mr. Graves is) continuing to fill them—namely, that

they are enabled to give us special information which we should hardly be able to obtain from any other source. I have often looked amongst the papers that are laid before our Society, and wished that statistics such as those that have been laid before you to-night by Mr. Graves might now and then have place. They are landmarks, because in the future our successors will look back to communications such as this one as a means of measuring the progress which will have been made in the interval.

We must not forget, and I am sure that Mr. Graves in the preparation of his Address did not forget, that although he only gives us the numbers of those who are actually employed, and who are living by this great industry in which we are concerned, every industry in this country, or a very large number of them, has been multiplied more than twice, we may fairly say, in their operations by the facilities which the electric telegraph has afforded them.

At the time when the Government proposed to take over the telegraphs to State control, I remember well that Mr. Edward Chadwick recorded, in a pamphlet, his belief that every telegram, as a commercial transaction, was worth, to the industry with which it was concerned, at least 10s. That measure, of course, was an empirical one, but I have no doubt that if it were now to be estimated again it would represent a very much larger figure.

The suggestion which Mr. Graves has foreshadowed, of some change taking place in the title of our Society, is one which is most interesting to us all. Hitherto a very happy union has existed between the Telegraph-Engineer and the Electrician, but there has been one thing wanting: the union has not produced any offspring. I do hope that during the next year the nativity which may, we hope, take place, and which we trust has been incubated this night, may bring forth a healthy child called an "Electrical Engineer," who will grow in the future to be a fine lad, a strong man, and, at some day in the next century, a hale old gentleman.

Mr. C. E. Spagnoletti : I have very great pleasure indeed in seconding the resolution proposed by General Webber. We have but to consider for a moment the statistics that Mr. Graves has

laid before us to be convinced of the amount of time and labour that he must have devoted to the preparation of his Address. It must be interesting to us all to find that the progress of our science has been so great as to make the enormous strides which he has been able to point out. Those who have not had the pleasure of being here to-night to hear the Address read I am sure cannot fail to be most deeply interested in it when it is presented to them in our Journal.

The motion was carried with acclamation.

The PRESIDENT: I am much obliged for the vote of thanks you have been kind enough to give me. You must excuse my saying much, if it is only because my unfortunate habit of reading very fast makes me so very thirsty that I cannot. My object in the Address was not to detail the commercial advantages that the world has derived by the use of electricity, but rather to sketch them, leaving others to fill up the blanks that remained as they would. But the direct value of the figures I tried to get at, you can measure, and I believe that I have certainly understated far more than overstated the number of persons who are employed.

A ballot for new members then took place, at which the following candidates were elected :—

Foreign Members :

Armand Deries. | James Tatham.
                 Richard Waring.

Member :

Robert Percy Sellon.

Associates :

Wilfred D. Bailey.	James Raeburn, jun.
John Chapman.	Charles N. Russell.
Charles James Dale.	Arthur J. Stubbs.
Stephen Stewart Goodman.	John M. Grylls Trezise.
Robert Greey.	M. W. Woods.
Arthur Marwood.	

Students :

Edmund J. T. Ryves. | Edward Stanley Shoults.

The meeting then adjourned until 26th January, 1888.

The One Hundred and Seventy-second Ordinary General Meeting
of the Society was held at the Institution of Civil Engineers,
25, Great George Street, Westminster, on Thursday evening,
January 26th, 1888—Mr. EDWARD GRAVES, President, in the
Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to
be suspended.

The following transfers were announced as having been
approved by the Council, viz. :—

From the class of Students to that of Associates—
    Charles George Lamb. | Louis Campbell Login.

Donations to the Library were announced as having been
received since the last meeting from R. Howard Krause, Local
Honorary Secretary for Austro-Hungary, and the Proprietors of
the *Electrician*, to whom the thanks of the meeting were
unanimously accorded.

The
President.
    The PRESIDENT : We now have to recommence the adjourned
discussion on Mr. Cockburn's paper on " Safety Fuses for Electric
" Light Circuits, and on the Behaviour of various Metals usually
" employed for their Construction." I think Professor Silvanus
Thompson is here. Will he kindly say a few words ?

Professor
Thompson.
    Professor SILVANUS THOMPSON : It seems to me that the
relation between the curvatures of the curves shown and the
dimensions of the terminals to which the fusible wires are
attached was hardly sufficiently emphasised in Mr. Cockburn's
paper. In diagram of tin (No. 2) the curves refer to two pieces
of tin wire of the same thickness, but the experiments were made
with two different sets of terminals. In the case of the upper
curve the terminals were larger than in the case of the lower
curve ; and the cooling effect of those larger terminals is marked
by the fact that it requires a larger current to produce, with a

Professor·
Thompson.

wire of given length, the same results as with the smaller terminals. Now, obviously, this bears upon the question, What length of wire really provides a safe fuse ? In Mr. Cockburn's paper he rather emphatically states that $1\frac{1}{4}$ inch should be the length for every fuse. Now I do not know whether he means that exactly as a dogmatic statement, to be taken without any exception ; but, obviously, if you have very thick terminals, $1\frac{1}{4}$ inch will not be a sufficient length ; and I am not quite sure, if smaller terminals to attach the fuse to were used, whether a shorter length might not be, under some circumstances, admissible. It is, to my mind, a question for experiment purely to determine what is the admissible safe length under any given conditions. It is clear that you cannot have very small terminals— you must have terminals large enough to be able to attach the fuse to them with comfort ; it will not do to have microscopic screws, and things of that kind. I would like to hear from Mr. Cockburn, in his reply, what he considers the practical limit.

The thermal conductivity also of the material of which the terminals are made will have a good deal to do with the amount of this end effect. If copper terminals of a certain size are used, there will be a certain cooling effect at the ends ; if brass terminals are used of exactly the same size, there will be a cooling effect of less degree, because brass conducts heat less well than copper does.

I was much struck by the figures that Mr. Cockburn put on the board when the paper was read, giving the actual currents at which fuses of a certain size, designed to go off with $5\cdot25$ ampères, went off. The percentages of excess in different cases were remarkably close. There were eight experiments in which the percentage differences of current were as follows:—$2\cdot86$ per cent., $0\cdot95$, $0\cdot95$, $0\cdot95$, $2\cdot86$, $2\cdot86$, $0\cdot95$, and $0\cdot95$ per cent.

Mr. ALEXANDER SIEMENS : Those fuses were made by us. The figure upon them does not represent the number of ampères—say 5—at which they should go off, but the gauge of our wire for which they should be used.

Professor SILVANUS THOMPSON : Pardon me ; I referred to certain fuses loaded with balls in the middle, not the Siemens

Professor Thompson. fuse, and marked to go off at 5·25. The actual currents that fused them were wonderfully close to that figure. I have never before heard of cut-outs that could be relied on to anything like 3 per cent. of the current that they were supposed to go off at. These numbers, being all under 3 per cent., and some of them under 1 per cent., are very remarkable.

Mr. Hedges. Mr. KILLINGWORTH HEDGES: I would preface my remarks by saying that I do not wish in the slightest degree to detract anything from Mr. Cockburn's valuable experiments, and his novel application of the fuses shown; but, at the same time, the idea of putting a wire in tension so that it parts suddenly when weakened by the passage of the current is not altogether new. I have on the table an apparatus which I used some years ago, which was designed for the same object. I thought that by putting a spring underneath the wire, or foil, I should make it jump apart, and be able thus to use a very short wire and obviate any danger of arcing. Also, I do not think Mr. Cockburn has exactly appreciated Sir W. Thomson's experiments, because it appears to me that Sir William has really done everything that has been put forward in this paper. The form illustrated on the wall is only one of Sir W. Thomson's fuses out of the many that he experimented with; and, with your permission, Sir, I should like to read his own description of the system in general. He says:—
" The essential conditions fulfilled in the construction of these
" fuses are—(1) Certainty and uniformity in action, (2) smallness
" in the percentage loss of energy in the fuse itself, (3) facility
" in replacing a fresh fuse. These objects are attained by making
" the fuses of two springy pieces of high-conductivity copper,
" in which heat due to the current is generated, and uniting
" them by a soldering of fusible metal which melts at a very
" moderate temperature. The copper springs are made of such
" form and are so placed as to give complete severance of the
" circuit with instantaneous stoppage of the following arc when
" the melting of the fuse has taken place. The fuses are of two
" kinds—the Spiral Fuse, which is the form of fuse designed for
" currents of from ten ampères down to a half-ampère; and what
" is called the Z Fuse, on account of the similarity of its shape

Mr. Hedges.

" to the letter Z, which is designed for any current of from six " ampères upwards to hundreds or to thousands of ampères."

The fuse is not rightly shown on the diagram. The weight gives a lateral strain on the fuse, and when the solder melts it springs asunder. But the great point about Sir William Thomson's fuse, and which I do not see touched upon by Mr. Cockburn, is as to the loss of energy. In a letter I have from Sir William Thomson he says that he has carefully measured this loss, and finds it to be only ·25 watt for 12 ampères of current. I do not know whether the author has experimented in that direction; if so, I should like to hear his results. My own experiments show that the loss with wire is very much higher, probably because of the difficulty of making a good connection with the fusible wire and the terminal. With my system of mica foils the loss may be taken to be about 1 watt per 6 ampères.

As regards the accuracy of foil, which has been questioned, I think that is simply a question of cutting the foil to the right size and using it in the proper frames of cut-outs, as Mr. Cockburn mentioned. If a large mass of metal is put near a fuse of any description, it will conduct the heat away, and the fuse will not melt at the point at which it is marked; on the other hand, if a fuse is put into too small a holder, it will melt too soon. On the wall there is a diagram of the results of experiments made by Mr. Blakesley and myself some years ago, and I think they will compare favourably with those results which were put on the blackboard by Mr. Cockburn at the previous meeting.

HEDGES' MICA FOILS.

Width.	Fusing Current.
Mm.	Ampères.
8	9·52
8	9·27
8	9·40
9	10·65
9	10·46
9	10·73
10	12·14
10	12·01
11	12·14
11	12·92
11	12·83

As regards the author's experiment giving a very uncertain result with tin foil, I should like to know definitely what fuses were used. . Of course my fuses are made commercially, having long gone out of the position of being simply made for testing and laboratory purposes, and I do not think it is fair to take such fuses and put them through laboratory tests and say they do not agree with the figures marked upon them. If an accurate cut-out is wanted a number of fuses all of the same size should be taken, and the differences in their melting noted, and if there is a wide difference it shows at once that an inaccuracy has crept in.

I am glad to see that Mr. Cockburn brings up a subject which I hope will be discussed this evening—that is, about the protection of lamps. I cannot see why fuses should not be made to a large extent to protect lamps—anyhow, to prevent that blacking which, if I may be allowed to say so, seems to be the *bête noire* of electric lighting, and which destroys the efficiency of lamps after they have been at work a little while. I have placed on the table fuses of different kinds : one is Mr. Edison's original fuse

which he laid down in the streets of New York. It only shows ^{Mr. Hedges.} what a very bad material lead is, because after they had been down for some time they had to be taken up and re-designed. Mr. Edison still uses lead, I believe, but instead of making his contact with a strip of lead he takes a piece of copper and unites it to the lead so as to get a really good joint with the terminal. I have also brought an interesting specimen showing the kind of thing that is sometimes put in; I took it myself from the switch-board in a large public building. It was supposed to go at 40 ampères, but it is made of lead wire, and I do not think could possibly melt under 800 ampères, possibly more. I am glad that the insurance companies have now insisted on fuses that are intended for use, and not only for ornament. Mr. Cockburn mentioned double-pole cut-outs. I should like to ask him whether he has experimented with his ingenious plan in actual work. I have carried out some experiments, and find, curiously enough, that when one fuse goes the other one goes too, which is a great nuisance; and not only does it go when they are at about the same melting capacity, but when one fuse has a very much larger area than the other: there seems to be an effect at the moment of melting which sends a current through the whole system and melts the other fuse.

On the wall is a diagram showing an old apparatus designed for putting in a second fuse, by a small magnet actuated by a shunt, to the main circuit; the idea being that one fuse should be the usual protection for the system, and the other one should come into action when the first fuse melted, and should be of very much larger capacity—much larger than would really be used under ordinary circumstances—so that in case of any tampering with the wires, or in the case of one fuse in a double-pole cut-out causing the other to go, the reserve fuse would not melt, but still protect the mains while the first one was being repaired. But if there was a bad short-circuit of course both would go, one soon after the other.

I think that in this discussion a very important question might be settled—as to what margin should be given to fuses. I have just received the latest edition of the Phœnix Fire Rules,

Mr. Hedges. and I see that there are still two instructions which do not agree. In one part of the rules it is said that 50 per cent. should be allowed for margin in a cut-out, while further on 25 per cent. is put down for double-pole cut-outs; and returning to the old rules of the Telegraph-Engineers, which, in spite of what the electrical journals tell us, are used by some people, the melting value is quite a different thing altogether. It is said there that the fuse is to be so arranged that no part of the system should attain a temperature of over 150° F. I cannot understand what that means, because the fuse would have to be of a very fusible metal indeed if its temperature is never going to exceed 150° F.

In conclusion, I shall be very glad to show the fuses I have placed on the table to anyone who wishes to see them after the meeting.

Mr. Preece. Mr. W. H. PREECE : This is a subject to which I have devoted a great deal of attention, more from its scientific aspect than its practical one ; although I am bound to confess that I have had a considerable amount of experience of the defects as well as of the advantages of cut-outs. The paper that Mr. Cockburn read is, so far as I know, the first systematic effort to place what really has been rather in a state of chaos into something like a scientific position. Mr. Hedges has alluded to the fact that there have been other workers in the field. It has been pursued by Mr. Killingworth Hedges, and I do not hesitate to say that his form of fuse has been one of the most useful that has been in the market. I have used them for the past four years in my own house, and I have more than once derived considerable benefit from their behaving perfectly at the right moment. Again, Sir William Thomson has devoted a considerable amount of atten- tion to the matter, and his fuse is certainly most efficient and extremely pretty in its form. I have sent a great many of them out to the Colonies, and I have heard of their performing their functions with perfect satisfaction. But I am very much inclined to think that this question of cut-outs is very much overdone. We have cut-outs forced upon us in all directions. The Phœnix Fire Rules, drawn up by Mr. Musgrave Heaphy with a great deal of skill and a great deal of care, impress upon all of us the use of

these cut-outs; but I think that Mr. Heaphy goes a little too far Mr. Preece. in compelling electrical engineers to scatter them all over the place ; for we must remember that, however perfect a cut-out may be, it also brings with it its own defects.   One building that I had to do with had so many cut-outs in it that I found, on going into the matter, that the cut-outs themselves had introduced into that building no less than 6,000 bad joints ; and every one of those 6,000 bad joints, sooner or later, was bound to be a fault, and bound to create discomfort.   So that we have not only the cut-outs performing their own functions, but also producing their own dangers.   I should say that out of every ten defects or failures in electric lighting that I have had to inquire into, nine of them have been due to the failure of cut-outs.

In 1884 I read a paper before the Royal Society on the " Heating Effects of Electric Currents," and the origin of that paper was the fact that we protect, and have for the last twenty years or more protected, all our submarine cables from the dangers of atmospheric electricity by supplying them with a cut-out.   At the end of every cable there is a lightning protector, usually of the flat plate type, but sometimes a vacuum tube, and as an additional precaution we also insert a piece of fine platinum wire which shall fuse before the current can possibly acquire sufficient strength to do damage to the insulated cable core.   It became a matter of very great consequence to know what should be the proper size of this platinum wire.   There are currents induced by atmospheric electricity that exceed very little the strongest currents used in actual work, and therefore it became desirable to have such a wire that would not fuse with the most powerful working current, but which would fuse when that current was exceeded by some 50 per cent.   On going into this I made a series of experiments, and proved a law that had previously been published in the *Electrical Review* by Mr. Kempe, and subsequently developed by Professor George Forbes in a paper read at the British Association, in which he showed by experimenting with wax that the fusing current could be expressed by the formula
$$c = a \, d^{\frac{3}{2}}.$$

Mr. Preece. That is the law that regulates the current that will produce a certain temperature. My first experiments, in 1884, were devoted solely to prove the accuracy of that law; and while my experiments showed that below a certain size, when the wires were very fine, the law was not verified, when we came to wires of ·010 inch and beyond, then the law was strictly and absolutely true. Having shown that law to be true for platinum, it became very desirable to know how far it was true for all sizes and for all wires. During the past twelve months I have been engaged in determining the value of the constant a for all metals, and at the same time have also been carefully looking into the question of the application of these wires to cut-outs. In my first paper, in 1884, I pointed out the error that might be introduced from the cooling effect of the mass of the terminals, and I showed, as Mr. Cockburn has shown, that we only get rid of that error when lengths of 6 inches are used; so that in all my first experiments that length was adopted. In a paper that I sent to the Royal Society in November last, but which is not published yet (I believe it will be published next week; there are many tables in it which may have delayed the printing), I have taken two cases—one where the length of wire acted upon was only 1¼ inch, that being the minimum length allowable for cut-outs, and about the average length; and in the other case the length of the wire was 6 inches.

I took three fiducial points. First of all, I wanted, if I could, to determine the current that would raise a wire to a certain temperature; and on this point I may put Mr. Killingworth Hedges right. In those Fire Rules of the Telegraph-Engineers, when speaking about 150°, they refer, not to the fuse, but to the mains and to the leads; and it implies that leads and mains should never be designed so that when the maximum current passes through them their temperature is raised to 150°. The points that I took for observation were the points where the wire reached the sensation of warmth. Our blood is about 98°. If you have a temperature of 100° you can feel it. But I found that the sensation of warmth was extremely uncertain; and in order to avoid being dependent upon the state of one's health

and upon the various sensations found by different observers (I always like to rely upon two or three observers whenever sensation is in question), I used a small thin flake of shellac, about the size of a pin's head, placed on the wire, and when the wire acquired a temperature of 77° C. the shellac melted. The second fiducial point is one that I used in my previous paper—that is, the point when wire becomes self-luminous. A wire becomes just visibly red-hot always at the same temperature; and whatever metal it be, or whatever substance it be, when it is raised to a temperature of 525° C.—very nearly 1,000° F.—then that substance becomes red-hot; and a red-hot wire is as good an indication of a current as any ammeter that ever yet was made. Then the third fiducial point was the fusing current. The first series of experiments were made with cut-outs. I took copper, aluminium, German silver, platinoid, iron, tin, tin-lead alloy, and lead; and for all these materials there were various sizes, commencing at ·010 inch, or 10 mils., and going up to ·040, or 40 mils. The measurements are given in inches and in centimètres, the gauge of the wire is given, and for all those wires and for all the results the constant a has been calculated; so that from the tables, using any wire, anybody will be able to find exactly the size of wire that will go with a given current, or, having the current, will know exactly what size of wire to put in to protect themselves from that current.

A very curious thing came out with those experiments. The shellac acted as a kind of flux and prevented the oxidation of tin; so that really my shellac flake acted almost in the same way as Mr. Cockburn's little lead pellet, and the results came out that when the wire was protected with shellac it always went with about 25 per cent. less current than when there was no shellac at all; and that is an interesting fact, proving, in fact, that what Mr. Cockburn has observed—the failure with the high current required by tin—is really due to the fact that the surface becomes coated with oxide.

Another interesting fact that came out, and upon which I want to dwell a little, is that when we take platinum these three fiducial points—the 77° C., the red-hot temperature, and the

Mr. Preece. fusing temperature—are very nearly in the ratio of 1, 2, 3 ; so that if you have a piece of platinum wire and raise it by a current to a temperature that will just melt shellac, twice that current will make it red-hot, and three times that current will fuse it. That is a fact very well worth knowing in platinum, because, as I shall point out in a moment, it enables platinum to be not only a very good cut-out, but a detector as well.

The second series of experiments were made, not to determine the fusing currents, but to see what took place when really that takes place that we meet with in practice, viz., a sudden short-circuit, throwing on to our leads a sudden current, and a sudden bursting up of our cut-outs. Ninety-nine times out of a hundred a cut-out does not act with a steady increasing current : it goes off with a sudden burst ; and you always see, when a cut-out goes, a flash, and you know at once that your cut-out has gone, and you put things right again.

It was a very important thing to know how these metals behave themselves under different circumstances, when currents of . great strength were passed through them ; so that I took tin, platinum, silver foil and alloy, aluminium-silver foil, silver wire, zinc foil, zinc wire, copper wire, brass wire, hard-drawn bright steel wire, mercury—in fact, everything I could get hold of—and put them under such conditions as they probably would have been in if they were used as cut-outs, and then put on a sudden powerful current and let the thing burst. It was most instructive to see how those metals behaved themselves. With tin, as Mr. Cockburn has mentioned, the surface becomes oxidised, the oxidised surface acts as a kind of skin to the metal, the metal fuses with great suddenness inside, and it bursts and scatters about the molten particles to a distance of 9 or 10 feet, burns holes in floors, and does all kinds of mischief ; and so with most of them. The results of the experiments I hope you will be able to read in the published paper soon, but the result is that the best metal to meet such a case of a sudden bang is platinum : it goes off like wax ; it does not burst with an explosion, but it melts off gently. Tin is very good in itself, but iron, copper, and all the others scatter molten particles about, and

really are extremely dangerous to use.   But whether we use
platinum, lead, tin, or any other metal, it is necessary to case
them in—as is done, in fact, in practice—covering them with little
glass bottles like Edison does, or in glass-covered cases as Mr.
Cockburn does, or in little boxes like, I think, nearly everybody
else does.

The third series of experiments were the most important, and
were purely scientific.   They were made to determine the value
of the constant a for all metals when free from any error arising
from the cooling effect of the terminals, and they were very
carefully made.   Copper, aluminium, platinum, German silver,
platinoid, iron, tin, tin-lead alloy, were all measured; the con-
stant a is given for all these metals—for inches when the
measurements are made in inches, and in centimètres when that
measure is adopted.   The formula is given which at once enables
us to find the fusing current for any wire, and the diameter of
the wire for any particular current; and I hope soon to be able
to supply engineers with tables that will give them the current
for every wire and for every metal, and the diameter for every
current and every metal.

I mentioned just now the fact that below a certain limit the
law that I put upon the board does not come out, but I will not
refer to that matter.   I see Professor Ayrton is here.   He pointed
out, in the discussion on my paper at the Royal Society, that it
is a necessary consequence from the known law that that should
not be true for fine wires.   He will give you reasons, I have no
doubt, himself presently.   I only want to point out that, as you
will see, those results come out wonderfully exact, when we
remember the great variation in the gauge of wires as supplied
by manufacturers.   When we remember the variations that exist
in the quality of the material supplied, it is not surprising that
cut-outs vary from 5 to 10 per cent. from the figures that are
generally attached to them.   If I tell you from my tables that a
certain size of wire will go with 10 ampères, I will not guarantee
that that is true within 5 per cent., because I do not believe there
is a single manufacturer in this country who will guarantee the
quality of the wire that he supplies to us to 5 per cent., and I

Mr. Preece. do not think it is possible to draw these malleable wires down to
a diameter that can be exact to within 5 per cent. If in our
cut-outs we get within 10 per cent., we shall be perfectly
satisfied, and my own impression is that we have certainly got to
10 per cent.

I do not object at all to protecting the two poles, say, of an
accumulator, or the two poles of a dynamo in a dynamo-room,
with separate and distinct cut-outs in different parts of the
building; but I do object most strongly to these compact
cut-outs being put in rooms, in halls, and in passages where,
by merely taking out the face of the case, a key, or a little
piece of wire, will short-circuit the whole of a circuit inside
the cut-out, and where the cut-out would be of no use at all.

I said just now that I rather like platinum, and I will tell
you why. I have one great objection to these cut-outs, and that
is their multiplication of bad joints. Every single screw is a
loose joint, and, sooner or later, unless it be constantly screwed
up, it is certain to produce a fault. This screw, as a rule, simply
binds down with a little tension a metal that is alterable. Nine
times out of ten in an old installation you will find lead, or an
alloy of lead, that oxidises on the surface. If you will go to any
installation that was put up five years ago, you will find that the
cut-outs are coated with an oxide of lead; you will find that
sometimes they have a greenish tint, generally with a white
tint; but you will never find them bright and clean. But I say
for small cut-outs there is nothing like platinum. It is cheap
when fine, it is unalterable, it never oxidises, you can screw it
end on end, and you will find that it will remain there in perfect.
condition for a very long time. It takes solder with the greatest
possible ease, and from the ratio I gave you just now, if a
current, say of 1, will give the sensation of warmth, a current of
2 will give the red-hot current, and a current of 3 will fuse it.
If the platinum cut-out is in a visible position (and every cut-
out should be in a visible position, never under the floor or
behind skirtings and in all sorts of inaccessible places), and
the current is gradually increased to a dangerous limit, the
platinum will become red-hot and give notice that there is

danger.   I say, therefore, that a platinum cut-out is an excellent Mr. Preece
and capital detector ; and in engine-rooms and battery-rooms,
by properly proportioning the cut-outs, the sensation of warmth
can be obtained, and the temperature of the wire observed, and
the cut-outs can be made to act as a species of detector and
be of great value to you.

Professor AYRTON, on being called on to speak, said that as Professor
Ayrton.
the substance of the remarks made by him at the Royal Society
on the occasion of the reading of Mr. Preece's paper would be
found in the paper by Professor Perry and himself in the
number of the *Journal of the Society of Telegraph-Engineers
and Electricians* just published, he did not propose to enlarge
further on the subject at present.   He would, therefore, only
mention that a series of experiments were being carried out at the
Central Institution for the purpose of ascertaining how the law
that to maintain a thick wire at a particular temperature above
the surrounding space required the current to be proportional to
the diameter raised to the power three halves merged into the
law that the current should be directly proportional to the
diameter for fine wires.

Sir DAVID SALOMONS : There is not very much that I have to Sir David
Salomons.
say on this subject except of a practical nature.   Fuses have
been my bugbear from the beginning ; and in the olden day (I
am speaking of as far back as 1874), when the wires were run
very much as bell wires are now, and perhaps not so well, cut-
outs were invaluable, for then the means of regulating dynamos,
the speed of the engines, and the methods of checking the
current, were unknown, compared with the knowledge of the
present day.   Now, with our incandescent lamps, the importance
of having an approximate definite point to cut out at is a matter
of practical necessity ; hence a welcome to reliable cut-outs.   The
effort to protect each lamp is practically impossible, nor is it
desirable, for, were the electro-motive force to rise, the increased
current is distributed over the whole system, so that one main
cut-out ought to go before any damage is done, and the lamps
are strong enough to withstand a considerable increase of current
above the normal ; so that there is no necessity to have that very

Sir David
Salomon«.
narrow margin that has been struggled for in the paper of
Mr. Cockburn.

But there are two most important points that have not been
dealt with at all, which I think enter far more into the question
of how metal fuses act than anything that has been mentioned
to-night, or on the last occasion ; because, interesting as those
experiments were, I think you will admit that 'they have been
simply a series of laboratory tests, which have yet to be verified
in practice in houses and in other situations.   Nothing has been
said as to how metal acts under direct or alternate currents.   I
cannot answer the question myself.   I rather put it to gentlemen
who are perhaps able to answer it.   I have a suspicion in my
mind that with alternate currents the metal acts in some different
way—not necessarily from the current itself, but from some mole-
cular action that may take place within it with alternate as
compared with direct currents.   It has long been supposed that
lamps broke chiefly at a particular point with a direct current,
according to whether the positive current entered one way or the
other.   I do not know whether there are sufficient observations
on record to show whether such is the case.   Probably all of us
here would like to know how many of the experiments men-
tioned, were conducted, not only by Mr. Cockburn, but also by
Mr. Preece, and what was the nature of the current employed
in each case.

Another question is how long the fuses experimented upon
were used.   I maintain most strongly that a fuse made to-day to
go at 5 ampères will not necessarily go at 5 ampères if it has
been in use for a very long period.   My aim has been to find a
successful fuse.   I have used Mr. Hedges' fuse, sometimes with
success, sometimes with failure.   I have had one that was to go
off at 30 ampères, and, taking several that were apparently of the
same size, they went with 30 ampères; but I have also one of
30 that after long usage will carry 100 ampères with perfect
safety: I believe it is going to this day, used with two or
three motors.   I believe it is the experience of a great number
of persons that fuses alter in time in two ways.   Mr. Alexander
Siemens, although he has just now refused to speak on this

question, has, in the committee-room, stated (and I do not think Sir David Salomons. it is unfair to make a reference to it) that most metals, especially alloys, were subject, after a time, to slight oxidation, which would increase until the fuse would go when not intended. My own experience is rather the other way; that is, with long use, if the fuse will stand the current intended for it, you can go on increasing the current, without injuring it, beyond the limit. Whether this is due to a species of crystallisation in the material itself, or oxidation, or an alteration in its form—that it becomes thicker by continual warming—I cannot answer faithfully, but I merely put forward these remarks that some gentleman who has made experiments on these points may give us his experience later on.

In his paper Mr. Cockburn, perhaps unintentionally, made but a slight allusion to some magnetic cut-outs made by Woodhouse & Rawson. They were really devised by my friend Mr. Cunynghame, and they were entirely his from beginning to end. It is a great gain to this Society that such a gentleman, who is a most able mechanician and inventor, has just been elected a member. I am sorry he is not here to defend his own device. Of all protectors that I have ever tried it is the only reliable one—it has the objection of mercury contacts, if it is an objection—and it goes off at the exact setting. If the ends which dip into the mercury are pointed there will be no splash when the cut-out "goes." In my own case this accuracy is most invaluable. All motors are protected, firstly, with fuses, for certain protection against serious damage; but the accurate limit at which the current should be cut is set on a cut-out of Mr. Cunynghame, and I have never found on any occasion, with arc lamps or motors, that it has failed, when once set—whether it be for 1, 10, 50, or any other number of ampères—by one ampère, and that is saying a good deal for the protection it offers.

There has been a tendency in late years—or months, I might say—to use china as the base to fix cut-outs to. I believe myself that it is a most dangerous precedent, and I hope that it will be very soon discontinued. The only accidents I have ever had have been with this kind of base, by men screwing them up a little too tight: the least thing then cracks them; and the result is

Sir David
Salomons
that when the fuse melts it may set fire to the wood behind it, or even cause in some situations a partial or complete short-circuit without your knowing it.

I quite agree with the remarks that have been made as regards double-pole cut-outs. They are in many cases a source of danger, except in the mains at points where they are well separated. I have not found that in all cases they both go at the same time, although it is a very common occurrence. There is no doubt that there is a tendency to have switches and cut-outs multiplied infinitely. Every switch loses a certain amount of power, and so does every cut-out. General Webber has pointed out privately, not before this meeting, the importance of having spring contacts instead of direct screw pressure; that screw pressure invariably brings trouble after a time by the metal becoming compressed, and that it becomes at once a bad or broken contact. By making fuses in such a way that they act also as switches you avoid two or three bad or partial contacts, and save expense besides. To show the importance of this in practice, in making certain alterations in the switch-boards at home, all the switches had to be taken down and the cut-outs simply left in. The switches had been made as massive and of as good construction as possible; notwithstanding, it appears that they absorbed, in charging the accumulators, two or three volts, representing one-third horse-power loss.

The reason that lamps had to be protected in former days was chiefly because they were very badly made, principally from want of skill. Going back four years, for every 100 Swan lamps that were put up, 70 of them went in each year; now I find something like 7 or 8 only go a year. That shows that the lamps are very much better made and require less protection than they did in the olden days, also that the governing of the current is better understood. For my own part, I would rather any day see a lamp go up bright, and at once seek the cause, than have a cut-out go and leave the room in darkness. In several houses where each lamp has been protected, it has been the only source of trouble experienced. Cut-outs are best placed in the switch, or close by, for incandescent lamp circuits, for the reason

that such positions are accessible and known; also, safety is Sir David Salomons.
ensured, because short-circuits almost invariably take place after
leaving the switch. I will not trouble you with any more
remarks, as I have no doubt there are many other gentlemen who
are desirous of speaking.

Mr. R. E. CROMPTON: I have read Mr. Cockburn's paper with Mr. Crompton.
interest, and, after listening to the discussion, I remark that the
author and various other speakers have stated that this subject
has hitherto not received sufficient consideration. I do not
think this is fair on the various firms who have been manu-
facturers of electric lighting plant during past years. I believe
that these cut-outs have already received a fair share of attention,
and, speaking for my own firm, I may say that we commenced
making improvements in these fusing cut-outs quite five years
ago. Some of those put into our earliest jobs are still giving
great satisfaction, and are answering their purpose just as well
as the more modern inventions. We found out years ago that
tin wire was the best material to use for the fuses, and we have
continued to use it ever since; and it is my personal belief that
plain tin wire is quite good enough for our purpose. In fact, I
know that tin wire fuses put into some of our jobs five years
ago are still in use; the wire is still unoxidised, and fuses
within a sufficiently close percentage to be of practical use.
I am not one of those who believe that it is necessary that fuses
should be made to act at such an extremely close percentage to
the determined current.

I have been disappointed not to hear more said in the course of
this discussion as to the best position for placing fusing cut-outs
in an installation. To my mind this question is of far higher
importance than that of the material of which the fuses themselves
are composed. The problem we have to solve in placing the cut-
out is that, whereas the most convenient position for inspection
and general accessibility is close to the switch, yet this is
decidedly not the best place for the fuse, which should be as
near as possible to the junction of the small protected wire with
the larger wire. Very often this junction is in an inaccessible
place, so that we are obliged to compromise, and in order to get

Mr.
Orompton. the fuse conveniently placed we leave a portion of the thin
branch unprotected.

My firm has carried out several installations under specifications
from Mr. Henry Lea, of Birmingham, and I wish to notice the neat
way in which he arranges the grouping of his fusing cut-outs.
He places them in groups in a glass-fronted case; the main wires
run along the passages close to the top of the walls; the case
containing the group cut-outs is placed close to the main wires,
and the branches are led away from them to the various rooms.
This arrangement necessitates some increase in length of branch
wire used, but it offers many advantages, giving facility for
testing as well as convenience for replacing the fuses. I daresay
that most of you know that the weak points of the insulation of
our modern installations of incandescent lamps lie in the switches,
fusing cut-outs, and lamp mountings, not in the wires themselves.
It is quite easy to get an extremely high insulation, amounting
to several megohms, if the testing is carried out previous to the
fixing of the above-mentioned accessories; but the instant they are
connected the insulation of the system, as a whole, falls, some-
times to a very low figure. The fire insurance companies have
recommended us—in fact, ordered us—to use slate, or other incom-
bustible material, for the bases of the switches and fusing cut-outs.
Slate is a nice material to work, but it sometimes, unfortunately,
has a very low insulation, probably due to veins of oxide of iron.
Porcelain or vitrite fittings, again, condense on their surface
films of moisture from our damp London atmosphere, and we find
when these materials are used it is quite impossible to obtain a
high insulation for the whole system. In a very large installa-
tion at Vienna, after great labour, we got the insulation up to a
very high figure, until we coupled in the main switch-boards,
and then we found quite a large leak; in fact, the total resistance
was cut down from 1,200 ohms to 300 ohms; and we found the
fault was in the material of the slate base of one switch-board.
These difficulties do not occur where hard, dry wood is used,
such as boxwood or beech. Fittings mounted on such
material have a very high insulation resistance, and it is quite
a question whether the advantage gained in this respect does

not counterbalance the very remote danger of their taking fire _{Mr.} from the melting of the fuse wire; in fact, I challenge all the ^{Crompton.} gentlemen who talk so glibly of tin wire getting red-hot and setting fire to its base or case, to produce such a fire by any short-circuit such as is likely to happen in practice. I believe that they would have to melt many thousand pieces of tin wire before they would produce the least charring or damage to a hard wood mounting. An immense amount of nonsense has been said and written on the subject of fire risks from electricity. These risks are so infinitesimal when the installations are carried out by men who know their business that it makes us electrical engineers lose patience when we hear the interminable discussions arising out of them. This question of the fuses setting fire to their bases and cases is one of them, and I here protest against such ideas gaining currency. I believe that a well-designed cut-out, made of hard, well-seasoned wood containing a tin wire fuse, is for most purposes a far better and safer thing than the slate and porcelain ones which have been so strongly recommended. I think Sir David Salomons is quite right when he animadverts upon the porcelain mountings, for they are so fragile that they are extremely likely to be damaged in fixing, or if afterwards removed by anyone but an extremely skilled mounter. I wish to say a word in corroboration of what Mr. Preece has said about double-pole cut-outs. These are an abomination, imported, I believe, from America. As Mr. Preece has pointed out, they are of themselves a great source of danger, and in no way can it be argued that they possess advantage of convenience over two single cut-outs, one put on to each main.

Mr. A. BERNSTEIN: I did not have the pleasure of hearing _{Mr.} Mr. Cockburn's paper read, and I can therefore make no remarks ^{Bernstein:} on it; but it seems to me, aside from the construction of the cut-outs, their arrangement in the circuit is a matter of considerable importance. Nothing is more inconvenient than to have the cut-outs scattered all over the building, sometimes in places which are very difficult to get at. It seems to me that they should be arranged in a somewhat systematic manner. As an example, I should like to describe an arrangement which I

Mr.
Bernstein. carried out a few years ago in a large dwelling-house in London, and which has proved very satisfactory.

The two main wires were carried in casings from the cellar to the top floor.   On each floor a separate branch led, first, to a slate board, on which the cut-outs for that floor were placed.   From this cut-out board the separate branches were carried directly to the different fittings.   Double-pole cut-outs were used throughout, but I did not consider it necessary to provide a cut-out for every single lamp.   Where several lamps were placed on one chandelier I was satisfied to use a double cut-out for the chandelier.   The slate boards were protected by means of a glass cover, and there was no fear of any short-circuit being made between the main wires and the double-pole cut-out.   In addition to the cut-outs in each branch, double-pole main fuses were used corresponding to the total amount of current to be used on each floor. The slate board contained another useful addition, and this was, that plugs were inserted in every branch.   By withdrawing these plugs each branch could be entirely separated from the rest of the circuit, and in this way be tested for insulation.

The whole arrangement has proved satisfactory.   In this installation I have used the Hedges cut-outs, which I found to be very reliable.

Mr.
Blakesley. Mr. T. H. BLAKESLEY : I am not prepared to take part in this discussion, but there are one or two points upon which I might perhaps say something.   Mr. Preece said something, which I think might be questioned a good deal, as to certain wires, when coated with shellac and used as fuses, starting before their calculated current was reached.

Mr. W. H. PREECE : Oh, no.

Mr. T. H. BLAKESLEY : I fancy you said that some fuse went at 25 per cent. below its designed current.

Mr. W. H. PREECE : I only said that in the case of tin the shellac acted as a flux and prevented oxidation.

Mr. T. H. BLAKESLEY : Well, even in the case of an unoxidisable wire, I should fancy that it would fuse before the current was reached which was destined to fuse it as a bare wire.

Mr. W. H. PREECE : No.

Mr. T. H. BLAKESLEY : Well, if the tin under those circum- stances went at all before its calculated time when not covered, I, for one, should not be in the least degree surprised—not because the conducting power of the wire is at all changed by the oxide or flux (possibly), but because the heat is not radiated or convected off as rapidly as it would be if there were no covering. That is the reason why such a wire goes before its time, not because it is chemically changed ; and Mr. Cockburn, as I understand, did not say that the tin wire was very liable to oxidise. I think his ground was that tin wire did not so change ; but, whether he said so or not, it is true that tin would not oxidise in comparison with most available metals.

Then as regards Mr. Preece's bursting fuses. That point I touched upon some long time ago—I think before this Society, in this room. He would find, I think, if he examined the case, that those fuses which burst were made of metals which expand when they fuse. Especially is this the case with lead. If a current is passed through lead wire it bursts with very great violence, which quite surprises anybody who is accustomed only to the soft nature of that metal. Liquid lead is very much more bulky than solid lead just about the fusing point, and the inside of the wire being at a much higher temperature than the outside, fuses, and bursts the still solid outside skin. In my opinion, at least, this is the proper explanation, and that is one of the reasons why I should prefer a foil to a wire. A circular wire is a figure which has the largest amount of cross section compared with its circumference, and therefore you would have, for a certain amount of cross section, the steepest heat gradient inside that wire. If you want the whole of the inside of the fuse to be as near one temperature as possible, so that the whole fuses together, you must thin out your material, i.e., use something of the nature of a foil instead of a wire.

A very important point was touched upon by Sir David Salomons as regards the source of electricity in any of these experiments ; and anybody who says that a fuse goes when it is not calculated to do so ought to state what his source of electricity is. If he can show that his current is a perfectly steady

Mr.
Blakesley.

one, well and good ; or if his current is a purely alternating one, there is no great question unsettled ; but if his current is a direct one, and at the same time not a uniform one,—if it is at all an unsteady current, always in the same direction, perhaps, but not constant,—it is virtually a uniform current plus an alternating current ; and if that current is measured on an ordinary galvanometer you will only have the mean value of that current given you, but the heat generation will not be in proportion to the square of the mean. The heat developed will be in proportion to the square of the steady current plus half the square of the alternating portion of it. If I may go to the board, I can show you, perhaps, a little more clearly by diagram.
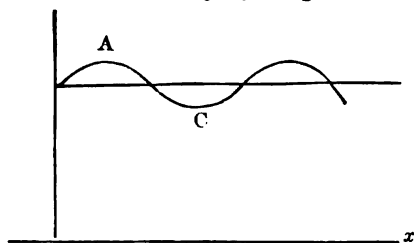
If the current may be represented by the ordinate of a curve, and the process of time by the velocity with which that ordinate moves in a direction at right angles to itself, we have a very usual and a very expressive method of depicting what goes on. A perfectly uniform current will be represented by a straight line parallel to the axis of x ; a simple harmonic alternating current by a curve which will be as much above as below the axis of x. But a current which, though always in one direction, has not always the same value, will be expressed by a sinuous line, as A C, at some distance above the axis of x. Now such a current will develop heat at a rate which is the sum of the rates due to a uniform current and to a purely alternating current respectively. These two currents must be such that, when superimposed one on the other, they will produce the actual current. Now a galvanometer will entirely ignore the alternating portion of such a current, as would also a voltameter, but not so a dynamometer or a safety fuse. If, therefore, a fuse is to be satisfactorily tested, either a dynamometer should be used to determine the

current, or, if a galvanometer is employed to measure, batteries should be employed as generators.

Mr. J. FARQUHARSON : I must say that I agree very much with what has fallen from Mr. Preece and from Mr. Crompton as to the use of fuses. I think it is overdone, and I have had no case within my experience in which a hard wood block, such as Mr. Crompton has suggested, would not have served the purpose as well as, or better than, porcelain. Before Mr. Cockburn replies, I should like to suggest to him some further explanation with regard to one point in his paper—this question of explaining the difference in the melting temperature of the same wire—by a suggestion that the oxide has something to do with it. Mr. Preece's experiments confirm Mr. Cockburn's; but I am almost as much in the dark as ever as to how this oxide, if formed, does preserve the wire. The melting point of a metal is, as I understand it, that temperature at which the cohesion of the metal no longer is able to withstand the force of gravity. I do not see how an oxide, if formed, would affect that, unless it be supposed that the oxide has some cohesive strength in it. Mr. Preece has pointed out that when the oxide is prevented by shellac the same effect is produced. Well, shellac is a very bad conductor, I admit, and might have something to do with the radiation of heat from it, but that should hasten the melting of the fuse. In the case of oxide, the film, I should think, is so excessively minute that its strength would not be appreciable. The practice that I follow in the fixing of fuses is that hinted at by Mr. Crompton : put one as near the root of every branch as we can, making use of a switch with a fuse when it is possible to do so.

Mr. W. H. PREECE: Might I just give the result of one experiment that I think, Sir, will clear up a little mist, especially in Mr. Blakesley's mind ? If we take tin ·018 inch (18 mils.), with no cooling effect of the terminals, it goes with 4·34 ampères. If this wire has upon it a thin flake of shellac, it goes with 4·9 ampères; if it have no shellac, and simply be affected by the cooling of the terminals, it takes 6 ampères before it goes. This effect Mr. Cockburn attributes, and I believe correctly, to oxidation—an effect that only occurs

Mr. Preece. when the wire reaches very close to its temperature of fusion. Before it reaches the temperature of fusion the surface seems to be coated with an oxide, and then it acquires a higher temperature than its own temperature, and that is shown by the fact that the wire becomes red-hot before it fuses. I mentioned that a red-hot body has reached a temperature of 525° C., and I think that the fusing temperature of tin is considerably lower than that—it is somewhere about 400° C.; and I think that will answer both Mr. Blakesley and Mr. Farquharson.

Mr. Crookes. Mr. W. CROOKES: Some years ago I tried some experiments with different metals for safety fuses. If I took either tin or lead, almost chemically pure, and kept to the same sectional area, it was not difficult to get uniform results; but I found it almost impossible to get two different specimens of the commercial metals that would give anything like uniform results. A trace of impurity in commercial lead or tin would give a difference of 10, 20, and even 30 per cent. in the amount of current they would carry without fusing.

The effect that Mr. Preece has mentioned as to tin forming a skin of oxide outside whilst the metal becomes red-hot within, is not at all unlikely to take place. I have noticed it myself, and the explanation Mr. Preece has given I believe is the correct one. There is another thing to be borne in mind: what is called tin in commerce is frequently an alloy of tin and lead; and when mixed alloys of metals are raised to a gradually increasing temperature the most fusible metal has a tendency to liquefy out and leave the least fusible metal in a spongy solid state. So that if the current rises very suddenly the wire will fuse at a lower temperature, because it gives way at the fusing point of the alloy; whereas, if the current rises very gradually, the most fusible metal may liquate out, and the residue fuses at a higher temperature.

I have tried some experiments on an entirely different kind of cut-out, which have not yet been published, and it may be interesting to the meeting if I allude to them. It is known that the metal nickel is very magnetic, but it loses its magnetism at a temperature of about 250° C. I arranged

as part of a main conductor a piece of nickel plate, held Mr. Crookes.
in position by a permanent magnet, and having a weak spring
tending to pull the nickel away, but not strong enough to
overcome the force of magnetic attraction. When the current
went through this piece of nickel, as long as the metal remained
cold nothing took place; but when the nickel got to a certain
degree of temperature, which was arranged beforehand by
adjusting its sectional area, it got hot, lost its power of
attraction by the magnet, flew away, and broke the current.
The advantage of this instrument is that nothing is destroyed;
when we want to reset the apparatus we have only to put the
nickel down on to the magnet and it is again in working order.

The PRESIDENT: Mr. F. Bailey, your connection with the The President.
Paddington installation may have given you valuable experience
in this matter?

Mr. FRANK BAILEY: I regret that I have nothing to say on Mr. Bailey.
the subject at present. At some future day I may have some-
thing to say.

The PRESIDENT: Does any other gentleman wish to make The President.
observations?

Mr. W. B. ESSON: I should like to have an opportunity of Mr. Esson.
endorsing all that Mr. Crompton has said regarding fuses and
switches. I think more nonsense has been talked about fuses
and electrical fires than about any other subject whatever. I
have often tried to set fire to hard wood with a fuse, but have
never succeeded. Once, and only once, have I seen a fire caused
by a defective switch; but that was a switch of the old-fashioned
type, without a spring cut-off, or anything of the sort, to cause a
quick break. The hindrances which are being constantly placed
in the way of electrical engineers are ofttimes very troublesome
and very unnecessary. I know of a case where an insurance
company insisted not only upon having all the fuses taken off
their wood beds and placed on slate, but after that was done, and
the whole placed upon a tablet about 3 feet square, they further
required the tablet to be covered up by what could be likened to
nothing less than an iron safe, having a door under lock and key
—I suppose to prevent an electric fire.

Mr.Mordey.   Mr. W. M. MORDEY said that he agreed with Mr. Crompton as to slate, which was a fairly good substance to work, but was not a sufficiently good insulator for high-tension working. He had, however, found that by boiling it in paraffin wax it became an excellent material. It was best prepared by being placed in paraffin wax so that the bottom was covered, and then kept heated until the whole of the surface appeared oily. After draining off the excess of wax it was ready for use. This treatment rendered slate an excellent insulator, and one which was practically incombustible. Referring to the use of electro-magnetic cut-outs, he agreed with Professor Crookes that they were quite satisfactory, but, as in all cases where mercury contacts were used, there was an objectionable arc and splash when a large current was broken. This could always be avoided by using a very thin piece of wire as a shunt across the contact : then the mercury contact was broken without any arc, and immediately afterwards the little wire shunt fused and prevented damage to the contacts and splashing of the mercury.

He ventured to congratulate the author on his paper, and thought the use of the pellet of lead on the fuse, to ensure prompt rupture, was an excellent arrangement. He had often met with the difficulty, which the author had overcome in an extremely neat and effective way.

Dr. Fleming.   Dr. J. A. FLEMING : I should just like to say one word to confirm the experience of Mr. Crompton in regard to the bad insulating qualities of many varieties of slate. I am inclined to think that the varieties of slate differ very much in insulating quality. I had some lamp sockets placed in my hand the other day in which the brass parts had been nearly fused in consequence of the slate possessing very small insulating quality. There are other materials besides slate, such as serpentine, which can be worked well ; and bases of this material have a higher insulating quality than many varieties of slate.

There is one thing that has not been dwelt upon at all this evening by those who have spoken on the subject, and that is the question of safety cut-outs for alternating currents of high E.M.F. The consideration of the form to be given to a safety cut-out, or safety catch, to prevent the starting of an arc, is to

some extent important even for low potential; but it becomes _{Dr.} _{Fleming.} very important when dealing with 2,000 volts or 10,000 volts, and it is quite impossible to use the forms of safety catches adopted for low electro-motive force for high electro-motive force.

One point in connection with Mr. Cockburn's interesting experiments comes out very well in some of the curves of the middle diagram, in which the curving part of the curve, before it becomes flat, shows very strikingly indeed the conductivity effect of the ends of the wires. That has been discussed theoretically by Sir William Thomson in a note to a paper by Mr. Bottomley, who has been experimenting on the energy expended in heating wires; and the question was previously examined by Professors Ayrton and Perry. The conductivity at the ends is of course very much affected by the form of the attachment, and I should like, if there had been time, to have asked some questions about the form of attachment which has been employed for the wires in the case of the experiments recorded in the middle diagram. In the bottom curves, we see the curves get flat much sooner, and that shows that there is much less proportional conductivity effect at the ends; and that is, of course, due to a great extent to the fact that the leads for the small currents are much smaller than the leads used for the large currents.

Mr. F. Brown: I should like to make one remark as regards _{Mr. Brown.} the danger of wood cut-outs, and also of china ones. A point has been missed as regards the dangers of cut-outs generally, except so far as it was touched upon by Mr. Preece—that is, loose contacts. I have seen wood cut-outs burned quite sufficient to make the insurance companies grumble, due to the wire getting loose and arcing. In one case I found that the lead had become loose and the bad contact had generated sufficient heat to completely burn away the wooden block. I think if we were not to depend upon screws, but to let every joint be soldered, we should not have so much danger. In a building where a great deal of steam was being used, the china cut-outs were a source of danger, because the steam condensed upon the china, and there was a constant film of moisture across which there was a steady leakage to earth.

Mr.
Bernstein.

Mr. A. BERNSTEIN : It does not seem to be generally known that there is a peculiar quality of slate found in Galicia which has quite different qualities from the English slate. It does not split, and it is entirely saturated with hydro-carbon ; its insulating qualities are far superior to the English slate, and it is very easily worked. This slate is found in the petroleum region of Galicia.

Mr.
Handcock.

Mr. H. W. HANDCOCK : While carrying out the experiments for Mr. Cockburn's paper, it struck me that the following might account for the fact that a tin wire, when loaded in the centre, breaks in the granular state on passing a current through it, while, if it be unloaded and not very long, it reaches a high state of fusion before breaking.

I have in my possession three samples of tin wire, moderately thick. Now each piece was cut from the same length, and in its turn clamped in the terminals which were here for your inspection at the last meeting. Sufficient current was passed through each wire to break it. The lengths taken between the terminals were 1, 3, and 5 inches. On examining the remains of the wire 1 inch long, it was found that before breaking it had become so molten that on fracture about half an inch had fallen away in the form of a bead of fluid metal. The state of the piece 3 inches long, on fracture, was, however, only semi-molten; for from the end of each fragment was suspended a long globule of metal, which had been semi-molten, but did not contain heat enough to keep it in that state over a sufficiently long period for it to detach itself from the cooler portions of the wire after the circuit was broken. The third piece, 5 inches long, was evidently only plastic, for it had merely broken and bent round to a gentle curve with its own weight.

Now take a wire loaded with a ball having about ten times its weight, as is the case in practice, and it will be found to break with a current a trifle below that required to break an unloaded wire : this break is granular. Repeat the experiment, using a much bigger weight, but still the wire breaks with the same current. Hence the capacity of the ball for heat does not alter the breaking point, provided the current has been allowed to pass long enough to bring everything to the steady state before raising it to its breaking value. Also, the exact size of

Mr. Handcock.

the ball is not of much importance, provided it is of fair weight compared with that of the wire, and not so big as to absorb all the heat when a breaking current is put on suddenly. What the above experiments show is, that if we take 'a wire which, when unloaded, will become molten before breaking, and put a weight on the centre, it breaks with almost the same current, and therefore almost the same temperature, and yet the break is granular; and also, if this weight be greatly increased, the breaking point is not altered. That is to say, that at a certain temperature the state, and therefore what we may call the strength, changes with great rapidity; in fact, we may express it by a curve of the character shown in Fig. 1. The region a of this curve represents
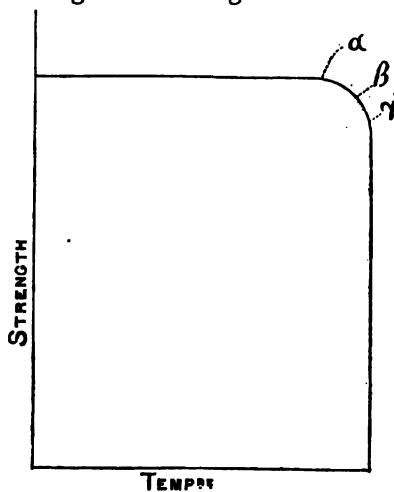
FIG. 1.

the granular state, β the plastic state, γ and downwards the molten state. For we found that as the length of the wire increases, and therefore its own weight, tending to break the wire, increases, fracture occurs at a period higher up in the bend of the curve, until, when the wire becomes long enough, or sufficient weight is added, the region where the break is granular is reached. After this, addition of weight does not alter the breaking current; that is to say, the strength of the wire is practically constant. The difference of temperature requisite to transfer the breaking point from β to a must be very small, the difference of current in some cases being almost inappreciable.

Mr.
Handcock.
If the wire be not broken at a, when there is little or no oxide, the oxide that forms after this becomes an important consideration, and forms, as we get round the bend, a casing which holds the metal after fusion. The whole has then to be heated up to, or above, red heat before fracture takes place. On examining a loaded wire broken by passing the current, its stucture, as seen under the microscope, is quite granular.

The sudden rise in the resistance of the unloaded wire before breaking shows that the change of state is rapid; but when the wire is loaded this sudden rise in resistance is not experienced. The above hypothesis would lead us to expect this, as the break in the latter case occurs before the real change takes place. I do not believe there is anything in the above remarks that clashes with the theory of change of state.
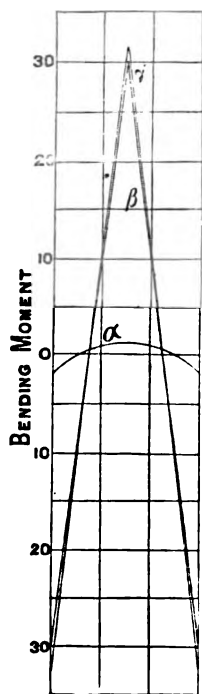
The following considerations show how favourable the placing of a weight at the centre of the wire is to breaking, and may account for the fact that the loaded cut-out, when used horizontally, generally breaks near the centre. First consider the unloaded wire as a beam fixed at both ends and loaded evenly all over with its own weight. Its bending moment is then represented by curve a (Fig. 2), taking the length of the beam as 1 and its weight as 1. But I found that the ball at the centre weighs ten times as much as the wire itself in a sample I took. Consider the wire next, then, as a beam loaded at the centre with a weight value 10. Its bending moment is then represented by curve β (Fig. 2). Adding these two together, we get curve γ, which represents the total bending moment of the wire. This shows that the bending moment of a wire loaded in this manner is about 31 times as great as that of an ordinary wire.

Fig. 2.

The steepness of curve γ, together with the fact that the wire is

Mr.
Handcock.

softest where the distance of the wire from the terminals is greatest, would lead one to expect the wire to break as near the centre as possible.

If the hypothesis put forth in Fig. 1 be accepted as true, then the fact that the loaded wire goes at the same current, whether straight or bent into a loop, is accounted for.

Now the law $C = a\, r^{1\frac{1}{4}}$ only holds good when there are no end effects, and then for certain sizes only. (C = fusing current, r = radius of wire.) a is generally regarded as a constant, but as the values of C thus obtained are generally wrong, I have taken C and r from my curves and worked out the values of a from the formula. Fig. 3 gives the results plotted for different metals.
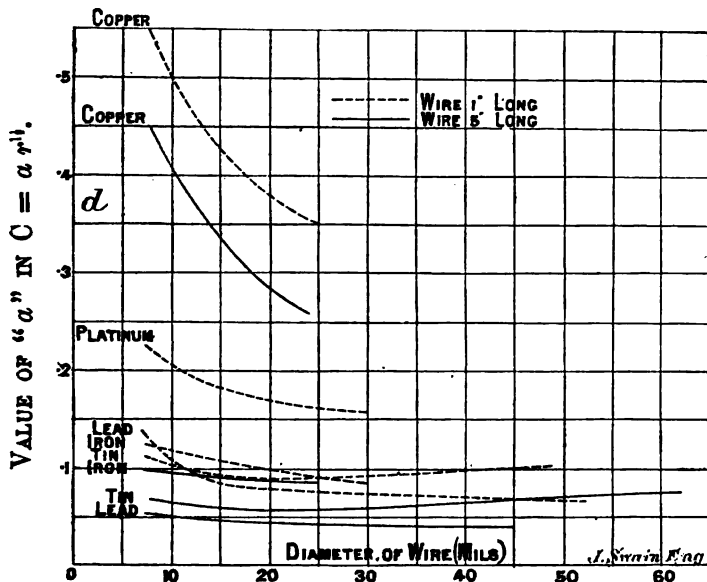
FIG. 3.

The results are all for the same terminals, as, of course, different terminals alter the value of a. a is calculated for two lengths of wire—1 inch and 5 inches. It will be noticed that as the conductivity of the metal decreases, the slope of the curve, and also the value of a, decreases.

The end effect is least for lead, its conductivity being least; and in this case we notice that for the smaller diameters the

Mr.
Handcock. curve slopes down and then, as r increases, becomes a straight line, a being then a constant. This seems to indicate that when end effects are annihilated the law $C \propto r^1$ may hold good for small wires, but gradually becomes $C \propto r^{1\frac{1}{2}}$ for the larger wires. I am at a loss to account for the curious dip in the curves for tin, unless it be that the quality of the metal used was varied to facilitate drawing into wire. In the curves for oxidation, about the same time was allowed in each experiment ; but too much trust should not be put in these curves, as oxidation is a matter of time more than anything else. It will be seen that in the curves on the wall connecting diameter and current the distance between each curve represents the end effect due to change of length.

The two curves represented in No. 2 on the wall being parallel, is a consequence of the law that if a bar be heated uniformly all along, and conductors be applied at each end to carry away the heat when the steady state has been reached, the heat conducted away is constant, irrespective of the length of the bar.

Copper does not appear to be good, as I have noticed in some cases that if a copper wire be made nearly red-hot by passing the current, and then cooled down, the oxide flies off, leaving a bright surface, and so the process of reduction of cross section proceeds. It is also one of those metals particularly liable to bursting when short lengths are used, requiring a big current, even if the current be raised slowly.

Since, as we are told, cut-outs are most useful when there is a sudden rise in current, there would scarcely be time for the engineer in charge to take action, after noticing a platinum cut-out to be red-hot, prompt enough to save damage. It therefore appears better to use such a metal as tin, which melts at about 230° C., instead of platinum, which melts at about 2,000° C., and which, being red-hot over such an extensive range, has so much better a chance of communicating fire to its surroundings.

I have noticed, on heating up an alloy consisting of copper and zinc, that the result was simply that the zinc volatilised off, leaving behind the copper. This is fatal to the use of such alloys for cut-outs.

It has been urged that cut-outs are overdone in installations. Mr. Handcock. This is true enough if an experienced electrician is in charge, and everybody about the premises has a knowledge of electricity. Many electricians say that abnormal excess of current is impossible, and so are short-circuits. But if, as is now very often the case in London, installations are put in to be run by the porter on the premises, who has to pick up what knowledge he can of the subject, a short-circuit of the dynamo, accumulators, lamp-holders—especially as many are now made—or other part of the circuit, is the likeliest thing in the world. Only a short time back it happened in the City that a pair of scissors was laid on a desk, and accidentally on the connections of a lamp, when, to the surprise of all, the scissors melted! What more likely to produce a destructive fire? With efficient cut-outs on the circuit this accident could not possibly have happened.

If, instead of each lamp being protected by a cut-out, there is only one to a room, when this one is called into action—as, for instance, by the short-circuiting of the lamp-holder—the whole room is put in darkness, instead of the defaulting lamp-holder only being cut out. I just wish to call your attention to an error in the diagram of melting curves for iron which accompanies Mr. Cockburn's paper. For ordinates the draughtsman has marked half-ampères, not ampères. That is to say, 34 ampères should be read 17 ampères.

Mr. A. C. Cockburn, in reply, said: As the time is so very Mr. Cockburn. late, I will not go into details of discussion very fully, but will touch upon the main points.

Professor Thompson asked a question with regard to the 1¼-inch length. I find for small-size fuses that 1¼ inch is certainly the best length to take; but when one uses wires of a larger diameter, then it is preferable to increase that length, as my three types of fuses show.

The Siemens fuse I referred to was sold to me as a 5-ampère fuse, and I quoted it as such. As to the tension of the wire referred to by Mr. Hedges. He is evidently confounding two distinct forms of fuse, viz., the forms usually attributed to Sir W. Thomson, in which *springs* are used, also a joint of fusible

Mr.
Cockburn.

solder, and my entirely new form, in which *no spring* or joint of fusible solder is used, but which consists of a strip or wire of tin, *weighted between* its points of suspension, and quite distinct in its action. Moreover, I would point out to him that safety fuses with springs (if I mistake not, identical in principle and construction with those of Sir W. Thomson) were the subject of a patent taken out by Mr. C. V. Boys and Mr. H. H. Cunynghame in the beginning of the year 1883—more than a year before that of Sir William Thomson. These were found to act satisfactorily for large currents, but not for small. In the case of the weighted wire, action is equally certain for all currents, large or small. Now the pull of a spring may alter with time; but that due to the weight method, being governed by gravity, only is constant, and, if the weight be properly proportioned, does not affect the diameter of the wire holding it up. It is therefore clear which system is the best to obtain certainty of action. In my experiments I did try springs, but found them very uncertain. For instance, take a fuse wire fixed at each end, and with a spring exerting a pull or tension at its centre. A current passing through such wire warms it, and therefore causes it to elongate. By its being fixed at the ends, and under the pull of the spring, such lengthening allows the wire to become curved, and on the current ceasing to flow the wire cools and does not regain its former state, but remains with a permanent bend or set; and on repeated heating and cooling, such as would take place in practical working, the bend would grow to such an extent as to render the spring useless, in addition to the bad effect of the wire becoming thinner, due to its increase in length. Attaching a weight does not cause the same error. I think I mentioned in my paper that my experiments were conducted with a strip of tin which was carefully gauged both for thickness and for breadth, my object being to verify the results of that against wire under similar conditions, clamped at both ends and used in a similar way. The results in the paper were the results of an average. I found that made-up mica fuses marked to go at 3 ampères usually went at 9 ampères. I should, if I had been issuing them, have preferred to call them 9-ampère fuses; in fact,

that is the plan which I adopt. I mark on the boxes which I
issue the current at which the wire should fuse; and I par-
ticularly aim that anyone using the fuses should know definitely
what they FUSE at. By that means a man will know, if he has
2 ampères flowing through the lead, that to allow 50 per cent.
margin he should put in a fuse that is issued to act at 3 ampères ;
and what I claim is that the fuse will act within 5 per cent. of
the particular fusing point that it is issued for, if used in its own
type of box, and therefore one really knows what is the *maximum*
current that can pass through the protected lead.

I am sorry I must differ with Mr. Preece with regard to
platinum ; and in fact for the smaller fuses I should more
particularly advocate a wire that could not get red-hot, which
platinum *must* do before it can fuse. Mr. Preece says that
such wire getting red-hot attracts attention to it when too
much current is passing. This may be all very well if the
user has the fuse always in view ; but in how many cases is such
the rule ? Far more often the fuse is placed in some out-of-sight
place, especially single and double lamp fuses, and enclosed in
a light-tight box. Platinum at twice the normal current becomes
red-hot, and does not melt till three times the normal current
passes through it. It is therefore clear to my mind that such
a substance cannot be either a safe or efficient fuse; and still
worse when used for a number of small fuses, dotted about all
over the place, each one with a chance of becoming red-hot, and
a source of danger, which a loaded wire such as I advocate cannot
become.   The expense of platinum must also be greatly against it,
compared with the cheapness of tin, especially for the larger sizes.

The device of the shellac is novel, but I should doubt its
uniform certainty in action.

As to the size of contacts or terminals to which fuse is to
be attached.   In my experiments I found the exact fusing point
of any wire greatly depended upon the mass of metal of contact-
pieces, and for this reason I divide my fuses up into four
distinct types, viz., A, B, C, and D, and construct four equally
distinct types of boxes, bearing similar letters, to go with same.
Thus, for example, all fuses of the A type only fit A type boxes,

Mr.
Cockburn. in which the mass of the metal contacts is kept constant,
B fuses the B type boxes, and so on; the fuses being tested
in connection with their own type of holder and fusing points
marked accordingly; so that one can be perfectly certain of their
acting within 5 per cent. of the current at which they are
issued to fuse at. Suppose it possible to fit it into a box with
a different size terminal, then it would undoubtedly be found
that the fuse would not be certain within the 5 per cent. limit
of its marked fusing point, such as it would if used in its own
box or holder.

As to the question of sudden short-circuit. This is not all
that must be guarded against; in fact, I maintain that what we
have to particularly guard against, especially in small fuses, is
any chance of the fuse becoming red-hot and dangerous, due to a
slight excess current passing through it not strong enough to
cause its complete rupture. If such a current passes through a
fuse wire, I maintain that the fuse ought to act, and that without
any chance of its first overheating. We know, of course, that
most fuses will act with a dead short-circuit. Take platinum : if
the current be doubled, platinum becomes red-hot, and yet the
fuse does not break. It has been said that platinum goes like
wax. Well, tin unloaded, I grant you, will scatter in all direc-
tions, making a regular splutter all about; but take the loaded
wire, as in my fuses, and you saw from the experiments I made
when reading my paper (I have the apparatus on the table now,
and shall be glad to repeat them) that the tin wire breaks off
·perfectly short, leaving just a little bit sticking out at either end
of the ball; it generally breaks about $\frac{1}{8}$ inch or $\frac{1}{16}$ inch on each
side of the ball—an absolute clean break, and no loose tin of any
sort or kind. I perfectly agree with the several speakers as to
double-pole cut-outs. I do not advocate them for every room,
but that every main lead, say from a dynamo or accumulator,
shall be properly protected with double-pole cut-outs. It has
been objected that when one fuse goes the other may go. Is not
that far better than that you should have a general burst-up of
your wires ?

Fear was expressed as to screws becoming unscrewed. As far

as possible—and I mentioned that in my paper—I always advocate Mr.
Cockburn. that not only shall a joint be as tight as it can possibly be screwed together, but that, where possible, it should be soldered. So the chance of danger is diminished ; and where the leads come in they should certainly be soldered in every case.

As to the oxidisation under the screw-head. If one uses a metallic " eye " similar to the one that I use in the fuse on the table, this objection falls to the ground, because there is no lead or tin to oxidise.

By experiments I have also found the fusing points of tin wire to be as Professor Ayrton spoke just now, viz., the current required to effect rupture proportional to the diameter of the wire; but when one gets to larger-sized wires, for larger currents, then the $\frac{3}{2}$-power law comes in.

The narrow margin for excess was referred to by Sir David Salomons. I prefer the narrow margin if a fuse can be depended upon. I do not say that on a circuit to carry 2 ampères a 2·1 or 2·2 ampère fuse should be put in—that would be far too near; but what I mean by the narrow margin is that the user of the fuse should know really within a very narrow margin what the fuse he is using will go at.

I was asked a question as to the nature of the currents used in my experiments. They were both from a dynamo and from accumulators. And as to the time, I have had some of the fuses running for a considerable period, after which they were tested and found to go at the two top points, due to any of the tin having got reduced or the wire getting hot and thinning. That is evidently due to the fact that if the wires got the least bit soft the effect of the weight would be felt and the wire broken. In fact, I put the weight at the point midway between the points of suspension, and at this spot the loss of heat by conduction should be least, therefore the spot which should first attain the plastic condition which the weight breaks through.

I certainly did not mean to cast any discredit upon magnetic fuses. I say that if they are properly looked after they are excellent, and I should advocate them on main leads; but it is quite clear that magnetic fuses cannot be put on all local circuits,

Mr.
Cockburn.

because they would be too expensive, and too troublesome to look to ; and it is on these local circuits that I say they are not the proper thing. As to the use of china, I have also found that it cracks if not used very carefully ; but that is got over if, instead of using concave bottoms, they are made as flat as possible.

Some gentlemen have spoken about the use of wood as liable to cause fire. If a fuse is used that does not get red-hot, I do not see that anything better than wood can be employed. But, on the other hand, if wires are used that can get red-hot, then I do say that wood is not the proper thing. Boxwood is the best wood for the small boxes.

In further reply to Mr. Crompton, I say that tin will oxidise when a slight current above the normal passes through it. I do not say that it will oxidise if you put a piece of tin capable of carrying fifty times the greatest possible current that can ever pass through it; though some people put such in, and call it a *safety* fuse. Such a piece of wire might be wrapped in tinder for all the harm (or good) it can do. I have, however, recently seen wood fuse-boxes that have been taken out of an installation fitted up by one of the speakers, the wood of which was burnt to a cinder, yet he emphatically asserts such cannot take place. With regard to slate. Slate, I know, has objections, not only on account of the mineral veins, but also because of the moisture which condenses on its surface. I have tried serpentine. I am now making tests with that substance, with the view of utilising it for cut-outs.

There are several other points, but I will not take up your time further. I have to thank you for the kind way in which you have received and discussed my paper, and trust that in the future it may lead to some definite and good results.

The
President.

The PRESIDENT : A very interesting paper has been followed by a most interesting and important discussion. One gentleman said that more nonsense was talked about cut-outs and safety fuses than about anything else connected with electric lighting; but it seems to me that it must be exceedingly convenient to have the power of assuring a timid intending user of the electric light that such provision exists, and that it can be relied on.

And at the same time it appears to be equally necessary that the person responsible for the installation should use it as sparsely as possible. I should hope that the discussion we have heard will tend in some degree to solve the vexed question of fire risks which is now under discussion. I rose thinking that I had to propose a vote of thanks to the author of the paper, but I find that it was already done on the 8th December last, and therefore I can only ask you to confirm the vote which you have already given him.

Mr. H. W. Borns wishes to introduce to the Society an instrument employed in Germany for detecting escapes of water from pipes, the passage of water through mains, &c. It is a micro-telephone arrangement mounted on a rod, which is placed on the pipe under examination.

Mr. H. W. BORNS : The instrument which I exhibit here by the kind permission of our President, and which I will briefly explain by your leave, promising not to detain you more than a few minutes at this late hour, is not mine, but the invention of Mr. A. Paris, of Altona, near Hamburg. It is a micro-telephone, and was kindly acknowledged as one of the many members of his microphone family by Professor Hughes in the discussion on the Telephone Researches of Professor Silvanus Thompson. The instrument is used for detecting water leakages from mains, service pipes, &c. It consists of a rod of a good homogeneous wood, to be placed on the pipe, and supplied with a cap into which the microphone fits. The rod is supported in a tripod stand, which can, however, well be dispensed with. The box has two removable nickeled covers. The microphone contains a carbon pencil, over which a carbon button plays. When not used the two are not in contact, contact being made by turning a screw. Another screw serves for adjusting the contact ; and the pencil may itself be turned by means of a small lever when the contact becomes corroded, so as to bring fresh surfaces into contact. One Leclanché cell suffices, attached to the tripod or worn by the observer by means of a belt. Connection is made by simply slipping little brass caps attached to the wires over the terminals. When the observer has placed the two telephones fixed on a

frame to his ears, he presses a push-button in the wires to close the circuit, and he then hears distinctly the taps of the water dripping from a leaky valve or the hissing noise of the water escaping from a pipe. The instrument has been in use for several years at the waterworks of Hamburg, Dresden, Bonn, Halle, &c., in Germany; at Riga, in Russia; Gotheborg, in Sweden, &c.; but this is the first specimen that has come to England, and I thought it might be interesting to the Society.

The PRESIDENT: Although the apparatus which Dr. Borns has introduced to us has been previously described in some of the technical journals, and was, as he has stated, alluded to by Professor Hughes in the discussion upon Professor Silvanus Thompson's paper in February last year, I am sure you will agree with me that our thanks are due to Dr. Borns for giving us the opportunity of having before us the first of these instruments exhibited in this country.

A vote of thanks to Dr. Borns was unanimously accorded.

The PRESIDENT: I now give notice that the next meeting of the Society will be held on February 9th, when a paper by Mr. Gisbert Kapp on " Alternate-current Transformers, with Special " Reference to the Best Proportion between Iron and Copper," will be read.

A ballot took place, at which the following were elected:—

Members:

Henry Hardinge Cunynghame. | John Clough Vaudrey.
Arthur Bromley Holmes. |

Associates:

Walter Poynter Adams.	Hugo Hirst.
Tom Scott Anderson.	John Kent.
James Bailey.	A. Macdonell.
Thomas Barlow.	Thomas E. Marsh.
Richard Henry Burnham.	Edward May.
Edward J. Burt.	George Morgan.
William Crawford.	Edward A. O'Keeffe.
L. A. Davies.	George Robertson, jun.
Arthur Albert Day.	Edward Yeld.
Herbert Cheney Hart.	

Students:

William Henry Shepherd. | F. W. Le Tall.

The meeting then adjourned.

THE LIBRARY.

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(Works marked thus (*) have been purchased. Of those not purchased or received in exchange, where the donors' names are not given, the works have been presented by the authors.)

IT IS PARTICULARLY DESIRABLE THAT MEMBERS SHOULD PRESENT COPIES OF THEIR WORKS TO THE LIBRARY AS SOON AS POSSIBLE AFTER PUBLICATION.

Abel [Sir F. A.] Accidents in Mines. 8vo. 196 pp. [*Proc. Inst. Civ. Eng.,* Vols. XC. and XCI.] *London,* **1888**

Anderson [William]. An Engineer's Education. Address delivered to the Junior Engineering Society, 7th October, 1887. 8vo. 17 pp. *London,* **1887**

Austrian Telephone Co. Drawings of all Apparatus, Material, &c., necessary for Telephone Exchange Installations, as used by the Telephone Company of Austria in Prag, Trieste, &c. Arranged by R. Howard Krause. [12 Photographs.] *Vienna,* **1888** [Presented by R. Howard Krause.]

Day [Major Francis J.], C.E. [*Vide* Royal Engineers' Institute.]

Electrician, The. The Electrician's Directory, with Handbook for 1888. 8vo. 254 pp. *London,* **1888**

Forbes [Prof. G.] [*Vide* Houston.]

Gee [W. W. Haldane]. [*Vide* Stewart and Gee.]

Houston [Prof. Edwin J.] Can the Original Reis Telephones transmit Intelligible Articulate Speech? 8vo. 11 pp. [*Journal of the Franklin Institute,* Jan., 1887.] *Philadelphia,* **1887**

—— On a Non-magnetisable Watch. 8vo. 3 pp. [Read before American Phil. Soc., Nov. 18, 1887.] *Philadelphia,* **1887**

—— Forbes's Electric Meters. 8vo. 15 pp. [*Journal of the Franklin Institute,* Dec., 1887.] *Philadelphia,* **1887**

Indian Government Telegraphs. Administration Report of the Indian Telegraph Department for 1886-87. Fo. 30 pp. *Calcutta,* **1887** [Presented by the Director-General of Telegraphs in India.]

—— History of Services of the Officers of the Superior Establishment, corrected up to 31st March, 1887. 8vo. 120 pp. *Calcutta,* **1887** [Presented by the Director-General of Telegraphs in India.]

Iron and Steel Institute. Journal of. No. 2, 1887. 8vo. 440 + lxvi. pp. *London,* **1887** [Exchange.]

Kew Observatory. Report of the Kew Committee for the Year ending October 31st, 1887; with Appendices containing Results of Magnetical, Meteorological, and Solar Observations made at the Observatory. 8vo. 26 pp. [*Proc. Roy. Soc.*] *London,* **1887**

Jacques [William W.]   An Empirical Rule for Constructing Telephone Circuits.   8vo.   13 pp.   [*Proc. American Acad. of Arts and Sciences,* New Series, Vol. XXIII.]   *Boston,* **1887**

Jacquez [Ernest].   Dictionnaire d'Électricité de Magnetisme, étymologique, historique, théorique, technique, avec la synonymie Française, Allemande, et Anglaise.   Nouvelle Edition.   8vo.   460 pp.   *Paris,* **1887**

Krause [R. Howard].   [*Vide* Austrian Telephone Company.]

L'Ingénieur Électricien.   Nos. 1 to 14 (all published).   Fo.   244 pp.
Paris, **1886**
[Presented by J. Aylmer, Member.]

Royal Engineers' Institute.   Occasional Papers.   Vol. XII., 1886.   Professional Papers of the Corps of Royal Engineers.   Edited by Major Francis J. Day, R.E.   8vo.   207 pp.   Plates.   *Chatham,* **1887**
[By Exchange.]

Stewart [Balfour] **and Gee** [W. W. Haldane].   Practical Physics for Schools and the Junior Students of Colleges.   Vol. I.—Electricity and Magnetism.   12mo.   221 pp.   *London,* **1888**
[Presented by Messrs. Macmillan & Co. (Publishers.)]

Tomlinson [Herbert].   The Influence of Stress and Strain on the Physical Properties of Matter.   Part I.—Elasticity (*continued*).   The Effect of Magnetisation on the Elasticity and the Internal Friction of Metals.   4to.   26 pp.   [*Phil. Trans.,* Vol. CLXXIX.]   *London,* **1888**

Wallich [G. C.]   Notes on the Presence of Animal Life at Vast Depths in the Sea; with Observations on the Nature of the Sea Bed, as bearing on Submarine Telegraphy.   8vo.   38 pp.   *London,* 1860

Wallich [G. C.], M.D.   The North Atlantic Sea Bed; comprising a Diary of the Voyage on board H.M.S. "Bulldog" in 1860; and Observations on the Presence of Animal Life, and the Formation and Nature of Organic Deposits, at Great Depths in the Ocean.   4to.   155 pp.
London, **1862**

Weber [H. F.]   Die Leistungen der Electrischen Arbeitsübertragung zwischen Kriegstetten und Solothurn.   8vo.   44 pp.   [Separat-Abdruck aus Nr. 1 und 2, Bd. XI., *Schweiz. Bauzeitung.*]
Zurich, **1888**
[Presented by C. E. L. Brown.]

ABSTRACTS.

T. C. FITZPATRICK—ACTION OF THE SOLVENT IN ELECTRO-
LYTIC CONDUCTION.

The three solvents used were water, ethyl alcohol, and methyl alcohol; the resistances of the trough, containing 250 cb. cm., being respectively 15,000 to 16,000 ohms, 50,000 to 60,000 ohms, and 4,000 to 4,500 ohms. The salts experimented upon were calcium chloride and nitrate, lithium chloride and nitrate, magnesium chloride and nitrate, ferric chloride, and mercuric chloride. With the exception of the last named, all the salts soluble in ethyl alcohol are deliquescent. The behaviour of mercuric chloride is peculiar: its solutions conduct very little better than the solvent alone, whether the solvent be water or alcohol, and the solubility does not seem to affect the question of conductivity. In all cases the solutions were prepared by diluting the original solution made by dissolving a known quantity of the salt in 500 cb. cm. of the solvent. The method has been previously described in the Report of the British Association, 1886, p. 328; tables for each salt and solvent are given in the paper.

With water as the solvent it was found that the conductivity values for equivalent solutions of the different salts are all of the same order, except mercuric chloride. The chlorides are better conductors than the corresponding nitrates, though not to any great extent. The behaviour of magnesium chloride is anomalous, the conductivity values for its solution being almost exactly one-half those of the corresponding calcium chloride solutions. This does not hold good with the nitrate, though its conductivity is less than that of the calcium nitrate. It would appear, therefore, that a solution of magnesium chloride containing one equivalent has the same conductivity as one of calcium chloride containing half an equivalent. For the ferric chloride solutions the conductivity is too high for the more dilute solution, the conductivity not being proportional to the quantity of salt in solution. This is probably due to the decomposition of the ferric into ferrous chloride.

For none of the alcoholic solutions is the conductivity proportional to the amount of salt in solution, the conductivity not diminishing directly with the increase of the dilution. The conductivities also vary considerably for the different salts. The values for the lithium salts are of the same order of magnitude, as is the case with the values for the calcium and magnesium salts; but the values of the lithium salts are 10 to 20 times greater than those of all the other salts.

A comparison of the same salt in different solvents shows clearly how largely the value of the conductivity depends on the character of the solvent. The results of numerous experiments by the author, as well as by other observers, would tend to show that the action of the solvents is twofold. Firstly, in certain cases the solvent causes a decomposition of the salt, the amount of such decomposition depending on (a) the temperature, (b) the solvent, (c) the state of dilution. Secondly, there is the formation of molecular groups in the solution; there is definite experimental proof that hydrates exist in concentrated solution; and it follows that such molecular groups, or more complex, ones exist in dilute solution.

Sir W. THOMSON—APPLICATION OF THE DECI-AMPÈRE OR CENTI-AMPÈRE BALANCE TO THE DETERMINATION OF THE E.M.F. OF VOLTAIC CELLS.

(Phil. Mag., Vol. 24, No. 151, Dec., 1887, pp. 514, 515.) .

Kohlrausch, in his "Physical Measurements," page 223, gives a description of Poggendorff's method of two opposed circuits, one containing a galvanoscope and the other a tangent galvanometer. The present method is the same, the tangent galvanometer being replaced by a standard ampère balance.

A battery of a sufficient number of cells is joined in circuit through a reversing key with a rheostat, a deci-ampère balance, and a standard resistance The poles of the cell to be tested are connected in circuit with a key and a sensitive mirror-galvanometer to the two ends of the standard resistance in such a way that both the battery and the cell to be tested tend to send a current in the same direction through that resistance. Care should be taken that the circuit of the cell to be tested is well insulated, and that both it and the standard resistance are free from other E.M.F. The standard resistance must be of such a form that no sensible error is introduced through heating. For use with the deci-ampère balance a resistance of two ohms is suitable, and this may best be constructed by winding an insulated platinoid wire one millimètre in diameter on a brass tube capable of holding half a litre of water, the temperature of which, when it does not differ much from that of the air, will be the temperature of the coil. Such a resistance coil will carry a current of one ampère for an hour without changing its resistance more than one-tenth per cent.

The results of tests on four Clark standard cells were almost identical with one another, and gave 1·439 Rayleigh, or 1·442 legal, volts at 11° C.; allowing the temperature correction of − 0·077 per cent. per degree rise of temperature, this reduces to 1·4846 Rayleigh volts at 15° C., which differs by less than one-thirtieth per cent. from Rayleigh's value of 1·435 at 15° C.

Dr. J. HOPKINSON—SPECIFIC INDUCTIVE CAPACITY.

In a paper read before the Royal Society the author gave an account of his experiments undertaken to determine the specific inductive capacity of various oils and other liquids. The results obtained were as follows:—For seven samples of colza oil, some being tested without any treatment, and others being dried over anhydrous copper sulphate, the value of K was found to lie between 3·07 and 3·14; olive oil gave K = 3·15; arachide, 3·17; sesame, 3·17; castor oil, undried, 4·82, and dried, 4·84. The experiments with ether presented much difficulty, as it insulated very badly, but in the first few minutes the value of 4·93 was found; for bisulphide of carbon K = 2·67; and for amylene K = 2·05. The results of the experiments on the benzol series may be tabulated as below, K being specific inductive capacity and μ the refractive index:—

BODY.			K.	μ.	μ^2.
Benzol ...	...	...	2·38	1·5038	2·2614
Toluol ...	...	...	2·42	1·4990	2·2470
Xylol ...	...,	...	2·39	1·4913	2·2238
Cymol ...	...	...	2·25	1·4918	2·2254

A. BARBARAT—TENSIONS OF TELEGRAPH WIRES AT VARIOUS TEMPERATURES.

Starting from the equation to the catenary, and taking into account the increase of length due to increase of temperature, as well as the change in length proportional to the change of tension due to the elasticity of the wire, the author has calculated a table which shows the relation between the factor of safety, the tension for wires of 3, 4, and 5 mm. diameter, the sag, and the temperatures corresponding to stretches of 60, 70, 80, 90, and 100 mètres.

Suppose a wire 5 mm. in diameter, with a stretch of 80 mètres, the tension at zero is 156 kilogrammes; then for any other temperature, say 16° C., we find the coefficient for multiplying this tension is 0·83, the tension is 130 kilogrammes, and the sag is 0·96 mètre; for a stretch of 100 mètres the tension would be 136 kilogrammes, and the sag 1·44 mètre.

MASCART—THE WORK OF TELEPHONES AND ALTERNATE-CURRENT MACHINES.

Two telephones in circuit may be looked upon as an alternate-current machine in connection with an alternate-current motor, and the efficiency of the receiving telephone should follow all the laws applicable to a motor.

The induced current and the movement of the diaphragm of the transmitter will be simple periodic functions of the period of vibration. The equations established show that, if the E.M.F. of the receiver is equal to the E.M.F. of the transmitter, then the efficiency is unity—the phases are equal—but no work is done, as there is no current transmitted; in all which points the telephones agree with the laws for motors.

From a consideration of the vibrations of the elastic diaphragm it follows that the efficiency depends only on the receiver and on the resistance of the line, and is independent of the transmitter and of the time of vibration. Comparing the work done by the transmitter with that taken up by the receiver, it follows that generally the acoustic efficiency is almost independent of the time of vibration; so also is the mechanical efficiency.

The resistance opposed to the current includes, besides the dead resistance of the line, the effects of self-induction and the counter E.M.F. of the receiver.

If the transmitter and receiver are similar instruments, the mechanical efficiency is never more than one-half; and if the damping is very slight the electric and acoustic efficiencies are equal to unity, and the mechanical efficiency is equal to one-half.

The transmitter and the circuit being given, it is possible to calculate the form of receiver in order that it may absorb the maximum of electric work. The formulæ are applicable to the case of two alternate-current machines, one being used as a generator and the other as a motor.

J. AMSLER-LAFFON and C. E. L. BROWN—EXPERIMENTS ON TRANSMISSION OF POWER BY FOUR ŒRLIKON DYNAMOS.

The transmission was intended to take place over a distance of 7½ kilomètres, from Kriegstetten to Soleure. The two generators were in series, as well as the two motors; three conductors being used for the line, as in Edison's three-wire system of distribution. The two dynamos in series could give an E.M.F. of 2,400 volts, but the ordinary working E.M.F. was 2,000 volts. At the experiments a resistance equal to that of the line—10 ohms—was inserted between the generators and motors, making, with them, the total resistance in circuit 23·44 ohms.

The chief interest of the experiments lies in the method adopted for measuring the mechanical power transmitted.

. All four machines were supported on knife edges, in line with the respective spindles, so that on setting them to work the action of the armatures on the magnetic fields caused the frames to be deflected out of the perpendicular. This movement of the frames of the dynamos about the points of support was shown by means of pointers 1½ mètres long, moving over scales so divided that it was possible to read directly the weight in

kilogrammes which it would have been necessary to apply at the end of a lever 25 centimètres long in order to produce the same amount of rotation.

In the actual experiments it was found that the arrangement of the machines on supports about which they could rotate gave rise to an error due to the magnetic attraction of the electro-magnets for the iron bed-plate on which the suspension arrangement was mounted. A second error might be introduced by the dead pull of the belts on the machines. In the final table of results allowance is made for the errors introduced by these causes. The greatest power transmitted during the experiments was 45 horse-power, with a current of 15 ampères and an efficiency of 72·6 per cent.

PINOT—A COPPER ACCUMULATOR.

In this new type of accumulator, which is a development of a battery proposed by Faraday and described by De la Rive in 1856 in his " Traité " d'Electricité," vol. 2, p. 605, the negative plates are formed of tinned iron wire netting, and the positives consist of plates of pure porous copper, obtained by subjecting porous electrolytic copper to a pressure of $3\frac{1}{2}$ to 7 tons per square inch. The positives are wrapped in vegetable parchment, and separated from the negatives by india-rubber insulators; the whole is immersed in a solution of zincate of potash or soda in a tin box.

On charging this accumulator, the zinc is deposited on the negative plates, while the positive copper plates are either partially converted into a suboxide of copper or the oxygen is occluded. On discharging, the zinc is redissolved and the copper loses its oxygen.

NOTE.—On the conclusion of the reading of the paper Mr. de Lalande drew attention to the similarity between the accumulator described and the battery brought out by Mr. Chapron and himself.

E. GIMÉ — APPLICATION OF ELECTRICITY TO THE STUDY OF OSCILLATIONS, ESPECIALLY OF PITCHING AND ROLLING OF SHIPS.

A glass tube, bent into a circular form, is partly filled with mercury; it has cemented into its walls a number of metallic contacts, which are connected to successive points in a box of resistances. The tube being placed in such a position that the rolling of the vessel takes place about its centre, the mercury will always occupy the lowest position, but different portions of the tube will successively be filled with it; and therefore more or less of the contacts will be short-circuited. The resistance is in circuit with a battery and a solenoid. The current in the solenoid will vary with the resistance in circuit, which

varies with the motion of the circular tube, while the action of the solenoid on its core varies with the current. The movements of the core therefore vary with the movements of the circular glass tube; and a writing point fixed to the core will trace a curve on a rotating cylinder covered with paper, which is a reproduction of the rolling of the ship. The abscissæ will represent the duration of the roll in seconds, while the ordinates represent the angle of roll in degrees.

A. CORNU—SYNCHRONISATION OF STANDARD CLOCKS.

(Comptes Rendus, Vol. 105, *No.* 23, *Dec.* 5, 1887, *pp.* 1106–12.)

To the pendulum of the clock to be synchronised is fixed, either above or below the bob, an iron bar magnet, curved to coincide with the circumference of a circle described about the point of suspension as centre. Two coils are arranged, one on each side, so that the curved magnet can swing through their hollow centres. One coil is in circuit with a battery and the synchronising clock, and acts by attraction on the magnet. The other coil is in circuit with a resistance only, and the current induced in this coil by the movement of the magnet tends to damp the motion of the pendulum.

The current in the synchronising circuit need not be very powerful, since the coil acts tangentially at the arm of a long lever. Owing to the weak current used, the sparks due to the extra current on breaking the circuit are very small; and their injurious effect may be entirely eliminated by placing a condenser or a polarisable electrolytic resistance in a shunt circuit. The arrangement described is the outcome of the theoretical views put forward in a previous paper (*vide C. R.,* vol. 104, p. 1463 and 1656).

C. WOLF—COMPARISON OF VARIOUS METHODS OF SYN-CHRONISING CLOCKS.

(Comptes Rendus, Vol. 105, *No.* 24, *Dec.* 12, 1887, *pp.* 1155–59.)

The paper is a criticism of that by Mr. Cornu, an abstract of which appears above.

Foucault was the first, in 1847, to make use of an electro-magnetic action to synchronise clocks. In his plan the pendulum was fitted with a soft iron core, which was attracted at each oscillation by an electro-magnet placed at the side at the moment when the standard regulator sent a short current through them. This plan required some kind of damping arrangement; and in 1867, when the author introduced it, at the request of Le Verrier, for synchronising the clocks of the Paris Observatory, he made use of two springs, which also served as contacts for sending currents into other clocks. Mr. Liais used much the same arrangement, but with spiral springs to act as dampers.

Foucault's plan was modified in 1858 by Jones, who replaced the ordinary solid metallic bob of the pendulum by a coil of wire in which the current from the regulator clock passes at each complete second. On each side of the

clock case was fixed a short magnet which penetrated entirely into the coil at the moment of its greatest amplitude. Airy, in 1859, made a modification by fixing the magnets to the pendulum and the coils to the clock case, thus obviating the necessity of using the suspension of the pendulum as a conductor

Verité, in 1863, proposed the use of an electro-magnet placed in line with the pendulum at the moment when it reaches the extreme point of its oscillation, and a core of soft iron placed horizontally on the pendulum at right angles to the plane of oscillation.

The essential point of difference between the methods of Foucault and Cornu and those now mentioned above is that the two former require some damping arrangement, which is unnecessary with the latter because the action of the sychronising arrangement is to hold back the pendulum in a position of stable equilibrium when it has reached the extremity of its oscillation. The plans of Foucault and Cornu both require a damping arrangement because there is no position of stable equilibrium, and if the motion of the pendulum were not damped it would strike against the electro-magnet or coil, and would thus swing beyond the limits within which the escapement acts regularly. Hence the author is in favour of systems in which no damping of the motion is required. Moreover, if from any accident the synchronising current be interrupted, the damping continuing its action will end by stopping all the clocks.

Of the non-damping systems, the author prefers that in which a soft iron core and electro-magnets are used, rather than coils and permanent magnets ; since in the latter, supposing the synchronising current accidentally interrupted, induced currents would be set up by the motion of the permanent magnets, which might interfere with the going of the clock.

NEGREANO—SPECIFIC INDUCTIVE CAPACITY OF LIQUIDS.

The principal objects of the experiments were to determine the dielectric coefficients of a series of hydro-carbons having homologous chemical composition ; to determine the index of refraction of the body at the same temperature as its specific inductive capacity ; to compare the specific inductive capacity with the molecular weight and specific gravity.

The condenser used was a multiple one, having five plates—A, B, C, D, E (counting from the top downwards). A, C, and E were 16 centimètres in diameter ; B and D 12 centimètres ; B, C, D, and E were generally fixed 1 centimètre apart ; A could be moved vertically. All the five plates were very perfectly insulated, and were supported on ebonite rods sliding in hollow ebonite columns, slit at the top, in which they could be fixed at any required height by means of clamping nuts. The whole apparatus was enclosed in a glass case, on the top of which was a brass stand, levelled by three screws, and fitted with a rod graduated in millimètres, and a micrometer screw going through the ebonite cover to the plate A. The liquid to be examined was

placed in a shallow flat dish carried by the ebonite insulators of the plate B. The thickness of the layer of liquid was measured by means of a second micrometer screw passing through the ebonite cover; the result being correct to 1-50th of a millimètre.

In carrying out the experiments one terminal of the secondary of an induction coil was joined to the centre plate C, the other terminal to the top plate A and the bottom plate E. The plates B and D were connected respectively to the two pairs of quadrants of an electrometer, the needle of which was connected to plate C, and, therefore, to one terminal of the induction coil. Using the apparatus as a double air condenser, the electrometer needle was brought to zero by adjusting the distance of A from B by means of the micrometer screw. It was found that a movement of A through 0·025 millimètre produced a deflection of the spot of light over 5 millimètres on a scale 1 mètre distant. After the empty dish was introduced, the needle was again brought to zero by adjusting A; a third and final adjustment being made after the liquid had been introduced into the dish. The experiments were in some cases repeated with a telephone in place of the electrometer.

The mathematical theory of the instrument is given, as well as very complete tables of the values obtained. The series of hydro-carbons selected for experiment was the benzine or aromatic series, nine different compounds being investigated. From an examination of the tabulated results, it would seem that the difference between the square root of the specific inductive capacity and the refractive index for the D line only affects the second place of decimals; that the specific inductive capacity increases with the complexity of the compound; that the ratio of the square root of the dielectric coefficient diminished by unity to the specific gravity increases, though unequally, as the series of compounds is ascended; that the same law applies to the ratio of the dielectric coefficient diminished by unity to the specific gravity; and that the ratio of the dielectric coefficient diminished by unity to the same increased by two and multiplied by the specific gravity is a constant for the particular series.

G. P. GRIMALDI—INFLUENCE OF MAGNETISM ON THE THERMO-ELECTRIC PROPERTIES OF BISMUTH.

(*Journal de Physique, Vol. 6, Dec.,* 1887, *pp.* 569–71.)

A thermo-electric couple, consisting of two copper wires soldered to the ends of a cylindrical rod of commercial bismuth 5 cm. long and 1 cm. in diameter, was placed equatorially between the two poles of a powerful electro-magnet. The junctions were kept respectively at 0° C. and at the temperature of the room. The couple was connected to an astatic galvanometer, and its E.M.F. was opposed by a constant thermo-electric couple in the circuit, so that the current passing through the galvanometer was very small. The resistance of the bismuth rod was negligible in comparison with the rest of the circuit.

When the electro-magnet was excited, there was a permanent deflection of

the galvanometer needle, due to a considerable decrease in the current of the copper-bismuth couple. If E is the E.M.F. of the couple out of the field, and E^1 when in it, then, if

$$\frac{E - E^1}{E} = d,$$

d was taken as a measure of the phenomenon. d is positive for commercial bismuth, negative for the chemically pure metal. It varies for different couples—from 0·0064 to 0·061, for example; its value varies with changes in the direction of the current, position of couple with respect to magnet, &c. It decreases for an increase in the difference of temperature of the two junctions. It is almost nil for fields of force less than 1,000 C.G.S. units. From 1,000 to 2,000 C.G.S. units it increases rapidly; for more intense fields it increases almost in proportion to their strength. The greatest value for d was 0·1104, obtained with a couple of copper and commercial bismuth in the equatorial position in a magnetic field equal to 18,860 M. (where M. is the horizontal intensity of the earth's magnetism).

VASCHY—LONG-DISTANCE TELEPHONY.

As the author says at the end of his article, the formulæ with which it abounds are not very simple, and it will be impossible within the limits of an abstract to do more than glance at some of the conclusions drawn from the mathematical reasoning.

The three points raised are—

    1st. What is the greatest distance over which conversation can be carried on for a certain kind of line?

    2nd. What effect have submarine or subterranean sections on the air-line?

    3rd. What sized wire should be adopted?

Preece has shown that the greatest distance at which speech is audible is equal to the square root of the fraction whose numerator is a constant depending on the nature of the line, and whose denominator is the product of the capacity and resistance. This formula takes no account of two very important factors, viz., the insulation of the line and its self-induction. Both these factors are duly taken into account in the author's calculations. He expected that the self-induction would have the effect of reducing the current, and thereby of weakening the sounds transmitted. He was therefore very much surprised, on applying his formulæ to the case of a copper air-line some hundreds of kilomètres long, to find that the injurious effect of the electrostatic capacity, far from being increased by the self-induction, was in a great measure counteracted, and that the transmission was improved.

To show clearly the important part taken by self-induction, and how inexact is any rule for finding the limit of hearing from the product of the

capacity by the resistance, the author gives the following results to which the theory leads:—

The self-induction has the effect of considerably increasing the current on arrival above the value which it ought to have according to Sir W. Thomson's theory.

In those cases where Thomson's theory shows very unequal weakenings of sounds of different pitch, and consequently considerable alteration in the tone of a complex sound, the self-induction largely prevents this change, and alters completely the character of the transmission.

If the ratio of the self-induction to the resistance—which depends on the nature of the line, and not on its length—goes beyond a certain limit which seems to be reached for copper wires of 4·5 mm. diameter, the tone is not sensibly affected, even when the line is very long. Supposing the self-induction nil, it would be found, on the contrary, that the line deadens the sharp sounds and only transmits the low notes.

Taking as a special case a line of 1,200 to 1,500 kilomètres, consisting of two copper wires 4·5 mm. in diameter, fixed about 1½ mètres apart, it is found that the strength of the current on its arrival is greater than it would be on a local circuit of the same resistance, but having neither capacity nor self-induction.

A condenser or an electro-magnet inserted singly in a circuit will diminish the current; but if both are inserted in series in the same circuit they correct the reciprocal injurious effects—at least partially.

The greater the self-induction, the more, supposing the E.M.F. equal, the electric undulations will be reduced, but the more regular will be their propagation. If the self-induction is large—or, rather, if the ratio of self-induction divided by resistance to the time of vibration is large—the speed of propagation will be sensibly uniform, and the alteration of the undulations will be small in spite of the resistance. From this point of view, therefore, it is desirable to increase the self-induction or to diminish the resistance.

The effect of leakage on the line is to add to the injurious effect of the factor capacity multiplied by resistance another term, viz., the self-induction divided by the insulation resistance; and to introduce an entirely new term depending on the resistance of the line, the insulation, and difference of potential between two points of the line.

The current arriving at either station is so much the weaker, other things being equal, the nearer the leakage is to the centre of the line. An underground or submarine section on the line is more injurious when it is near the middle of the line than when its position is towards either of the two ends.

F. DROUIN — NEW METHOD OF READING REFLECTING INSTRUMENTS.

The usual mirror is replaced by a thin disc of glass. The scale being behind the instrument, the observer in front sees the scale directly through

the glass; while he sees reflected from the front surface of the glass the image of an object such as a black line on a white background, placed also in front of the instrument and to one side. When the glass disc is deflected through an angle a, the virtual image of the mark is displaced through a distance $d \cdot \tan. 2 a$ ($d =$ distance from glass to scale). The method can be used in a well-lighted room, and does away with all trouble of lamps.

F. VON HEFNER-ALTENECK—A NEW FORM OF DYNAMO BY SIEMENS AND HALSKE.

(Elektrotechnische Zeitschrift, Vol. 8, Pt. 4, April, 1887, pp. 154–59.)

In constructing dynamo machines it is desirable to avoid large or wide pole-pieces, which have the effect of deviating many of the lines of force and causing them to pass through the surrounding air rather than through the armature. Long connecting yokes and magnet limbs are also undesirable, as they increase the resistance of the magnetic circuit.

Bearing these two points in mind, Messrs. Siemens and Halske have lately designed a new form of dynamo. The "Ring" dynamo consists of an external revolving Paccinotti ring, with internal fixed electro-magnets—generally four—arranged in form of a cross. In this form, all the lines of force proceeding out from the poles of the internal magnets must be cut by the ring, which completely encloses these latter. Moreover, the leakage of lines of force from one pole to another will be small, as the poles are wide apart; while the yoke is common to all four electro-magnets, and is very short, being, in fact, the central portion of the cross. The design is not altogether new, as it has been employed in alternate-current machines; but its application to continuous-current dynamos is claimed as original. Amongst the advantages of this "Ring" dynamo may be mentioned the following:—There is very little iron on the armature, whilst the cores of the magnets are heavy, hence the back action of the armature on the magnetic field is small. The armature being altogether outside admits of repairs being readily effected when necessary, without taking the machine to pieces. Owing to the comparatively large size of the armature, the machine can be driven at a slow speed; hence the surface resistance between commutator and brushes is small, and the latter can be kept on the commutator with less pressure than usual. Large machines of this type are comparatively light. One with a ring 104 centimètres outside diameter and 64 inside weighs 24 cwt., and has an output of 25,000 watts at 480 revolutions. Owing to their less weight and slow speed these "Ring" dynamos can be readily coupled direct to the driving engine. In this case the magnets are fixed to the frame of the engine, the shaft of which is prolonged beyond the outer bearing of the engine to form the shaft of the dynamo, thus doing away with all forms of coupling. Four large "Ring" dynamos, each capable of giving an output of 75,000 watts at 150 revolutions, have just been completed in conjunction with their engines.

H. SESEMANN—CALORIMETRIC METERS FOR CURRENT AND E.M.F.

Two thermometers are fixed on a wooden stand of triangular section: one serves to measure the temperature of the air; the other is surrounded by a coil of German silver wire, and measures the rise of temperature due to the heating of the coil by the passage of the current. The second thermometer is provided with a vernier sliding vertically alongside it. The zero of the vernier is placed opposite the division on the second thermometer which corresponds to the temperature of the room as shown on the first one, and the additional length of the mercury column due to the heating of the bulb of the second thermometer by the coil can then be read off on the vernier. The instrument is calibrated by comparison with a standard ammeter or volt-meter.

W. KOHLRAUSCH—EXPERIMENTS WITH ACCUMULATORS.

The author gives some results of a large number of experiments made with Huber's accumulators. Each cell contained eight negative plates of lead and seven positive lead plates, with four per cent. of antimony; the cells were 12 × 16 × 24 centimètres, the plates being square, and the side measuring 15 centimètres. The total weight of a cell, including acid, was 12·5 kilogrammes; the normal current was 0·635 ampère per square decimètre of plate, or 20 ampères per cell; six cells were used in series. The chief point noticed is the change of E.M.F. as the cells were alternately charged and discharged. Before charging, the E.M.F. per cell was 1·95 volts; on commencing to charge, this rose at once to 2·1 volts, and then gradually during the first $2\frac{3}{4}$ hours to 2·2 volts; in the next $1\frac{1}{2}$ hours the E.M.F. increased to 2·4 volts. Gas began to be given off when the E.M.F. was 2·35 volts. On disconnecting the source of electricity the E.M.F. fell to 2·1 volts. On commencing the discharge the E.M.F. was 1·95 volts, which fell to 1·85 volts after $3\frac{1}{4}$ hours' discharge, and to 1·75 volts after a further $\frac{3}{4}$ hour; on disconnecting, the E.M.F. rose to 1·9 volts. The charge comprised 89·8 ampère-hours and 196·2 watt-hours. Hence the mean E.M.F. during the charging was 2·185 volts. The discharge gave 81·4 ampère-hours or 153·7 watt-hours, or a mean E.M.F. during the discharge of 1·89 volts. The efficiency in ampère-hours comes out as 90·7 per cent., or in total electrical work as 78·4 per cent. These figures agree very well with those found by von Waltenhofen with the accumulators of Farbaky and Schenek, viz., 91 per cent. and 78·5 per cent. respectively.

Dr. R. RÜHLMANN—BERNADOS' SYSTEM OF ELECTRIC WELDING AND SOLDERING.

Previous attempts have been unsuccessful, either because the arc produced between two carbon rods was used, or, if the metal itself was used as one

electrode, it was made the positive one, whence resulted burns and oxidation of the surface. Bernados uses the metal as one electrode, but it is the negative one, the reducing action of which prevents oxidation.

The current from a shunt-wound dynamo giving 120 ampères and 175 volts is used to charge a battery of 490 accumulator cells, arranged in 7 rows of 70 cells in series. The current and E.M.F. necessary for any particular piece of work can then be readily adjusted by taking one, two, three, or more rows of cells in parallel to obtain the proper current; while the requisite E.M.F. is obtained by adjusting the number of cells in series which are included between the two points from which the current is taken off. The size of the carbon must be varied to suit the current being used, which is different for different kinds of work; it is the choice of the right current and E.M.F. which makes the welding a success or not. The negative pole of the cells is connected directly to the work in hand—boiler plates, angle irons, tubes, or what not; the conductor from the positive terminal leads to a convenient handle for clamping the carbon, and provided with a shield for protecting the hand of the workman from the heat.

The current required is very frequently large, and consequently special accumulators had to be designed which were capable of standing very rapid discharges. The plates consist of strips of lead alternately straight and corrugated, fixed in lead frames; in this way a very large surface is obtained. The simplicity of the apparatus, consisting as it does merely of a carbon gripped in a convenient handle, permits of the work of welding being done in any position; this is an immense advantage, as it renders unnecessary the lifting and turning about of heavy weights. Special arrangements are made if the joint to be welded is in a vertical plane, or if it has to be made on the underside of a piece of work. The word "welding" has been used for want of a better, but in reality the two pieces of metal to be united are fused along the joint, and solidify again as soon as the arc is moved forward. In this way any two metals almost may be united, and the operation is not limited to the fusing together of iron and iron, or steel and steel; it can also be carried on under water.

ANON.—OBSERVATIONS ON STORMS OVER THE GERMAN TELE-
GRAPH SYSTEM IN 1886.

(*Elektrotechnische Zeitschrift, Vol. 8, Pt. 11, pp. 485-94.*)

From the numerous tables accompanying the review of the action of thunderstorms on the telegraph system of Germany for the year 1886, the few following facts are selected. In all, 2,291 observations were made on 110 days at 483 stations. The 2,291 observed storms came from the following directions: 677 from S.W., 506 from W., 262 from S., 217 from N.W., 213 from S.E., 149 from E., 116 from N.E., 80 from N., and 71 unassigned. In January, 5 observations were made on 1 day; in February, none; in March, 1 on 1 day; in April, 178 on 11 days; in May, 682 on 16 days; in June, 487 on 26 days; in July, 442 on 19 days; in August, 284 on 13 days; in September, 188 on

16 days; in October, 23 on 6 days; in November, none; in December, 1 on 1 day. The greatest number of storms observed in any one hour of the day was 318, between 3 o'clock and 4 o'clock in the afternoon; the next greatest number (287) occurring between 5 o'clock and 6 o'clock, and 282 between 4 o'clock and 5 o'clock. The fewest storms were noticed between midnight and 6 o'clock a.m., viz., 35 from midnight to 3 o'clock, and 24 from 3 o'clock to 6 o'clock. There were 2,728 cases of damage during 586 storms, of which 1,479 were to outdoor parts of the lines, and 1,249 to indoor instruments, &c. The following is a tabulated statement of the cases of damage during 1886:—

Damaged.	Number.	Total in Use.	Percentage.
Posts...	... 1,243	1,084,740	0·114
Insulators	... 219	3,447,781	0·006
Galvanoscopes	... 74	11,294	0·655
Telephones...	... 17	6,505	0·261
Morse instruments ...	... 22	9,620	0·229
Post lightning dischargers	... 4	5,876	0·068
Plate ,,	... 119	22,116	0 538
Bobbin ,,	... 982	7,434	13·210
Line wires fused	... 16	{ 223,268 kilomètres, or 1 per 13,954 kilomètres.	
Office wires fused	... 11	...	...

Of the 1,243 posts, 357 were entirely destroyed, and 886 partially; 176 insulators were smashed, and 43 torn out of the posts; 16 galvanoscopes had their coils fused, and 58 were demagnetised. Further, 2 Hughes instruments were damaged out of 221, and 17 relays.

The following table of comparisons with former years is interesting:—

	1882.	1883.	1884.	1885.	1886.
Number of observations ...	... 2,684	2,064	3,258	2,597	2,291
Number of destructive storms ...	506	495	629	608	586
Percentage of destructive storms...	18·85	23·98	19·31	23·41	25·58
Cases of damage	... 2,261	2,046	2,864	2,911	2,728

Dr. E. LIEBENTHAL—VON HEFNER ALTENECK'S AMYL ACETATE STANDARD OF LIGHT.

(*Elektrotechnische Zeitschrift, Vol. 8, Pt. 11, p. 505.*)

The author has investigated the effects of changing the dimensions of various parts of the lamp from the normal values as settled by the inventor. An important point in the constancy of the standard is the height of the flame; and the following table shows the results obtained by altering the height from the normal height of 40 millimètres:—

Height of Flame.						Illuminating Power.	
20 mm.	...	...	...	...	...	0·38 standard candles.	
25 ,,	...	...	...	...	...	0·55	,,
30 ,,	...	...	...	...	...	0·70	,,
35 ,,	...	...	...	...	...	0·85	,,
40 ,,	...	...	...	...	...	1·00	,,
45 ,,	...	...	...	...	...	1·12	,,
50 ,,	...	...	...	...	...	1·25	,,
60 ,,	...	...	...	...	...	1·50	,,

Increasing or decreasing the diameter of the wick-tube by 2 mm. at a time diminishes the light by 1 per cent.; so that the dimensions chosen by von Hefner Alteneck give the maximum of light. An alteration of the portion of the wick-tube which rises above the body of the lamp by 1 mm. makes a difference of 0·2 per cent. in the light.

In the old form of the amyl acetate lamp the height of the flame is gauged by a sight carried on the top of a small rod fitting into the body of the lamp. Dr. Liebenthal has introduced an improvement on this arrangement. The lamp is provided on one side with a vertical screen, the upper edge of which carries a small camera; the lens is turned towards the flame, and has such a focal length that the image of the tip of the flame is seen clearly on a disc of white alabaster glass which forms the back of the camera; by means of a scale on the glass the exact height of the tip of the flame can be read in millimètres, the middle division corresponding to 40 mm.

VON HEFNER ALTENECK—NEW HYDRAULIC POWER-DYNAMOMETER.

(Elektrotechnische Zeitschrift, Vol. 8, Pt. 12, pp. 514–17.)

A pulley is fixed on the shaft in the usual way; over this pulley fits loose a second pulley rim; the two are kept together by means of keys fitting in key-ways cut in the pulley, not parallel to the shaft, but at an angle of 45 degrees to it. The outer pulley rim is entirely closed in on the side away from the machine by an iron disc, in the middle of which is a shallow circular chamber; into this chamber or cylinder fits a piston or plunger attached to the middle of the inner pulley; the chamber is filled with a liquid, preferably glycerine, and is kept tight by a leather collar, as in a Bramah press. The direction of the key-ways is so arranged that when power is transmitted by a belt to the outer pulley it revolves over the under one, as a nut would on a screw; in so doing the piston is forced more or less into the cylinder, compressing the glycerine in the chamber, from which a tube leads to a manometer, allowing the amount of pressure to be read off.

From measurements of the actual machine the radius (R) is determined for which the pitch of the thread is 45°; this R, as well as the radius of the piston (r), is measured in mètres. n is the number of revolutions per minute, h the difference in level of the two surfaces in the manometer, s the specific gravity of the liquid used; then the horse-power

$$N = 0\text{·}594\,R\,r^2\,n\,(h + 0\text{·}00206\,n^2\,r^2\,s),$$

where the second term within the bracket is a correction for the centrifugal force of the liquid inside the rotating chamber.

LIST OF OTHER ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM,

Appearing in some of the principal Technical Journals during the months of
NOVEMBER and DECEMBER.

(*Philosophical Magazine, Vol. 24, No. 150, November, 1887.*)

R. EDLUND—The Theory of Unipolar Induction.

(*Vol. 24, No. 151, December, 1887.*)

O. HEAVISIDE—Resistance and Conductance Operators, and their Derivatives, Inductance and Permittance, especially in connection with Electric and Magnetic Energy.

PROCEEDINGS OF THE ROYAL SOCIETY.

(*Nature, 29th December, 1887.*)

C. R. ALDER WRIGHT and C. THOMPSON—Development of Feeble Currents by purely Physical Action, and on the Oxidation under Voltaic Influences of Metals not ordinarily regarded as Spontaneously Oxidisable.

(*Comptes Rendus, Vol. 105, No. 20, 14th November, 1887.*)

P. DUHEM—Theory of Magnetism. **P. JANET**—Transversal Magnetisation of Magnetic Conductors.

(*No. 23, 5th December, 1887.*)

P. DUHEM—Magnetisation by Induction.

(*Journal Télégraphique, Vol. 11, No. 11, November, 1887.*)

Dr. ROTHEN—Telephony (*continued*).

(*Vol. 11, No. 12, December, 1887.*)

Dr. ROTHEN — Telephony (*continued*). **W. H. PREECE** — Rapid Telegraphy.

(*Bulletin de la Société Internationale des Electriciens, Vol. 4, No. 42, November, 1887.*)

E. DUCRETET — Automatic Recorder of Signals transmitted by Optical Telegraphs. **P. PICARD** — Specific Conductivity and Resistance of Bodies. **LORD RAYLEIGH**—Magnetisation of Iron and Steel in Feeble Magnetic Fields.

(*Vol. 4, No. 43, December, 1887.*)

VERNES—Electric Lighting of Theatres. **LORD RAYLEIGH**—Magnetisation of Iron and Steel in Feeble Magnetic Fields.

(*La Lumière Electrique, Vol.* 26, *No.* 45, *5th November,* 1887.)

G. RICHARD — Sir W. Thomson's New Measuring Instruments. **P. CLEMENCEAU** — Electric Lighting of Railway Stations. **E. WÜN-SCHENDORFF**—Submarine Telegraphy (*continued*). **W. C. RECH-NIEWSKI**—Systems of Distribution by Transformers. **C. DECHARME** —Isoclinic Magnetic Curves.

(*Vol.* 26, *No.* 46, *12th November,* 1887.)

G. RICHARD—Use of Electric Brakes for Goods Trains. **L. PALMIERI**— Production of Electricity by Evaporation and Condensation. **E. WÜN-SCHENDORFF**—Submarine Telegraphy (*continued*). **C. REIGNIER** —Formula of Magnetisation. **A. PALAZ**—Photometry of Incandescence Lamps at the Antwerp Exhibition.

(*Vol.* 26, *No.* 47, *19th November,* 1887.)

B. MARINOVITCH — Latest Improvements in Automatic Batteries. **E. KETSCHE** — Single-stroke Trembling Bells. **E. WÜNSCHEN-DORFF** — Submarine Telegraphy (*continued*). **P. H. LEDEBOER**— Electrical Measurements for Practical Purposes. **B. MEYLAN**—Coupling Dynamos in Parallel.

(*Vol.* 26, *No.* 48, *26th November,* 1887.)

J. MOUTIER—Induction in Branch Circuits. **E. WÜNSCHENDORFF**— Submarine Telegraphy (*continued*). **W. C. RECHNIEWSKI**—Continuous-current Transformers. **C. REIGNIER** — Regulation of Continuous-current Dynamos. **E. DIEUDONNÉ** — Optical Recording Telegraph. **A. LEDUC**—Alteration in the Calorific Conductivity of Bismuth in a Magnetic Field.

(*Vol.* 26, *No.* 49, *3rd December,* 1887.)

E. WÜNSCHENDORFF—Submarine Telegraphy (*continued*). **J. MOUTIER** —Induction in Branch Circuits. **L. PALMIERI**—Laws and Origin of Atmospheric Electricity. **J. HOPKINSON**—Specific Inductive Capacity. **E. KETSCHE**—Edison's New Arrangement for Duplex and Diplex Telegraphy.

(*Vol.* 26, *No.* 50, *10th December,* 1887.)

C. E. GUILLAUME—De Freycinet's New System of Units. **G. RICHARD** —Belt- and Rope-driving. **E. WÜNSCHENDORFF**—Submarine Tele-graphy (*continued*). **J. MOUTIER**—Induction in Branch Circuits.

(*Vol.* 26, *No.* 51, *17th December,* 1887.)

B. MARINOVITCH—Cost of the Electric Light in Private Installations. **H. W. BROWNE**—Duplex Telegraphy with Ordinary Morse Instru-ments. **C. REIGNIER and P. BARY**—Magnetic Permeability. **E. WÜNSCHENDORFF**—Submarine Telegraphy (*continued*). **E. DIEU-DONNÉ**—Electric Tramways.

(*Vol.* 26, *No.* 52, 24*th December*, 1887.)

C. **REIGNIER**—Gramme Dynamo with Helix at Top.   **E. ZETSCHE**—Electro-optic Signals.   **W. C. RECHNIEWSKI**—Electric Tramways.   **E. WÜNSCHENDORFF** — Submarine Telegraphy (*continued*).   **E. DIEUDONNÉ**—Electric Lighting of the Palais de l'Industrie.

(*Vol.* 26, *No.* 53, 31*st December*, 1887.)

E. **DIEUDONNÉ**—Melograph and Melotrope.   **A. PALAZ**—Telegraphic Communication with Moving Trains.   **E. MEYLAN**—Cauderay's Electric Meters.   **E. WÜNSCHENDORFF**—Submarine Telegraphy (*continued*).

(*Annalen der Physik und Chemie, Vol.* 32, *Pt.* 3, *No.* 11, 1887.)

F. **TEGETMEIER** and **E. WARBURG**—A Special Kind of Polarisation in Crystals.   **G. WIEDEMANN**—Magnetic Researches.   **F. EXNER**—Note on Contact Theory.   **H. LORBERG**—Reply to Clausius respecting Dynamos.

(*Vol.* 32, *Pt.* 4, *No.* 12, 1887.)

G. **QUINCKE** — Abnormal Phenomena in Dielectric Fluids, especially in Colza Oil.   **S. ARRHENIUS** — Conductivity of Phosphorescent Air.   **R. KRÜGER**—Resistance of Thin Metal Plates.

(*Beiblätter, Vol.* 11, *Pt.* 11, 1887.)

H. **BÄCKSTRÖM**—Conductivity of Crystals.   **JAMES MOSER**—Analysis of the E.M.F. of a Cell into its several Potential Differences.   **G. GUGLIELMO**—Electric Leakage in Moist Air.

(*Elektrotechnische Zeitschrift, Vol.* 8, *No.* 11, *November*, 1887.)

C. **HEIM**—Use of Magnesium in Primary Batteries.   *Anon.*—Siemens & Halske's New Bridge for Very Small Resistances.   **Dr. PIRANI** — Behaviour of Iron under the Action of Weak Magnetising Forces.   *Anon.*—Edison's Phonoplex.   **J. SACK**—Electric Seismograph.   **C. BOHMEYER**—Alternate-current Electric Clocks.   *Anon.*—Uppenborn's Ammeters and Voltmeters.

(*Vol.* 8, *No.* 12, *December*, 1887.)

C. **HEIM**—Use of Magnesium in Primary Batteries.   **Dr. E. GERLAND**—Latest Improvements in Dynamos.   **Dr. A. v. WALTENHOFEN**—Reply to Dr. O. Frölich.   **Dr. O. FRÖLICH**—Second Reply.   **P. NIPKOW**—Continuous-current Transformers.   **Dr. A. TOBLER**—Old and New Methods of Testing Submarine Cables during Laying.   **O. CAUTER**—Duplex Telegraphy.   *Anon.*—Improvements in Cuttriss's Siphon Recorder.

JOURNAL

The One Hundred and Seventy-third Ordinary General Meeting
of the Society was held at the Institution of Civil Engineers,
25, Great George Street, Westminster, on Thursday evening,
February 9th, 1888—Mr. EDWARD GRAVES, President, in the
Chair.

The minutes of the previous meeting were read and
confirmed.

The names of new candidates were announced and ordered
to be suspended.

The following transfers were announced as having been
approved by the Council, viz.:—

From the class of Associates to that of Members—
Arthur C. Cockburn.

From the class of Students to that of Associates—
O. M. Andrews.   W. E. Hayne.
R. W. Hayne.   G. H. Hume.

Donations to the Library of the Society were announced as
having been received since the last meeting from John Aylmer,
Local Hon. Sec. for France ; the Director-General of Telegraphs

in India; C. E. L. Brown, Esq.; and Messrs. Macmillan & Co.; to whom the thanks of the meeting were unanimously voted.

The following paper was then read :—

ON ALTERNATE-CURRENT TRANSFORMERS, WITH SPECIAL REFERENCE TO THE BEST PROPORTION BETWEEN IRON AND COPPER.

By Gisbert Kapp, Member.

The principle on which all induction coils, and therefore all the different forms of alternate-current transformers, depend is that of a magnetic circuit interlacing with an electric circuit. In its simplest form this relation can be represented by two rings threaded through each other (Fig. 1), one representing the electric circuit (coloured red in the diagram to indicate the copper wire), and the other the magnetic circuit (coloured blue in the diagram to indicate the iron core). Modern transformers have all a closed core, so as to compel, as far as possible, all the lines of the magnetic circuit to pass through the electric circuit, which consists of a primary and secondary coil; but they differ from each other in the particular manner in which the circuits are arranged, with a view to attain the following objects :—Reduction of the length of the two circuits, reduction of the weight of materials employed, ventilation, facility of manufacture and repair, efficiency of insulation, and reduction of cost of manufacture. Omitting for the present those considerations which more nearly concern the practical manufacture, we may divide transformers broadly into two classes—one in which the copper coils are spread over the surface of the iron core, enveloping the latter more or less completely; and the other in which the core is spread over the surface of the copper coils, forming a shell over the winding. I propose to call the former "core transformers," and the latter "shell transformers." A familiar example of the first class is the armature of an ordinary Gramme dynamo, whilst the

particular type of transformer introduced by Mr. Zipernowski may serve to illustrate the second class.   They are represented in the wall diagrams (Figs. 2 and 3), the copper being coloured

Fig 2.
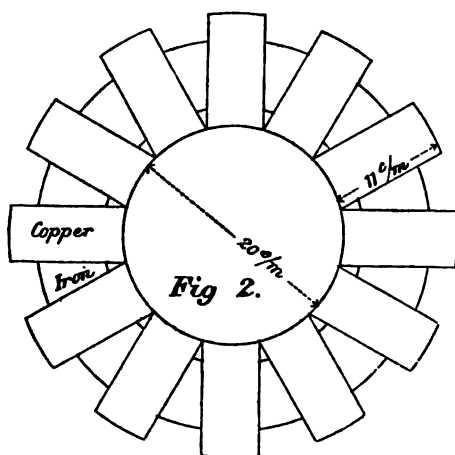
red and the iron blue.   It is evident without mathematical investigation that, whatever may be the proportion between the external and internal diameter of the ring, the electric circuit

Fig 3.
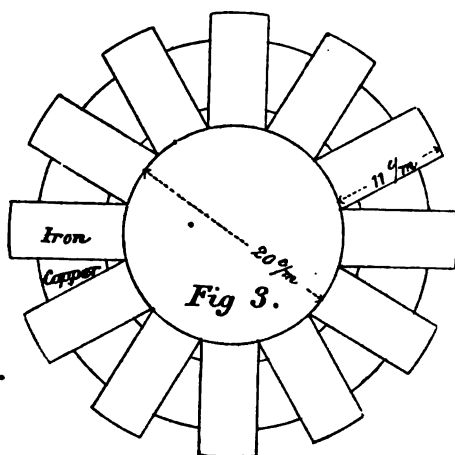
must be shorter than the magnetic circuit in the Gramme ring or core transformer, whilst the opposite must be the case in the Zipernowski ring or shell transformer.   When, a few years ago, this type was introduced, it was, on account of its short magnetic

circuit, generally believed to be an immense improvement upon
the Gramme ring; and I have heard it stated that with the
Zipernowski outside core absolute perfection has been reached,
since "every inch of copper wire contributes to produce electro-
"motive force."   I propose presently to lay before you a short
investigation into the relative merits of the Gramme and Ziper-
nowski rings, and also into the best proportion of copper to iron
in either; but before entering into this subject I would submit a
few theoretical considerations, so as to obtain a basis for this
investigation.

Since the electro-motive force developed in the secondary
circuit is proportional to the coefficient of mutual induction
between that and the primary circuit, and since self-induction
tends to produce lag and so reduce the output of the apparatus,
it is obviously advantageous, both for efficiency and output, to so
arrange the coils that their coefficient of mutual induction should
be a maximum for the given coefficients of self-induction in the
primary and secondary circuit.   According to a well-known law,
the maximum value which the coefficient of mutual induction
can have is equal to the square root of the product of the two
coefficients of self-induction; and the necessary and sufficient
condition for obtaining this maximum is that the same number
of magnetic lines of force should pass through both circuits.   In
other words, the whole of the flow of force should take place
within the core, and no free poles should be formed.   This con-
dition can easily be fulfilled by a suitable arrangement of the two
circuits in close proximity, and, as a matter of fact, is fulfilled in
all modern transformers.   We shall therefore assume that the
same flow (F) of magnetic lines of force passes through both
circuits, and that the electro-motive forces produced in the two
circuits bear to each other the same proportion as their respective
number of turns.   We shall further assume that the electro-motive
force impressed on the terminals of the primary coil, or the
current sent through it, is a simple sine function of the time—
in other words, that the electro-motive force developed in the
armature of the dynamo is such a function.   Whether the latter
condition is generally fulfilled by modern machines I am unable

to say ; but I believe that the presence of a transformer in the circuit has the tendency to smooth down any deviations from a true sine curve should they occur in the armature, and, further, that a curve of E.M.F., of whatever shape, produced by a machine will, after filtering through two or three transformers, come out as a true sine curve. It would be of value if someone having the necessary mathematical attainments would investigate this point ; but for our present purpose it is sufficient to assume that the electro-motive force produced by the machine follows with fair approximation a simple sine function, and that the deviations are unimportant. This assumption has up to the present been made. by all who have investigated the subject of transformers.

Let, in the following :—

n represent the number of complete cycles performed per second;

F „   „   flow of force in C.G.S. lines ;

B „   „   maximum induction (crest of wave);

τ „   „   number of turns in the coils, the indices 1 and 2 being used to distinguish the primary from the secondary coil ;

E „   „   maximum electro-motive forces (crest of wave) in volts, the indices 1 and 2 being used to distinguish the primary from the secondary coil ;

$e = \dfrac{E}{\sqrt{2}}$ „   „   average electro-motive forces ;

I „   „   maximum currents ;

$i = \dfrac{I}{\sqrt{2}}$ „   „   average currents ;

L „   „   length of the magnetic circuit in centimètres ;

a „   „   area of core available for the flow of lines in square centimètres ;

$R = \dfrac{L}{\mu a}$ „   „   magnetic resistance of the core ;

μ „   „   its permeability ;

r „   „   the electric resistances:

then we have the following well-known relations :—

$$B = \frac{F}{a} ; \quad F = \frac{4\,\pi\,\tau\,I\,10^{-1}}{R} ; \quad E = 2\,\pi\,n\,F\,\tau\,10^{-8} ; \quad \frac{E_1}{E_2} = \frac{\tau_1}{\tau_2}.$$

Calling e_{t1} and e_{t2} the average electro-motive forces at the primary and secondary terminals, and e_1 and e_2 the average electro-motive forces in the coils respectively,

$$e_{t1} = e_1 + r_1 \, i_1 \, ; \; e_{t2} = e_2 - r_2 \, i_2.$$

When a transformer is at work we have the following phenomena :—

1. A wave of impressed primary electro-motive force.
2. A wave of counter electro-motive force in the primary coil, but not exactly coinciding with the impressed.
3. A wave of primary current not coinciding with either.
4. A wave of magnetisation lagging behind the primary current wave by something less than a quarter period.
5. A wave of electro-motive force in the secondary coil lagging behind the magnetisation wave by a quarter period.
6. A wave of secondary current, coinciding with the former (5) in period if the external circuit contain no self-induction—a condition approximately fulfilled where the current is only used for lighting glow lamps.

The problem now is to find what relations exist between these different waves as regards their relative positions and magnitudes. At first sight it may seem that this would prove a very difficult problem; and if an algebraic solution be attempted it is, in fact, if not very difficult, at least very complicated; but it is easy to treat the matter geometrically in a very simple way. As this method has already been published some time ago, it will not be necessary to demonstrate it, and I shall limit myself to a brief description, pointing out a modification of the original method by which the energy dissipated in magnetising the core can be taken into account. It will be seen from the above equations that F and E are proportional. Consequently, if we know what E.M.F. is to be put on the primary terminals of a given transformer, we also know with very close approximation what the induction and total flow of lines will be. If it were not for the slight disturbing influence of the resistance of the primary coil, we would know these data exactly. But the loss of E.M.F. by resistance can, for obvious reasons, only be trifling,
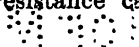
and will in most cases be settled beforehand. We can therefore determine F with perfect accuracy, and from the constructive data of the core also the exciting power (τ I) which will produce this flow. It will be shown presently that it is inexpedient and uneconomical to work transformers with a high induction, and we may therefore regard the permeability to be constant for all points in the cycle. In this case τ I and F must at all times be proportional.

Let, in Fig. 4, the radius of the circle represent the maximum effective exciting power which we assume to coincide with the magnetisation, so that the projection on the vertical of the radius O P as it revolves (clockwise) round O represents the effective exciting power in ampère-turns, and, to a different scale, also the flow of force at any instant. The line representing the current in the secondary must evidently lag behind O P by a quarter cycle, and can be calculated from the formula for E_2, and from the resistance of this circuit. This gives us O J_2, the maximum exciting power due to the secondary coil alone. By erecting a vertical on O P in P, and making P J_1 = O J_2, we find the line O J_1, which represents in position and magnitude the maximum exciting power due to the primary coil alone. This gives us also the primary current, and we can now determine the loss of electro-motive force due to the resistance of the primary coil. Let O R_1 represent this in direction and magnitude. The counter electro-motive force in the primary must evidently be in advance by a quarter period over the magnetisation; that is to say, it must be represented by a certain length on the line O Y. Its amount can be calculated from the formula for E_1. Let O E_t represent it to the same scale as was used for O R_1: then the parallelogram O R_1 E_{t1} E_1 gives us at once the point E_{t1} and line O E_{t1}, which represents in position and magnitude the
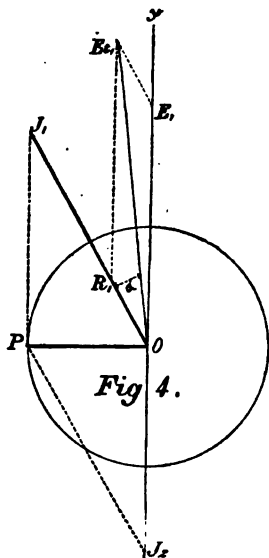
maximum electro-motive force impressed on the terminals of the primary coil, and a is the angle of lag of current behind impressed E.M.F.   The work done on the primary coil is found by the well-known formula

$$W_1 = \frac{I_1 \, E_{t1}}{2} \cos. \, a \, ;$$

$$W_1 = i_1 \, e_{t1} \cos. \, a.$$

That is to say, the true work is equal to the product of the apparent work, as measured by a dynamometer and voltmeter, and the cosine of the angle of lag.   If the apparatus were supplied with current at a constant and unidirected E.M.F. the apparent and the true work would be equal, and the ratio $\dfrac{1}{\cos. \, a}$ indicates how much larger must be the capacity of an alternate-current plant to do the same true work as a continuous-current plant.   I propose, therefore, to call cos. a the "plant efficiency" of the transformer.

It was stated above that transformers should be worked at a comparatively low induction.   This might seem at first sight a retrograde step, since with dynamo machines considerable gain in efficiency and output has resulted from adopting an induction up to 20,000 and more; but by reference to Fig. 4 one of the reasons why a low induction is preferable will be at once apparent.   Suppose that the line O P represents the limit to which the induction may be pushed without diminishing the permeability: then, if the same transformer be worked at double the electro-motive force, we would require an exciting power not only twice, but many times, as great as previously.   This would bring the point P considerably to the left without increasing the length of the line P J$_1$.   It would also increase the length of the line O J$_1$—that is, the primary current—and therefore the heat generated in the primary coil, whilst the angle of lag would become greater and the plant efficiency smaller.   Similar results would follow from an increase in the magnetic resistance of the core, or from the omission of a core altogether.   An early experimental type of transformer made by Messrs. Gaulard & Gibbs had a core made partly of iron wire and partly of wood, and therefore no

well-defined magnetic circuit; but, as the primary spirals alter-
nated with the secondary spirals, our previous assumption regard-
ing the coefficient of mutual induction remains valid, and the
above formulæ and graphic method could be applied to this
apparatus if we knew what value to assume for the resistance of
the magnetic field.   This, however, we do not know, and
I only mention this early apparatus because the experimental
investigations of Dr. Hopkinson and Professor Ferraris* have
proved that there is a considerable difference between the
apparent and the true work; that is to say, the point P must
have been a considerable distance to the left, making the plant
efficiency small.   In modern transformers the point P lies so
close to O that the plant efficiency may reach as high as 99 per
cent., so that the difference between real and apparent work
supplied to the apparatus when giving full output becomes
trifling; and for a practical determination of efficiency the readings
of measuring instruments on the primary and secondary circuit
may be used, with only such corrections as may be applied from
the diagram.

A high plant efficiency is, however, not the only reason why
transformers should be worked at a low induction.   A far more
important reason is the heating which takes place in the iron of
the core if this be carried rapidly through cycles of intense
magnetisation.   This heating is not due to eddy currents
(although these currents in a badly designed core would produce
the same effect), but seems to be the result of what may be
termed dissipation of energy by molecular friction or hysteresis.
The energy dissipated per cycle per cubic centimètre increases in
more than simple ratio with the induction, and it seems also to
increase as the periodic time decreases.   As yet very few
experiments have been made to determine the hysteresis for
different samples of iron, the most important data published
being those given by Professor Ewing and Dr. Hopkinson in the
Philosophical Transactions, part ii., 1885.   Professor Ewing

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* "Ricerche Theoriche e Sperimentali sul Ge :eratore Secondario Gaulard
"e Gibbs," by Galileo Ferraris. Turin, 1885.

distinguishes between static hysteresis (slow period) and viscous hysteresis (quick period), but the experimental data refer only to the former.   According to Dr. Hopkinson, the energy required to carry one cubic centimètre of annealed wrought iron through a complete cycle of induction equal to 18,251 lines is 13,356 ergs, and for hardened tungsten steel with an induction of 14,480 lines the energy reaches 216,864 ergs. Professor Ewing found that with very soft annealed wrought iron and B $=$ 13,190 the energy dissipated by hysteresis was 9,300 ergs.   He also tested annealed iron wire by carrying it through lower cycles ; and, as the results are of very great importance in the construction of transformers, they are given herewith :—

Induction.		Energy.
1,974	...	410
3,830	...	1,160
5,950	...	2,190
7,180	...	2,940
8,790	...	3,990
10,590	...	5,560
11,480	...	6,160
11,960	...	6,590
13,700	...	8,690
15,560	...	10,040

These figures refer to static hysteresis. As regards viscous hysteresis, no experiments have been made ; but in the case of B $=$ 8,500 and n $=$ 80 Professor Ewing estimates the energy dissipated per cycle at 5,000 ergs, or 32 per cent. above that of static hysteresis.   When applying the figures of the table to transformers it may therefore (until further experiments have been made) be advisable to add from 30 to 40 per cent. to the energy there given.   Thus, if an induction of 18,000 be adopted, the energy dissipated would amount to 18,000 ergs per cubic centimètre per cycle ; and with 80 cycles per second this corresponds to ·144 watt per cubic centimètre of core. It would evidently be extremely difficult, if not altogether impossible, to provide cooling surface enough for this rapid generation of heat, and hence it is necessary to work at a lower induction, the exact

amount to be determined for each transformer by the cooling surface of the core and coils, and by the heat generated in the coils themselves. So far, then, theory points to the necessity of low induction. As regards practice, I believe the makers of transformers have already found out that it does not pay to press the iron magnetically too hard. My own experience was at least in this direction. In the first transformer which I designed jointly with Mr. W. H. Snell, and which is on the table, we worked at an induction of 20,000, and there were produced two very undesirable results. In the first place, the core heated to such a degree that continuous work was out of the question; and, in the second place, the apparatus emitted a most unmusical sound, and it was evident that on this account alone it was not fit for practical use. The sound may have been partly due to the employment of wood for the framework, but we believed that the high induction was principally the cause, and by dropping the E.M.F. so as to get an induction of 15,000 we found that these evils were lessened. We then constructed another transformer, in which the induction was reduced to 10,000. This transformer is also on the table. In it the sound was suppressed, but the temperature still rose by about 30° C. when continuously at work. We have therefore, in a later design, adopted a still lower induction. Although it is possible to reduce in this manner the amount of energy dissipated in the core, this waste cannot be entirely prevented, and must therefore be taken into account when determining by the aid of the diagram the working conditions of the apparatus. For this purpose it would, strictly speaking, be necessary to know, in addition to the total energy dissipated per complete cycle, also the rate of dissipation at each point of the cycle—a knowledge which we do not possess. It seems, however, reasonable to assume that the rate of dissipation of energy by hysteresis follows the same law as the rate of dissipation of energy due to eddy currents—that is to say, that it is proportional to the rate of change of induction. When the induction passes through zero the rate at which heat is generated would be a maximum, and this would gradually diminish to zero

as the induction approaches its positive or negative maximum.
On this assumption we can imagine eddy currents substituted
for hysteresis; and in an iron core perfectly devoid of the latter
property—that is, infinitely soft—the same amount of heating
could be produced by imperfect subdivision, or, better still, by
subdividing it perfectly, but wrapping round it a closed con-
ductor of such a resistance that the heat generated in this
conductor equals that dissipated by hysteresis in ordinary iron.
For the ordinary transformer consisting of a perfectly subdivided
core of iron not infinitely soft and a primary and secondary coil,
we would substitute an imaginary transformer consisting of a
perfectly subdivided core of iron free from hysteresis, a primary
coil, a secondary coil, and a third closed coil of suitable resist-
ance. The current in this third coil would coincide with that
in the secondary coil, and its exciting power would be added

Fig 5.
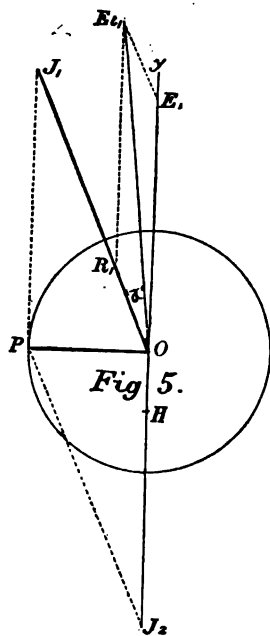
to that of the secondary coil. For
example, let the volume of iron affected
by hysteresis in a particular transformer
be 1,000 cubic centimètres, and let the
energy of hysteresis be 5,000 ergs: the
total energy dissipated would, with 80
cycles per second, amount to 40 watts.
If the electro-motive force produced in
the secondary coil be 100 volts, then
our imaginary third coil would have
either the same number of turns as the
secondary coil and a resistance of 250
ohms, or it might have half the number
of turns and a resistance of 62·5 ohms,
or any other combination giving the same
loss of energy. The mean value of the
fictitious exciting power would in all
cases be the same, namely, the number
of turns in the secondary coil multiplied
by ·4 ampère.   Let, in Fig. 5, O H represent the maximum
value of this exciting power due to hysteresis: then the total
exciting power tending to demagnetise the core, and which

must in a certain measure be balanced by the exciting power of the primary coil, is represented by the length O J_2—being the sum of O H and H J_2, where H J_2 equals O J_2 of Fig. 4 —and the point J_1 will be pushed higher up as compared with Fig. 4. The net result of this alteration is an increase in the amount of energy which must be supplied to the primary coil. It is thus possible to include the effect of hysteresis in the geometrical method of representing the working conditions of a transformer. I have dwelt at some length on this question because, notwithstanding its apparently somewhat abstruse character, it is really of very great practical importance. In some transformers the heat generated in the iron core is in excess of that generated in the copper coils, even at full output. Now, where transformers are placed in parallel between high-potential mains, it is, for obvious reasons of safety, not advisable to allow customers to handle a switch on the primary circuit, and the current must therefore be on, whether lamps are being lighted from the secondary or not. When the supply is continuous, as it obviously must be in a general system, the cyclic changes of magnetism in the core of every transformer will be kept up day and night; and the question of heating becomes of far greater importance than is the case in dynamo machines, which, as a rule, are only worked for a certain number of hours per day.

On the basis of the foregoing we can now investigate the relative merits of the Gramme and Zipernowski rings, and the best proportion of copper to iron in each. I have selected the two original types and one modification of each as being fair representatives of the two large classes into which in the beginning of this paper I have divided all transformers. These "rings" are both equally difficult to manufacture, and contain serious practical defects; but from a purely electrical point of view they are probably as good as the majority of transformers now in the market. Their practical defects constitute for my purpose a positive advantage, because nobody will care to claim either as his special type, and I shall thus escape the somewhat invidious task of having to compare the actual transformers made by rival manufacturers. As it is obviously impossible to conduct an

investigation of this kind on absolutely general lines, it was neces-
sary to assume rings of definite dimensions. I have selected rings
of circular section having an internal diameter of 20 cm. and an
external diameter of 42 cm., shown in full size in the wall diagrams
(Figs. 2 and 3). The mean primary potential is assumed to be
2,000 volts, and the secondary 100 volts. The copper coils in
Fig. 2 and the coils forming the iron shell in Fig. 3 are
supposed to touch each other on the inside, and, being of equal
depth all round, to separate on the outside, exposing part of
the inner ring. In the Zipernowski transformer as actually
made the inner diameter of the ring is rather smaller than
shown in Fig. 3, and the shell winding covers the whole surface
of the conductor, being of less depth on the outside than on
the inside ; but the magnetic resistance is in either case so low
that the difference between the assumed and actual arrangement
does not materially affect the result, whilst the calculations for
the former are somewhat less complicated. The total space
occupied by insulation I have assumed to be 1 cm., or 2·5 mm.
between the core of the Gramme ring and the primary coil, and
2·5 mm. between that and the secondary coil. The same space
has been allowed for the Zipernowski ring. We can now assume
different sectional diameters for the inner ring (core or coils), and
depth of winding for the covering (coils or shell), so as to make
up the 11 cm. sectional diameter of the whole ring ; and deter-
mine the output for each combination so that for continuous
work the apparatus should keep moderately cool. From analogy
with dynamos I estimate the total energy which may be wasted
without producing overheating at 260 watts ; and the output has
in all cases been calculated on this basis for an induction of 8,500,
a periodic time of $\frac{1}{80}$ second, and hysteresis at 5,000 ergs. As it
would serve no useful purpose to burden this paper with a repro-
duction of the somewhat lengthy calculations, I have plotted the
results in the curves II. (Figs. 6 and 7). The curves I. show the
output which might be obtained if hysteresis did not exist, in
which case the whole of the 260 watts would be transformed into
heat within the coils. The thickness of copper winding (Fig. 6),
and that of the iron shell (Fig. 7), is plotted on the horizontal ;

the output on the vertical.   From Fig. 6 it will be seen that the
maximum output with the Gramme ring is 6,400 watts with a
thickness of copper winding of about 1·7 cm., leaving 6·6 cm. for

Fig 6.
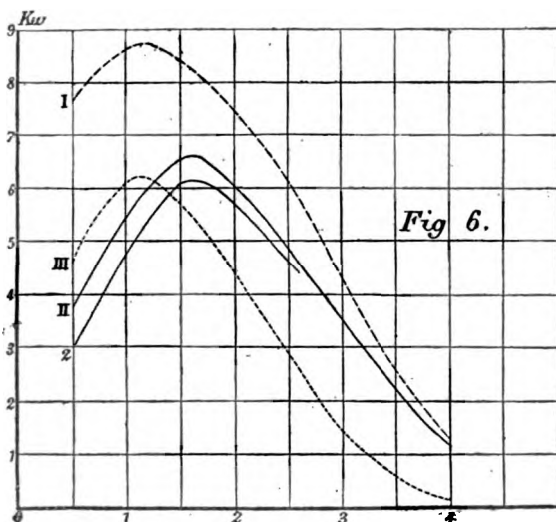
the sectional diameter of the core.   The cross-sectional area of
the magnetic circuit is in this case about 80 per cent. of the cross-
sectional area of the electric circuit, and the gross volume occu-
pied by iron is about equal to the gross volume occupied by
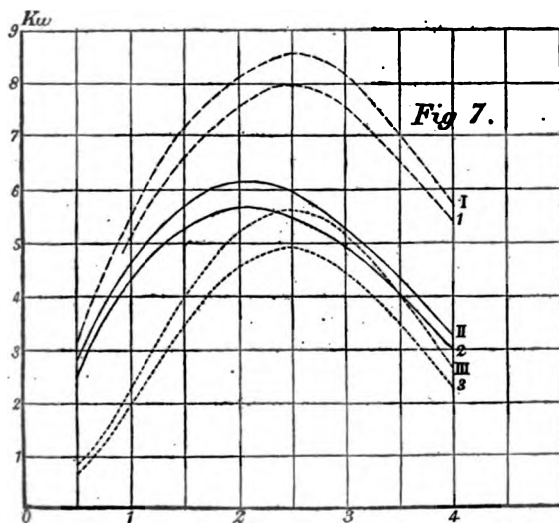
Fig 7.

copper.   The maximum output for the Zipernowski ring (Fig. 7) is 6,100 watts for an iron shell 2 cm. thick, leaving the sectional diameter of the coils 6 cm.   The cross-sectional area of the magnetic circuit is in this case nearly eight times that of the electric circuit, and the gross volume occupied by iron is $1\frac{1}{4}$ times that occupied by copper.   Thus, with the same external dimensions, the Zipernowski ring gives a slightly lower output than the Gramme ring; but it must be remembered that the weight of copper is also slightly smaller.   I have made the same calculations for oblong rings, as shown in Fig. 8.   Imagine the

Fig 8.
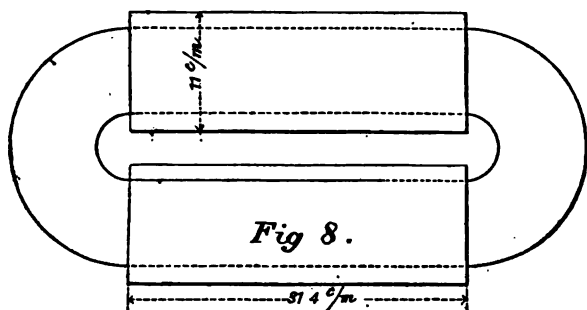
Gramme ring cut in halves, these straightened and laid side by side, and the two cores joined by semicircular pieces at the ends. The copper coils will in this process not be altered, but the length of the magnetic circuit and the volume of iron in the core will be increased; hence more energy will be absorbed in hysteresis, and the output will be reduced.   In Fig. 6 the curve 2 represents the output for the oblong Gramme ring.   By adopting the oblong shape for the Zipernowski ring we do not alter the mass of iron in the shell, but we lengthen the electric circuit, and increase the waste of energy in the coils.   In this case also the curve of output 2 (Fig. 7) must lie below that of the circular type.   Curve 1 (Fig. 7) shows the output of the oblong type if there were no hysteresis.   In Fig. 6 this must obviously coincide with curve I.   The maximum output of the oblong type is—For the Gramme ring, a little over 6,000 watts; and for the Zipernowski ring, 5,800 watts.

Up to the present we have only considered the output as limited by the amount of energy which may be wasted without

overheating. But there is another and equally important con-
sideration which affects the output, namely, the question of
self-regulation. It is evident that in a transformer fed at a
constant potential the induction is a maximum when the
secondary circuit is open; for in this case the counter electro-
motive force of the primary is very nearly equal to the
electro-motive force at the terminals. At full work the two
differ by the amount lost in resistance, and the induction is
reduced in the same proportion. The electro-motive force
generated in the secondary coil is proportional to the induc-
tion, and we must deduct from it the loss in resistance in order
to obtain the external electro-motive force. Thus, if 1 per cent.
be lost by resistance in the secondary, and 1 per cent. by
resistance in the primary, the difference of available electro-
motive force between open circuit and full output will be 2 per
cent. For good lighting this may be considered the permissible
limit of variation, and on this basis I have calculated the output
of the four transformers above described. Curve III. in Fig. 6
gives the result for the circular and oblong Gramme ring. It
will be seen that with a stout core the limit of output due to
heating is reached sooner than that imposed by the condition of
self-regulation, whilst for thin cores the reverse is the case.
The best proportion of iron to copper is indicated by the inter-
section of curves III. and II. for the circular, and III. and 2 for
the oblong transformer. In the latter case the core would have
to be 7·2 cm. in diameter, and the winding would have to be
1·4 cm. deep. The output is 5,900 watts. For the Zipernowski
ring curve III. gives the output as limited by self-regulation in
the circular, and curve 3 in the oblong type. The diagram shows
that the limit of output imposed by the condition of self-
regulation is in all cases reached sooner than that due to heating.
For the oblong transformer the maximum output is slightly
below 5,000 watts, and the best proportion of iron to copper is
given by the abscissæ of the highest point of curve 3. The
external shell would have to be 2·4 cm. thick, and the sectional
diameter of the coils 5·2 cm. From the above figures it appears
that in the circular and in the oblong type the Gramme ring

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dimensioned as shown in Figs. 2 and 8, gives more output than a Zipernowski ring of the same over all dimensions; the latter is therefore not superior to the Gramme ring, as commonly supposed, but somewhat inferior.

It might be, perhaps, objected that I have selected proportions different from those found in actual circular shell transformers, which have a much stouter ring of smaller mean diameter. More of the internal space is filled by the shell, and the central hole left is much smaller than shown in Fig. 3. The iron wire forming the shell is not put on in regular layers, but the turns cross each other in a more or less confused manner. There is no objection to such winding from an electrical point of view, as perfect insulation between the wires is of small importance. For the Gramme ring such winding would, on the other hand, be quite inadmissible; and it is on this account that I have selected a somewhat larger internal diameter of the rings, which gives room for regularly wound coils. But even if the rings are much stouter their proportion of output is not materially altered. If we imagine the ring of Fig. 3 contracted so as to reduce the central opening to 10 cm. and the extreme diameter to 32 cm., we obtain about the proportion of a Zipernowski transformer.

For convenience of calculation I have assumed a ring of these proportions to be changed into the oblong type, as shown in Fig. 8, but with this difference—that the external winding (coils or shell) on each side is only 15·7 cm. long instead of 31·4 cm.; the diameter being 11 cm., as before. Calculating the output in the same manner as before, I find that for the core transformer the maximum output as limited by heating is 3,230 watts, and is reached when the copper winding is 2 cm. deep; whilst the maximum output as limited by the condition of self-regulation (within 2 per cent.) is 3,020 watts, and corresponds to coils wound 1 cm. deep. The two curves cross each other as in Fig. 6, and the ordinate of the crossing point corresponds to a maximum possible output, as determined by the joint limit of heating and self-regulation, of 2,950 watts with coils wound 1·3 cm. deep. In the shell transformer the limit of output due to heating is 3,030 watts when the shell is 2 cm. thick, and the limit of output

due to self-regulation is 1,650 watts when the shell is 2·2 cm. thick. These figures show that even in stout rings, having proportions more nearly comparable with those of the Zipernowski transformer as actually constructed, the core type is better than the shell type.

This result refers, however, only to transformers having a core or a shell of circular section; and the question is whether by an alteration in the form of the shell the shell transformer could be improved. Any departure from the circular form of the shell must increase the length of the magnetic circuit, and must so far be detrimental; but if we can at the same time reduce the length of the copper coils, this disadvantage may be more than balanced by the reduction in the resistance of the circuits. As a matter of fact, the magnetic resistance of the shell is so low that even a considerable increase in the length of the magnetic circuit does not materially affect the difference of phase between the primary and secondary current. The two are almost diametrically opposed whatever may be the magnetic resistance. We can therefore adopt any shape of shell which will allow the length of the copper coils to be reduced. This is actually done in most modern transformers. The shell is rectangular, with the short side of the rectangle parallel to the plane of the coils, and thus the mean diameter of the coils is reduced. In addition to this, the circular form of coils has been abandoned in several of the more modern transformers, so that the shell may fill more or less completely the interior of the coils.

In core transformers as now generally made the coils are not arranged all over the core, but are disposed in two sets, one on each limb of a single core. Each set of coils contains a primary and secondary circuit wound upon each other, or one between the other, so as to obtain perfect symmetry. Were this not so, and were one limb wound with the primary and the other with the secondary, external poles would be formed at opposite points of the core, and the output would be reduced. In a similar manner the coils in shell transformers are wound upon or in between each other, but there is generally only one set of coils and a double core. The distinctive characteristics of the two types are therefore as follows :—

Core transformers—One core and two sets of coils.

Shell transformers—Two cores and one set of coils.

The following are some of the principal modifications of each type :—

Core transformers—Gaulard & Gibbs, Lowrie-Hall.

Shell transformers—Zipernowski, Ferranti, Mordey, Wright, Kennedy, Statter, Westinghouse, Snell and Kapp, Gaulard & Gibbs.

As most of these transformers are on the table, I need not describe them at length. The original Gaulard & Gibbs transformers had an open magnetic circuit, and cores which could be more or less inserted into the coils so as to regulate the E.M.F. of the secondary—a provision obviously necessary where the transformers are coupled in series. In the apparatus shown in 1883 at the exhibition in the Aquarium, there were four distinct induction coils; and in that employed for lighting on the Metropolitan Railway in 1884 there were sixteen distinct induction coils, the circuits being formed by a compound cable consisting of a central primary wire and six secondary wires grouped round it. In 1885 was introduced a type of transformer with closed magnetic circuit, in principle identical with that shown in Fig. 8, but the two limbs further apart. In the same year Messrs. Gaulard & Gibbs introduced small shell transformers in their Tivoli installation, each transformer feeding one 50-candle-power lamp. The two circuits are formed by a compound cable coiled into a solenoid, through which is passed a bundle of iron wires; the projecting ends are then bent over to close the magnetic circuit on the outside of the solenoid, and the whole is encased in a perforated metal cylinder with wooden ends. In their latest design of transformer the coils are circular in plan and rectangular in section, and are surrounded by groups of U-shaped soft iron stampings slipped over from both sides and held together by two circular cast-iron plates with a central bolt. The primary circuit is split up into two coils, with the secondary between them.

In the Lowrie-Hall transformer there are two sets of primary and secondary coils, laid horizontally one above the other. The core is formed by thin broad sheets of soft iron insulated from

each other by varnished calico, and the projecting ends of these plates are alternately bent up and down respectively so as to complete the magnetic circuit, the whole being clamped together in a horizontal cast-iron frame.   The Ferranti transformer is similar in the mechanical arrangement, but, belonging to the shell type, has only one set of coils of rectangular section.   The core is formed of thin iron strips of moderate width insulated from each other, doubled over at the ends, and clamped in a cast-iron frame.   Mr. Rankin Kennedy has devised various transformers of the shell type.   For the general supply of alternating currents he proposes to use a main current transformer at the generating station, in which currents from low-tension dynamos are to be converted into high-tension currents to be sent into the mains.   The shell of this transformer is built up of moderately wide but very thin strips, to form a rectangular frame of considerable depth, the strips being fastened by bolts at the corners. The core is composed of a series of strips of double width passing like a web through the middle of the opening of the rectangle, and thus subdividing it into two openings through which the winding passes.   In another type, which Mr. Kennedy calls the "Piled Form of Subdivided Transformer," the iron portion consists of H stampings, in which the central web forms the core and the two down strokes of the H the shell, the coils being wound over the web.   A number of these wound frames are piled upon each other, and side by side, the whole being clamped between cast-iron covers.   Diagramatically, this construction can be represented by Fig. 1 if, instead of employing only two links, we use a number of them formed into a chain consisting alternately of copper and iron circuits.   In another form of apparatus Mr. Kennedy uses a Siemens shuttle armature overwound with a shell of iron wire.   Mr. Wright's transformer may be described as a Zipernowski ring with coils of rectangular section and a shell of rectangular iron frames instead of the original iron winding. Each frame is cut across a corner, so that it may be placed over the core ; and as the edges are packed close on the inside, but radiate on the outside of the ring, the frames form gills for the dissipation of heat.   In Mr. Mordey's transformer the shell consists of thin

rectangular iron plates with a rectangular opening, the strip cut out being laid across the frame to form the core of the coils. The ratio in the length of the sides of the rectangle must evidently be as 4 : 6, and that of the strip cut as 2 : 4, in order to obtain a uniform section throughout the magnetic circuit. The apparatus is built up by alternately slipping a frame over and a strip through the coil. The core of the Westinghouse transformer consists of rectangular frames, each with a central web connected to the frame on one side only, so that it can be bent back to slip over the coils. The whole is mounted in a cast-iron weather-proof box for outdoor use. The shell in Mr. Statter's transformer consists of E-shaped stampings slipped over the coils alternately from either side. To obtain the same area of iron throughout, the width at the bottom of the stamping is twice that of the limbs. In the transformer designed by Mr. Snell and myself the shell consists of U stampings forming a double trough into which the coils are laid. The covers of these troughs are formed from the metal removed from the interior of the stampings. The whole is held together in a cast-iron frame so arranged as to allow air to circulate through the core and round the coils.

To render transformers perfectly safe it is necessary to avoid leakage between the two coils. For this purpose Mr. Kent has devised a very simple apparatus, consisting of an insulated sheet of metal wrapped round the inner of the two coils, but not forming a closed circuit in itself. This sheet of metal, which thus separates the two coils completely, is connected to earth. Now, if the insulation of the primary coil should fail, the leak, before reaching the secondary coil, must pass through the sheet of metal, and is thus conducted to earth, causing the primary cut-out to melt, and thus cutting the faulty transformer out of circuit. Another safety appliance has been devised by Captain Cardew. Its object is to disconnect the primary circuit from the mains if any part of the secondary circuit acquires a certain potential above that of the earth, which will take place if a leak occurs between the two circuits. The apparatus consists of a cast-iron box on the bottom of which is laid, into a shallow recess, a strip of aluminium foil ter-

minating at both ends in circular discs. About one-eighth of an inch clear above one of these discs is set a metal disc with screwed stem at the back passing through the glass cover of the box, and connected by a fine fuse wire with any point of the secondary circuit. The box itself is connected to earth. The fuse wire holds back a contact spring so arranged that on breaking of the wire it will fly down to its contact and short-circuit the primary terminals of the transformer, after which its own primary fuse will melt and cut off the supply of current. If the potential between earth and the insulated disc in the box should rise above a certain limit (which can be regulated by screwing the disc up or down) the static attraction between the disc and aluminium foil lifts the latter into contact, allowing a leakage current to pass through the fuse wire of the contact spring, and thus releasing the latter.

In the present paper I have only dealt with what may be called elementary principles in the construction of transformers. As, however, the application of transformers is a matter of far greater importance to our profession at large, I trust you will allow me to say a few words on this subject, not with the object of imparting information, but in the hope that a discussion of real practical value may be the result. Up to the present, distribution by transformers has been made either on the single-series or the single-parallel system; compound parallel, the three-wire system, or any other refinement of direct-supply methods, have, to my knowledge, never been attempted with transformers. The series system must fail for want of self-regulation where the lamps are to be connected in parallel and controlled independently of each other; but for very extensive and sparsely lighted districts, with the lamps supplied by each transformer in series on the Bernstein system, a series arrangement of transformers will admit of perfect self-regulation, provided the primary current be kept constant, and will, moreover, be cheaper than the parallel system. For general purposes, and especially for the dense lighting required in towns, the only practical method at present in use is to connect the primary terminals of the transformers all in parallel, and the lamps also

in parallel, across the secondary circuit of each transformer. In principle this arrangement is adopted in England, America, and on the Continent; but the methods differ in these countries. Here the distribution is made by a high-tension network of mains, and the transformer of each subscriber is directly connected with the mains. In America a double network of overhead mains is employed, one for the high-tension and the other for the low-tension currents. These mains are supported generally on the same poles, to which are also fixed the transformers, and the subscriber's connection is made with the low-tension mains wherever convenient. On the Continent a network of low-tension underground mains is used for distribution in the same way as if the supply were on the direct system, and this network is fed by alternating currents at certain points where fairly large transformers are installed. The primaries are connected with the station either by overhead wires or by a special kind of cable containing two circuits insulated from each other and arranged concentrically. The cable is protected by a double lead covering and by an iron sheath formed of spirally wound tape. Of the three methods here described our own appears to be the worst, the American slightly better, and the Continental the best. To string high-tension wires over and across our streets, allow high-tension branch leads to pass into our houses, and give every subscriber a little transformer to himself, more or less within the reach of the inmates, seems to me to be positively courting disaster. Such rough and ready methods may do as long as the light is not generally installed throughout a district; but once assume that every householder is using it (and it is that we are hoping for), the thousands of branch wires and transformers in the houses must constitute a very serious element of danger. In the American plan the high-tension wires are not brought into the houses, and in so far there is little danger to the inmates; but of course in the streets there is the same danger as here from the overhead wires.

Now the Continental plan may almost be called absolutely safe. With an arrangement like Mr. Kent's dividing sheet to prevent leakage, or Captain Cardew's ingenious apparatus for

detecting it and cutting off the supply, the secondary circuit can never acquire a potential sufficiently high to endanger life or property. This network can be composed of comparatively light cables, because we can feed it at frequent intervals and thus ensure constancy of pressure at all places and at all hours. The transformers would be fairly large—say from 500 to 1,000 lights each—and could be installed in rooms to which no person but the authorised attendant has access; and by placing the primary feeding mains also underground accidents would be rendered almost impossible. The method of using an underground secondary distributing network has also the advantage that, should at any future time storage batteries become sufficiently improved to render distribution by continuous currents possible, the whole of the cables would be available for this purpose without any change.

The PRESIDENT: It is usual that the discussion upon a paper shall commence as soon as its reading is terminated, but, for a reason which I will shortly explain, it is considered preferable by the Council that it should not be so on this occasion; and I will now, therefore, move that a hearty vote of thanks be accorded to Mr. Gisbert Kapp for his valuable paper.

The motion was carried unanimously.

The PRESIDENT: I now have to say that another paper, on "The Distribution of Electricity by means of Transformers," has been brought before us by Mr. J. Kenneth Mackenzie. This paper was prepared with the intention of being submitted to the Council some weeks ago, but owing to a serious accident, from which I am glad to see, by his presence here to-night, Mr. Mackenzie has recovered, he was prevented from sending it in sufficiently early to admit of notice of it being sent out at the same time as Mr. Kapp's paper was announced. Mr. Kapp, however, agrees with the Council that this paper should be read this evening, and I now call upon Mr. Mackenzie, therefore, to do so.

The following paper was then read :—

THE DISTRIBUTION OF ELECTRICITY BY MEANS OF SECONDARY GENERATORS OR TRANSFORMERS.

By J. Kenneth D. Mackenzie, Member.

One of the difficulties which arises in all transmissions of electrical energy is the conveyance of a current suitable at one and the same time for any and every class of receiver. Each receiver requires, in order to work under proper conditions, a current suitable to itself, which current must have the proper proportions of potential and quantity. Now, as it is necessary, when the factor of *distance* is brought in, to increase the potential of the current in order to overcome the resistance due to that distance, it follows that receivers cannot be used which are not capable of supporting the particular current necessary to work them at an increased distance. In the transmission of the current for glow lamps the distance is determined by the resistance of the lamps.

If it were a question what type of glow lamp is best, the reply would mainly be, that one whose resistance is the greatest—other considerations, of course, such as efficiency, being equal. Should it be necessary to convey current for lighting purposes over a radius of several miles, the resistance then incurred would no longer be in proportion to the E.M.F. necessary to be employed. Likewise, in the transport of power over a given distance, if each motor corresponded to a certain construction, it would be necessary to modify and vary each one, according to its position from the main source. Lastly, if it were a question of transmitting electric energy to work at the same time arc lamps, glow lamps, and motors, the current suitable for one type of receiver would not be adapted for the others; in other words, according to the nature of the receiver employed, so must be also that of the current.

It was to overcome these most serious difficulties which stood in the way of the practical development of electric lighting and transmission of power, that Messrs. Gaulard & Gibbs grasped the idea of interposing at the receiving point between the supply

wire and the receivers, a supplementary apparatus or generator, as a transformer of energy, which would deliver to the receiver with which it was connected a current proper to such receiver. The secondary generators thus in a manner prepare and modify the current transmitted from the source, and give it the necessary proportions of potential and quantity which the particular receivers supplied by them require, in order to work at the highest point of efficiency.

This idea in itself was not new when M. Gaulard commenced his experiments, Jablochkoff, Fuller, Sawyer-Man, Sir Charles Bright, and many others having previously striven to bring the matter to a practical issue ; but success did not crown their efforts, for the simple reason that the basis of all their experiments was the Ruhmkorff coil more or less modified; and it was only when M. Gaulard came to the conclusion that induction apparatus constructed upon the theory founded by Faraday, and exemplified by Ruhmkorff, was unsuited for the purpose, that a step was made in the right direction.

The principles underlying transformers may be clearly seen by reference to Figs. 1, 2, 5, and 6, where what may be called

TO GENERATOR
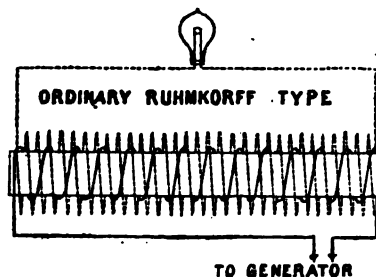
Fig. 1.

the "Ruhmkorff and Faraday" type are shown, and the Gaulard type in Figs. 3 and 4.

In the figures illustrating this paper " primary " or *inducing* current wires are shown in *black*, and " secondary " or *induced* current wires by *dotted lines*.

In the Ruhmkorff type the secondary is either superimposed upon the primary, as in Fig. 1, or it is wound upon a core distinctly separate from, though contiguous to, the primary, and is

alone acted upon by the magnetic changes set up in the core, as in Figs. 2, 5, and 6. It is for present purposes immaterial whether the core is polar or non-polar. In the Gaulard type

FARADAY'S
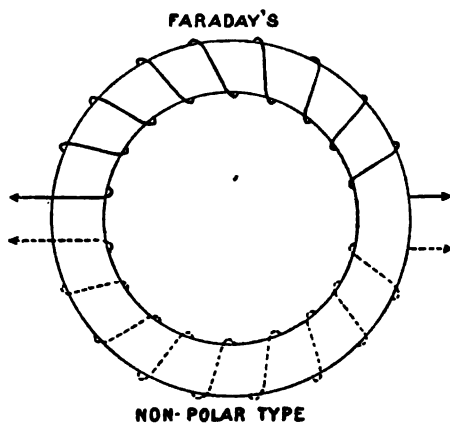
NON-POLAR TYPE
FIG. 2.

the secondary is placed in an identical position in regard to the iron core with the primary, and is thus acted upon both by the core and the primary or inducing circuit, as in Figs. 3

GAULARD'S POLAR TYPE
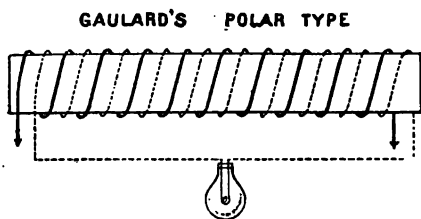
FIG. 3.

and 4. This is one of the two principal features wherein the Gaulard-Gibbs transformer differs from all previous ones, the other principal feature being that the metallic mass of the two circuits is equal; and in these two details of construction lies the secret of the success obtained by these gentlemen.

Other improvements in the construction of these instruments —such as making them non-polar, increasing the mass of iron to prevent loss through saturation, and other details—came afterwards, though, from the number of ingenious minds directed

towards the development of the system thus practically started,
such results were only to be looked for; but the following

GAULARD'S
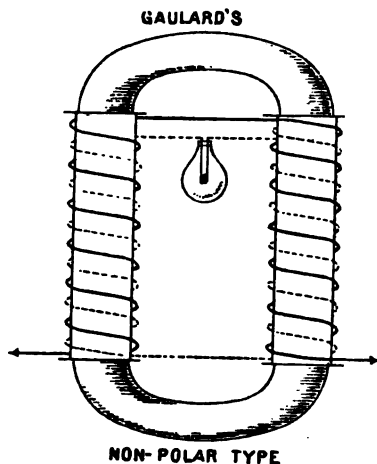
NON-POLAR TYPE
Fig. 4.

summary of what are now known as essential features in transformer
construction may undoubtedly be ascribed to M. Gaulard:—

1. The reduction to a minimum resistance of the primary
   and secondary circuits.

2. The obtaining of a maximum coefficient of induction in
   the two circuits with a minimum in the dimensions
   and weight of the apparatus.

3. The placing of the primary and secondary circuits in
   identical positions in respect to the core or magnetic
   field.

4. The proportioning of the two circuits, primary and
   secondary, so that their metallic mass should be
   equal.
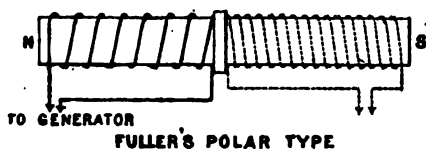
TO GENERATOR
FULLER'S POLAR TYPE
Fig. 5.

Given these essential features, necessary for the construction
of an efficient instrument of electrical conversion, it is easy to

see that the mere details of construction may be varied almost *ad infinitum;* and such, it appears, is now the case.

In the earlier type of secondary generator, such as was

FULLER'S
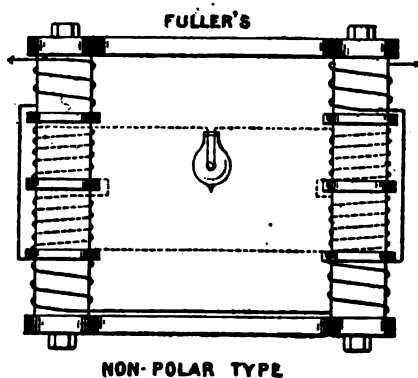
NON-POLAR TYPE.

FIG. 6.

employed in the lighting of several stations on the Metropolitan Railway during the early part of 1884, the transformers were made of a wire cable (Fig. 7) composed of a central core of

POLAR TYPE.
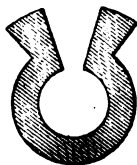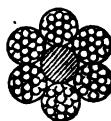
FIG. 7.   FIG. 8.

No. 8 B.W.G. copper wire, insulated, and surrounded by six cables laid thereon, each containing twelve insulated strands of No. 24 B.W.G. This central No. 8 wire constituted the primary circuit, the 72 No. 24 wires forming the secondary circuit. The cable thus formed was wound round a soft iron wire core, suitable terminals being provided for connecting the primary to line and the secondary to the lamp circuits. As may perhaps be remembered, five stations were lighted, viz., Notting Hill Gate, Edgware Road, Gower Street, King's Cross, and Aldgate, the generating plant being placed at Edgware Road Station. The primary conducting wire was about 15 miles long, and was formed of

seven strands of No. 16, the current in the primary line being about 11·3 ampères.

These generators were fitted with brass shields to the cores, by the lowering or raising of which more or less of the cable was exposed to the influence of the cores, and thus enabled lamps of various potentials to be employed from one apparatus. It was principally the desire to show how the potential of each column could be varied which induced M. Gaulard to make his transformers of a polar form, it being afterwards decided to make them non-polar only when the exigencies of actual and permanent employment of the system on a practical basis required such a change. The advantages accruing from this form were of course apparent from the first, and the matter was the subject of much discussion between M. Gaulard and myself at that time.

As is well known to electricians, the value of a current induced in a secondary circuit by one flowing in a primary depends principally upon the four following conditions of the primary :—

   1st. The strength of the primary current.
   2nd. The rapidity of the interruptions or alternations of the
            primary current.
   3rd. The number of spirals which compose the circuit
            traversed by the primary current.
   4th. The number of spirals which compose the secondary
            circuit.

The influence of the iron core upon the secondary current is for the present neglected.

In the first condition above given, the secondary current increases as the *square* of the ampère value of the primary ; and in the second it increases *directly* with the number of alternations or interruptions of the primary.

Conditions 3 and 4 affect the potential of the secondary circuit. It would hence appear advantageous to employ a large ampère primary current with a fewer number of alternations, since in the first case the generation of an induced current would be proportional to the *square* of the inducing current, and

in the second case only directly proportional; but bearing in
mind the formula

$$W = C^2 R,$$

C should not be greater than is consistent with practical efficiency.
On the other hand, should the factor C be kept low, an increase
of secondary energy would have to be obtained by increasing the
number of alternations, and this might bring about loss through
heating in the iron core round which the two circuits are formed
in the secondary generator, and therefore it is necessary to
establish a relative proportion between each of these factors.

The multiple-cable form of transformer first used by Gaulard
was discarded for one in which discs of copper of the form shown
in Fig. 8 were employed in some cases for both primary and
secondary circuits, and many of these are still in use. The great
advantage that resulted from this form of conductor was that
each turn of either circuit was directly upon the insulated core,
and that no superimposed layers involving loss through mutual
induction between the spirals were necessary.

This question of mutual induction in superimposed layers of
wire, one with another, is of importance, as it is easy to see that
since each wire through which an alternating current is flowing
serves as a medium for inducing a current of an *opposite*
direction in the one next to it, which consequently acts against
it and against the third or succeeding one, and so on throughout
the series, a loss is bound to result.

Transformers, like other electrical instruments, can be con-
nected in several ways, but the only two that need claim notice
are the series and parallel methods of connection. So much has
been said and written upon this subject of late, and such peculiar
statements made, that I deem it advisable here to make a few
remarks.

When Messrs. Gaulard & Gibbs first exhibited their system
at the Westminster Aquarium, in the beginning of 1883, only
one transformer was there shown at work, and this one was
consequently either in parallel or series with the dynamo. The
advantages that would have resulted were a series system of
connection possible being so apparent, M. Gaulard inclined more

to this method of working, and used his best endeavours towards arriving at a successful result.   He knew the impossibility of directly maintaining a constant potential in a secondary circuit whose resistance was being varied whilst the current in the primary was kept constant (an essential feature in series working), and therefore strove so to construct his apparatus that the action of the core upon the two conductors should vary with the alterations of resistance in the secondary.   This he did by altering the position or exposing more or less of the core, and some attempts were made to effect this automatically.

As has been before said, the value of the secondary circuit varies directly with the *square* of the current in the primary, and *directly* with the number of turns; other considerations, such as the magnetic influence of the core and the number of current alternations, being constant.

The E.M.F. of a secondary circuit, however, is dependent upon another important factor, namely, the resistance to which it is opposed; and the following table of experiments made by M. Uzel, and published by Professor Galileo Ferarris in his report to the Jurors of the Turin International Exhibition, 1885, shows the increase of secondary E.M.F. with increase of resistance.   It will be observed that the current in the secondary falls at the same time.

PRIMARY.		SECONDARY.		
A.	V.	A¹.	V¹.	R.
12·13	23·4	12·02	15·0	1·24
,,	31·4	12·0	24·0	2·0
,,	53·0	11·83	45·0	3·8
,,	70·0	11·73	65·0	5·5
,,	93·0	11·58	87·0	7·53
,,	107·0	11·3	102·0	9·0
,,	126·0	11·13	119·0	10·6
,,	145·0	10·95	133·0	12·6

where A = ampères in primary, V = potential at poles of generator primary, A^1 = ampères in secondary, V^1 = potential at poles of secondary, R = resistance in ohms of the outer part of the secondary circuit.

It will thus be seen that the resistance in the secondary forms an all-important factor when designing apparatus for producing a required result; and a long series of experiments has established a certain ratio to be observed in order to obtain the effect sought for.

Experimentally it was found that with a certain current in the primary circuit of a given number of spirals or turns a certain electro-motive force was obtained in the secondary with a given resistance, and upon these data the construction and design of apparatus employed as series transformers were based.

The work done by the primary current when traversing a number of generators is proportional to the resistance offered by these generators, or, in other words, by the counter E.M.F. opposed to the primary current by the secondaries. This necessarily is proportional to the number in use, and also proportional to the individual resistance of each secondary.

It can therefore be seen that the total energy of the primary current varies directly with the amount of work done by the secondary circuits of the generators, thus enabling complete economy in working to be obtained.

The economy resulting from this system of working in series would be substantial were it also possible to ensure constancy of potential at the same time between the poles of the secondary with a varying resistance in that circuit. The difficulty is that the E.M.F. increases in the same ratio as with the resistance; and this, consequently, points to the fact that transformers, when connected in series, must supply lamps also in series, or, in other words, the external secondary circuit must diminish in resistance as the lamps are turned out, instead of increasing, as is the case when the lamps are in parallel. More than two years ago I made some experiments upon this subject with Mr. Bernstein, the results being perfectly satisfactory so far as regulation went. Another way out of the difficulty, if series transformers and lamps in parallel are to be used, is by the employment of subsidiary resistances, or " choking coils," as they are called, which take the place of the lamps when turned off. Fig. 9 shows this in a theoretical diagram.

With series transformers and series lamps, all danger from fire risk is reduced to a minimum. No main fusible cut-out at the transformer secondary poles need be used; for, as has been before

SERIES TRANSFORMER

VOLTMETER   AMMETER

SHARP & KENT'S
CHOKING COILS
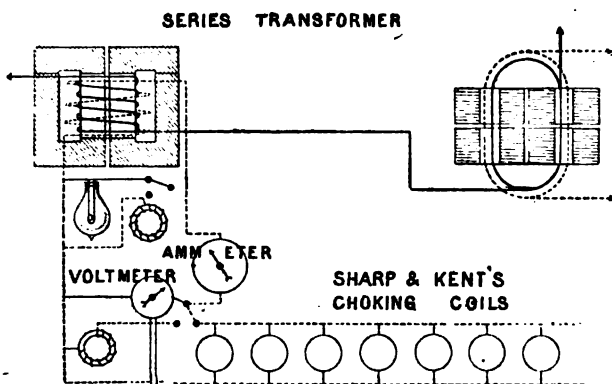
Fig. 9.

said, if a short-circuit occurs, the potential immediately drops, so that the flow of current is consequently that due to the resistance of the secondary helix, including the short-circuit. I have frequently run series transformers on short-circuit, and no heating has resulted, the potential at the terminals being imperceptible on a voltmeter, whilst the current indicated by an ammeter in the short-circuit was solely that due to the resistance of the ammeter and the secondary circuit of the transformer itself.

Another difficulty that lies in the way of working transformers in series is that experienced in maintaining a constant current on the line; and although many attempts have been made to do this automatically whilst employing alternating-current machines, no great success has as yet been attained.

Having briefly reviewed the system of working transformers in series, and shown the objections inherent to that method, I will now refer to the parallel system—upon which all present installations are based—treating the subject in as practical a manner as possible.

At the outset one important feature, inseparable from all parallel systems of transmission of energy, may be mentioned, especially since it shows how, by the use of transformers, difficulties may easily be surmounted.

This is that the fall of potential along a line varies in pro-
portion as the current is increased when changes are made in
the load.   The difference of potential between the source of
supply and the extremity of the line is small when the current
is small, but very considerable when a heavy current is used;
and consequently, if the lamps near to the source be kept at a
constant pressure, those far off will vary according to the inter-
mediate load.   In direct working this objection can only be met
by employing a conductor of such size as will render the
resistance of that conductor of no account, but then all con-
siderations of economy will have to be abandoned.

This difficulty has been experienced in all large direct-supply
works, since the fall of potential depends upon the ratio that
the amount of loss on the line itself bears to the maximum
amount of work carried by the line, this in many cases being
quite 20 per cent.   By employing a high potential, however, the
current to represent a given amount of energy may be so reduced
that this ratio may be made as low as 1 per cent., per mile;
and consequently transformers in parallel are capable of main-
taining practically the same potential in their secondary circuits,
irrespective of their position upon the line, or of the variation
of the load at intermediate points.   The following figures may
make this matter clear :—

Suppose a dynamo giving an output of 150,000 watts,
equal to, say, 1,500 ampères and 100 volts.   If the conductor be
proportioned so that one square inch of sectional area be used
for every 1,000 ampères, the size of the conductor necessary will
be 1·5 square inches.   This conductor having a resistance of
about ·028 ohms per mile (neglecting heat resistance), the loss
per mile on the line will be $1,500^2 \times$ ·028, or 63,000 watts—
equal to about 42 per cent. of the total output of the dynamo
per mile.

By the use of transformers, however, the same energy could
be transmitted over the same distance with a very different
percentage of loss.   The output being 150,000 watts, may be
represented by a current of 50 ampères and 3,000 volts.   The
resistance of a cable required to carry this current, calculated on

the same basis, would be about ·864 ohms per mile, and the loss, therefore, would be $50^2 \times ·864$, or 2,150 watts—equal to about 1·4 per cent. of the total load per mile.

These figures merely serve to show one of the enormous advantages that are reaped by the employment of transformers in the distribution of large amounts of electricity over great distances.

With regard, now, to the practical working out of a system of supply based upon parallel transformers, I will endeavour to show what has hitherto been done, the methods employed, and the results obtained.

The dynamo here claims our first attention, and, being the generating source of supply, forms, perhaps, the main feature in an electrical installation. In the first place, it must necessarily be of what is known as the alternating type. Up to very lately this class of machine had in this country received but a small amount of attention; now, however, since the transformer system has become known and appreciated, several makers have gone into the subject, and many marked improvements have resulted. One of the chief of these is the employment of iron in the armature, and such good results have been obtained by making the mass of iron very considerable in both armature and field, that this plan is now regularly adopted. One of the most important points in this class of machine is self-regulation, more especially with regard to small variations in potential; the changes of load occurring on a supply system, which produce extensive alterations in the external resistance of the circuit, and consequently large variations in potential, being easily met in other ways, such as by the introduction, either automatically or by hand, of resistances into the exciting circuit, or by varying the speed of the machine itself. As much as 2 volts per foot of wire may be obtained from the armature of a dynamo without any very considerable speed; and it is of importance to get this amount, if not more, when we remember that the magnetising effect of the current in the armature coils, as well as the amount of self-induction, is proportional to the number of turns in the coils. Now, since both are objectionable when amounting to much,

high E.M.F. and steadiness of potential may be obtained by greatly increasing the mass of iron, so that half saturation is never exceeded. Some very large alternating-current dynamos are now in use in this country, probably the largest being those by Ferranti, employed at the Grosvenor Gallery Central Station. Others, by the same maker also, are now in hand for the new supply works of the London Electric Supply Corporation, Limited, at Deptford, whose dimensions appear rather formidable. Their armatures are to be, I understand, about 42 feet in diameter, and will be driven direct from quadruple expansion compound surface-condensing engines of a marine type, indicating 5,000 H.P. each.

A type of alternating-current dynamo is shown in Figs. 10
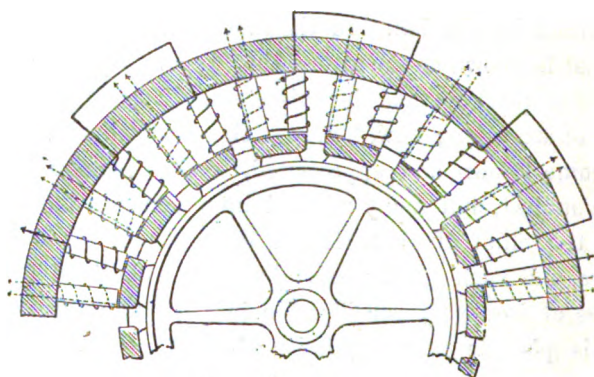
KINGDON'S DYNAMO
Fig. 10.

and 11, which, though founded on a system previously known and patented, has lately been altered so as to render it workable and efficient by Mr. Kingdon, who, early in 1886, submitted to me his ideas upon the subject. Those who had previously worked in the same direction had failed to make a serviceable machine from having overlooked the importance of laminating all the iron acted upon electrically, as well as other minor details requisite to make the idea practicable.

A series of bobbins, whose cores form parts of one whole continuous iron structure made of laminated soft iron plates, are divided into two divisions : one of these is rendered magnetic by separate excitation, and is shown in the figure by black lines ;

the other, as shown by dotted lines, is acted upon inductively by
the revolving laminated inductors, which may form part of the
fly-wheel of an engine.   It will be seen that if the black bobbins
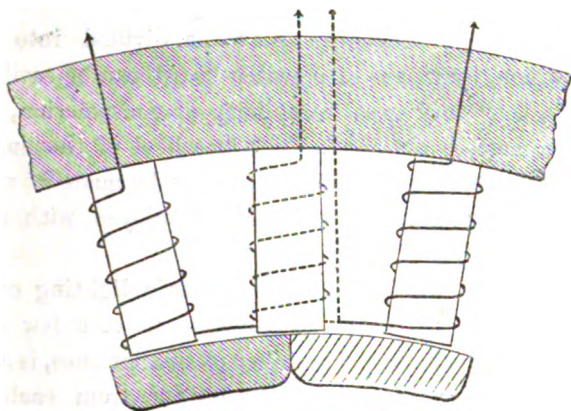
FIG. 11.

be coupled up and excited they will each have an alternate
polarity, and that if an iron armature or inductor be placed across
the pole of one of these bobbins and the pole of the next non-
excited bobbin, a current will be set up in the wire forming this
second bobbin, which current will tend to make its core of an
opposite polarity to the one next it, and which is joined with it by
the inductor.   As the revolving inductor alternately connects the
induced bobbin with its neighbours, each of which has an opposite
polarity, it follows necessarily that the current that will flow in
the induced bobbin will be alternating.

I would draw your attention here to one striking feature in
this machine, viz., that the same mass of iron serves both for the
field magnets and the secondary coils, these all being, so to speak,
prolongations or teeth of the iron mass.   A series of alternate
bobbins, excited and not excited, if magnetically connected in this
manner by revolving inductors, will yield a current from those
bobbins not separately excited, which current will depend for its
value upon the strength of the magnetic field due to the excited
bobbins, the size of the induced bobbins, the quantity and
dimension of the wire thereon, and the speed at which the
inductors revolve.

Experiments made with a dynamo of this nature have so far given very good results, some of the main advantages that may be claimed for such a type of generator being that there is no revolving or moving wire, no collectors and consequently loose contacts ; that the machine may be divided into separate circuits, which, by means of a switch-board, can be easily varied ; and, lastly, that very great simplicity of construction, and consequent low cost, is obtained. The fly-wheel of the engine may carry the inductors, and as this can easily be made of very great size, slow-running machinery may be employed, with its consequent advantages.

In designing supply works for electric lighting on a large scale, I am of opinion that the employment of a few very large dynamos, instead of a larger number of smaller ones, is a mistake, and that, generally speaking, the output from each dynamo should not exceed 10 per cent. of the total capacity of that particular station. In all running machinery, be as careful as one may, accidents will happen; hot bearings will, when least expected, occur; and with a heavy load on an engine running under such conditions serious damage may be caused, whilst, if a stoppage be made, a very heavy proportion of the total output will be affected. It is true that with a larger number of small engines the initial cost is greater, the depreciation greater, maintenance and attendance more costly; but, nevertheless, I am inclined to think that these disadvantages are more than counterbalanced by the greater security gained in subdividing the producing plant to a larger extent, especially when it is remembered how important it is in works of this nature that no breakdown in the supply should occur.

It may be well to remark here that it has been found quite practicable to couple alternating-current machines in parallel, so as to throw them in and out of circuit whilst current is on the line. Messrs. Zipernowski effected this some while ago, and I find that the Westinghouse Electric Company also work their machines thus. Practically speaking, all that is necessary is to synchronise the dynamos, and whilst their alternations are thus synchronous they can be coupled together or disconnected at will.

I cannot speak from much experience myself on this subject, having only experimented with some of Messrs. Siemens' machines. I found, however, that whilst it *was* possible to synchronise them and couple them in parallel, yet the slightest thing would throw them out of gear, if I may so speak, and destroy the balance. Even a small piece of waste falling on the belt was sufficient to do this.

With dynamos, however, having a large mass of iron in the armature, I believe it to be much more easy to couple them together, and this may be the reason why others have had more success than myself. The subject is an interesting one, and of great importance, I think, in large central stations employing alternating currents and transformers.

Until some practical means of electric storage, suitable for installations of this nature, be devised, the whole question of electrical distribution cannot be said to be completely solved. In gas works plant can be laid down which will continually produce gas, which can be stored and drawn upon during the few hours in which the demand is greatest; but with electric lighting, if carried out on a large scale upon this system, it is necessary to furnish plant capable of meeting the maximum demand, although such demand may only exist during one hour in the twenty-four. This necessitates the machinery standing idle during a part of the day, and consequently loss through unprofitable employment of capital; hence it is most essential that due care and forethought be exercised in arranging the number and sizes of engines and dynamos in any specific installation.

Two points now claim our attention, as they bear directly upon the question of the dynamo. Shall this yield a current of the potential required in the supply mains? in other words, shall it be a high-potential and small-current dynamo, or shall it be one which gives out a large current at a lower potential? In the latter case it will be necessary to pass this current through a transformer which will convert the electricity supplied into its required proportions, or, in other words, will raise the potential and diminish the current. In actual practice I do not believe

that this application at present exists. I had the advantage of discussing the matter thoroughly in 1884 with Dr. Orazio Lugo, of New York, when he was in this country watching the early experiments of Messrs. Gaulard & Gibbs on the Metropolitan Railway, and we made some experiments which led me to consider such an arrangement advisable in certain cases, and I have no doubt that before long it will be put into use in certain installations now in contemplation. The arrangement is shown in Fig. 16, where, however, two such main transformers are seen coupled together, so as to supply current upon a three-wire system.

I am inclined to believe that this employment of potential-raising transformers, even with their attendant loss in the transformation of the energy, offers advantages over direct working, since the cost of construction of low-potential dynamos is less than that of high ones, and greater reliability may be obtained, as well as more perfect control over the supply into the mains.

Upon this subject, however, makers of alternating-current machines may be able to enlighten us more fully, as the questions involved apply more directly to details of construction in the dynamos. These main transformers can of course be equally applied, whether the other transformers be connected in parallel or series upon the supply mains.

We will now turn our attention to the mains connected to the primaries of the transformers supplying them with current for transformation, as well as those delivering the current from the secondaries. I usually term the former "supply mains," and those which run from the secondaries into the lamp or other receiving system "distributing mains," and as such I will hereinafter continue to refer to them.

Figs. 12, 13, 14, and 15 show theoretically various methods of connecting transformers to the supply mains.

Fig. 12 shows one transformer in parallel arc to the supply mains supplying lamps in parallel.

Fig. 13 shows two transformers coupled together in parallel, and also in parallel with the supply mains, their secondaries being also joined in parallel to the lamp circuit.

Fig. 14 shows several transformers in parallel, their secondaries being also all united in parallel, thus forming a network system of distributing mains.

Fig. 12.
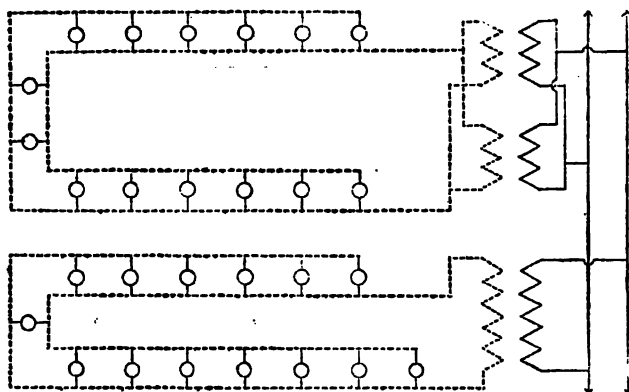
Fig. 13.

Fig. 15 shows a three-wire system of distribution, the dynamo having two circuits, and the transformers being placed in parallel across each of the two sets of supply mains.   These mains would
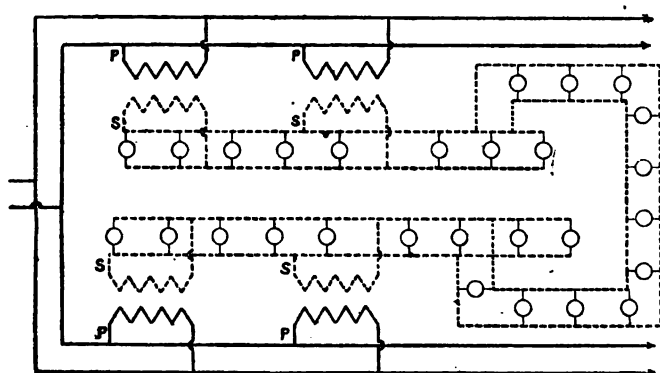
Fig. 14.

of course be calculated upon the same lines as if an ordinary direct-supply three-wire system were being erected.

I do not know whether this application exists in practice, but it would offer advantages in many ways in a large undertaking.

Since the supply mains have to carry currents of very high

potential, it is advisable that their insulation from one another, and from earth, be as high as possible, and consequently, where feasible, it is better to put them above ground. Such, however,
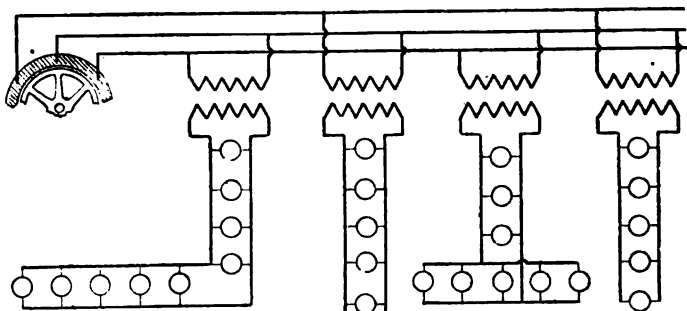
FIG. 15.

is not always possible, as local authorities in some places, notably in Paris, will not permit this, and the question of proper insulation then becomes a serious matter. Another question also
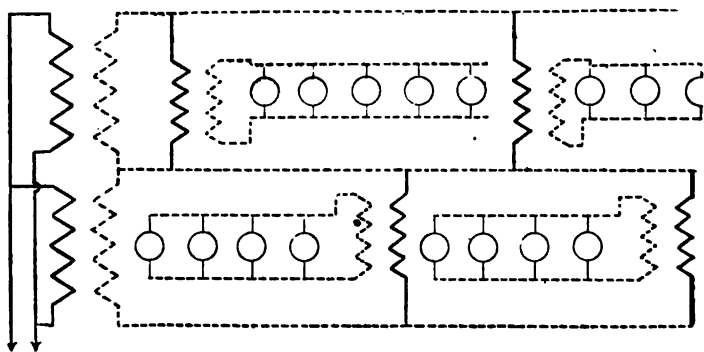
FIG. 16.

involved, and one which makes an important feature to be considered, is the tendency that alternating currents have to break down the dielectric of their conductors, especially when such conductors are in close proximity to the earth ; and retardation of the current under these conditions also comes into the question to an extent not occurring with aerial lines. I know of but one supply station working on this system with high-potential transformers in parallel where the supply mains are placed under ground, and this is at Tours, in France. M. Naze, the engineer who carried

out the work, however, tells me that he has experienced but little
difficulty from these sources, although the installation has been
at work now for over two years.   The mains, both supply and
distributing, are laid in earthenware troughs under the pavement
in the streets.   Fig. 17 shows in cross section an arrangement I
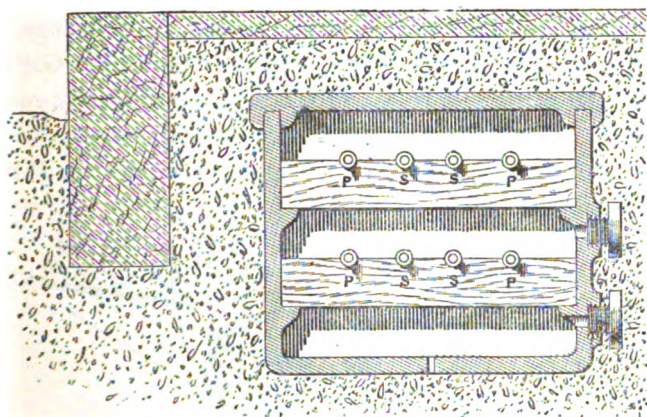
Fig. 17.

would propose, which resembles in many respects that adopted at
Tours, and which I have reason to believe would be found efficacious.
The troughs (rectangular in form) would be about 4 feet long
and 15 inches deep, having spigot and socket joints at the ends,
after the manner of ordinary socket pipes, so that complete
continuity could be maintained.   Transverse pieces of wood,
resting upon longitudinal projections inside and on the sides of
the troughs, would serve to support the mains, both supply and
distributing ; and a cover, recessed along each edge of its bottom
as shown, would make the conduit thus formed fairly water-
tight.   This cover would also have lapped ends.   Any leakage
that might occur into the trough would fall to the bottom, and
an arrangement for drain-off holes would be provided, so as to
keep the conduit clear from accumulated water.   The transverse
wood joists in themselves offer a fair amount of insulation, and
if paraffined would be but little hygroscopic.   The mains would
be led away, where required, into the houses through stuffing
glands in the sides of the troughs.   Repairs and alterations

to connections could easily be effected in a conduit of this description. The question involved is, in my opinion, of considerable importance, as sooner or later conductors for lighting purposes will have to go underground in most of our European cities.

Where the supply mains are erected overhead, they may be either supported on poles fixed to the roofs of buildings, as has been done in the supply system from the Grosvenor Gallery, or they may be carried on poles which are placed along the roadways or streets, as is usually the case in the United States.

Fig. 18 shows the top of the poles that are used by the
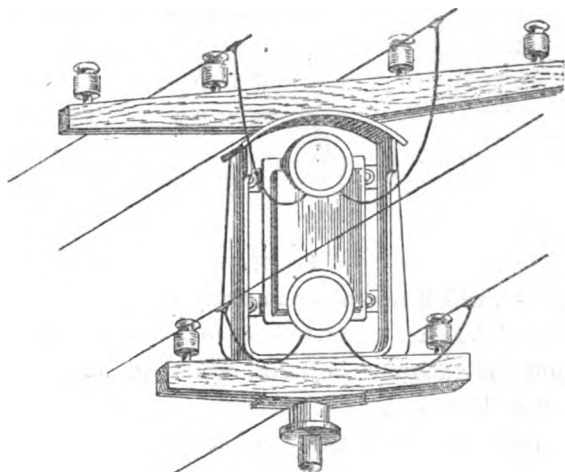
FIG. 18.

Westinghouse Electric Light Company, of Pittsburgh, and employed in most of their installations. It will be observed that the transformer itself is supported and carried by the pole in an arrangement devised by Mr. Stanley, of that company, which is very complete and effective.

Generally speaking, where the supply mains are somewhat heavy, it is, in my opinion, advisable to suspend them on bearers, having a factor of safety of at least ten times that of the strain to which they would normally be subjected. The mains would be suspended from these bearers by insulating rings encircling the main, and attached by wire to the bearer at regular distances of

about a yard.   The bearers should themselves be of stranded
copper wire in cities like this, where free sulphur is so prevalent
in the air; and this has lately been done in London, the old steel
bearers being replaced by the new copper wire ones.   Possibly
phosphor-bronze would be very suitable for purposes of this
nature, as it possesses great tensile strength.   Both bearers and
mains should be carried on insulators, which should have the
form of double shackles, as this enables branches to be taken off
without disturbing the line when up, and also affords greater
strength and solidity.   When the Grosvenor overhead circuit
was first put up for use with series transformers, Mr. Brougham
and I employed cast-iron boxes on the poles, into which the ends
of the supply mains were brought and sweated into insulated
brass sockets, as shown in Fig. 19 ; but although great care was
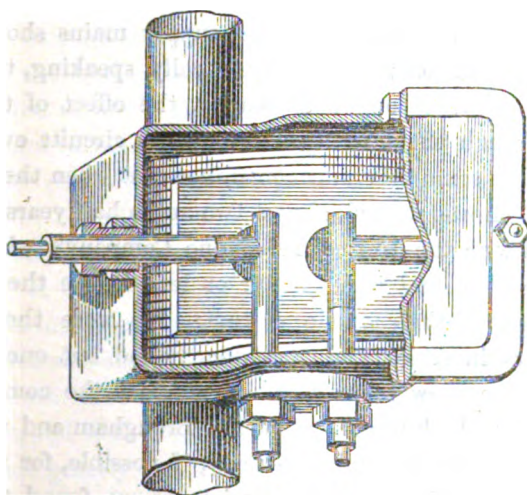
Fig. 19.

taken to close all the joints carefully, moisture to a considerable
amount collected inside, and they were ultimately abandoned.
In less humid climates, however, they might prove adaptable ;
and if the transformer cases shown in Fig. 18 can be kept water-
tight, certainly these also should be so.   The arrangement of
connection in these " junction boxes " explains itself.
   One point of interest in connection with the use of bearers

should be noted, which is that since the bearers run parallel throughout their length with the supply mains, they become inductively charged with electricity.   Now, if the ends of the bearers be left open, no current can flow through them; and similarly with the case of a series transformer with the secondary ends open: a counter electro-motive force is opposed to that existent in the adjacent main, which, to a greater or less degree, is retarded thereby.   Hence it appears advisable to "close the "bearer circuit," if such a term may be employed, and to close it by means of lightning arresters to earth at each end.   This is not a "metallic" closing of the circuit, but somehow it appears to produce the desired effect, and also at the same time affords a protection to the line.   I am speaking here of lines of several miles in length; of course on shorter circuits the effect is not considerable.

The distance between the two supply mains should be not less than 12 inches; and as, practically speaking, they are at a given moment of opposite polarity, the effect of the current flowing through them is, upon telephonic circuits even, almost nil.   Possibly some here may remember that when the Grosvenor supply station was started, almost two and a half years ago, havoc was played with the United Telephone Company's wires in that part of London, and to a greater or less degree the effect was felt throughout their whole system.   We were then working transformers in series, and consequently had but one wire; and the annoyance grew to such an extent that the company commissioned Dr. Hopkinson to meet Mr. Brougham and myself and study the matter, to find a remedy, if possible, for the trouble they suffered.   After several experiments, we found that it was only necessary to run a return wire, and the difficulty would cease, which was at once done.   I mention this fact as it may be of interest now that telephone exchanges exist in almost every large town, and the proprietors might raise difficulties in the way of electric light enterprises based on the employment of alternating currents, from the fear that their systems of telephonic communication might be seriously affected and prejudiced.

One peculiar feature connected with electric lighting systems

employing alternating currents I wish to bring to your notice, as it may be of interest as well as instructive. It relates to the use of lead-covered cables as conductors. In many ways these cables offer advantages as regards mechanical protection which are not possessed by others, but the mere fact of the existence of this metal covering brings into play difficulties which might not have been supposed likely to occur.

An installation has been erected in Aschersleben, in Germany, by the National Company, owners of the Gaulard-Gibbs patents, which has for a long time worked successfully. When it was first started, however, lead-covered wires were employed as supply and distributing mains; and though great care was exercised in laying them down, it was found that no insulation could be maintained, as they were always breaking down and becoming useless. The reason was due to this interesting fact—that the alternating current flowing through the cable set up an induced current in the lead covering, which at places along its length used to spark across to the supports—at first very slightly, but sooner or later to a considerable extent; the lead covering would then at these places become melted, and so exposing and injuring the dielectric, completely break down the insulation. The lead-covered cables have since been discarded, and ordinary conductors employed. It must be noted, however, that under any circumstances good insulation is difficult to maintain in this installation, for Aschersleben being the site of chemical and salt works, everything becomes more or less impregnated with that substance after a time, and so the chances of a breakdown through imperfect insulation are greatly enhanced.

In designing an extensive electric light installation in a town, one of the main points to be considered is where the transformers are to be placed. Generally speaking, two courses are open—either to place a transformer in each house, or to locate transformers at various places in the town with distributing mains from them along the streets, overhead or underground, to supply the houses as required.

In many cases the first method is preferable, where, as at the Grosvenor Gallery central station, a supply is given to but a

comparatively small number of the neighbouring houses, and customers can be, as it were, selected, and where the installation is in the first instance erected as an experiment. Under these and similar conditions, transformers placed in each house to be lighted is perhaps the best method of proceeding ; but in all installations based on a thoroughly practical design distributing stations must be employed as a means of obtaining a more perfect supply and control over the system. The transformers are thus placed out of the reach of all but the company's servants, and cannot be tampered with, whereby accidents are more easily avoidable. The position of these transformers depends, of course, upon the amount of lighting to be done in their neighbourhood, and I prefer to so locate them that the furthest house supplied is not more than 120 yards from the nearest transformer. This allows but little fall of potential in the distributing mains from each instrument, and, as Professor Forbes showed in his Cantor Lectures before the Society of Arts, is about the most economical distance at which lights can be worked.

The Westinghouse Electric Company, in their installations, couple all their secondaries or distributing mains together through-out the area lighted, thus equalising the potential over the district, and also in the case of a breakdown of one transformer preventing the extinction of the lights which were fed by that particular instrument. This system is practically a " network " system of distributing mains (see Fig. 14), and involves a large amourt of calculation in proportioning the sizes of the conductors, especially of those which join one group with another, if an installation to be thus carried out be properly and carefully gone into. I believe in practice, however, our American friends do not trouble themselves much with regard to this matter, but continue on with the cables used in the neighbouring districts.

Transformers, when thus placed to supply districts, may be located in cellars, in little street boxes under lock and key, or may be placed on the poles as before described.

I have here a plan of the mains, supply and distributing, worked out for the town of Port of Spain, Trinidad, which installation will be an interesting one, from the extent of the

area lighted. I have not adopted the network system in this instance at present, on account of the scattered position of the houses; but later on probably all the secondaries will be coupled up, when the lighting in the town becomes general.

All transformers should be protected by fuses or cut-outs, and one of these should be placed on each wire leading from the supply mains to the instrument. On account of the high potential employed, I prefer to use that kind which consists of a thin wire of a length not less than six inches, having a weight, such as a lump of plaster of Paris, in the middle, in order to break the circuit quickly when the fuse goes. It is astonishing the length of arc that can be maintained by a current of 2,400 volts when the circuit is broken, and therefore I think these fuses should be long and well weighted. The secondaries should also have fuses attached between the poles of the transformers and the distributing mains, though these may be of the ordinary type in use with low potentials.

A switch should be provided to open the circuit leading to the primary poles of the transformer; and all switches which thus control high-potential circuits should be made to snap open very quickly, and be so constructed as to make it impossible that they should remain in any other position than hard on or hard off.

With regard to the wiring of houses and buildings supplied with electricity by means of transformers, the rules in force in ordinary installations are of course applicable throughout; but I would remark that on no account should existing gas fittings be utilised in any building, as the chances of a breakdown are very considerably increased. At the Grosvenor Gallery supply station this was brought forcibly to my notice on more than one occasion, and I may mention one instance which occurred some two years ago which will show you what risk is run when gas fittings are so employed.

In a large shop in New Bond Street some of the gas fittings were utilised by the contractor for the attachment of glow lamps, and all went well till one evening a short-circuit occurred on one of the gasoliers, and the shop was left in darkness. They at once

sent over to the Gallery to know what to do; and shortly after, a servant came round from a consumer's house in Bruton Street to say that all the lights were out, and in a few moments other messengers came, informing us of further failures of the light elsewhere on the supply circuit.

We found that the current had gone to earth at the shop in Bond Street, destroying the insulation between the primary and secondary of the transformer, and thus putting the primary current to earth. A weak spot existing in the lamp wires at the other house immediately developed itself in the same manner, again breaking down the insulation and putting the primary current to earth at that place as well. The result was that all those consumers situated between the two breakdowns were left in darkness, as the current traversed the earth, or rather the gas pipes, between the two places where the breakdowns occurred, instead of going through the line. When the two faulty transformers had been switched out of circuit, the current again flowed through the line and the intervening transformers, thus restoring the supply.

I would remark that at that time the transformers were in series, but I can quite conceive a similar state of affairs occurring on a parallel supply system.

There appears no real necessity ever to use gas fittings; but if it be a question of expense, let them be entirely disconnected from the gas pipes, and then of course they may be used as any ordinary electric light fittings.

Every house, when supplied from distributing mains, should have a main switch to control the supply within the building, and where meters are employed this should be placed between the meter and the main. Preferably these should be "double-pole" switches. Fusible cut-outs ought also to be employed in the house mains, and these should be proportioned to suit the maximum current supplied to the house.

In the States, the Westinghouse Electric Company employ almost invariably glow lamps of 50 volts, as they find that these last longer and are stronger than the ordinary 100-volt lamps.

I have now tried to lay before you and explain the general

principles of electrical distribution by means of transformers, without entering into theory or into other scientific and theoretical details which the subject involves, since such matter requires independent discussion. My object has been to give you a general outline of the principles involved, and their practical application, as well as some of the experiences I have gained in the matter since I first took it up with Messrs. Gaulard & Gibbs in 1883.

Before concluding, however, I would like to draw your attention to the results of some experiments made by Mr. William Stanley, jun., of the Westinghouse Electric Company, and which were briefly described in the American journal the *Electrical Engineer* of September, 1887. I have had no experience myself of what he describes, and can only refer to him as my authority.

Figs. 20 and 21 will show the theory of his apparatus, which,
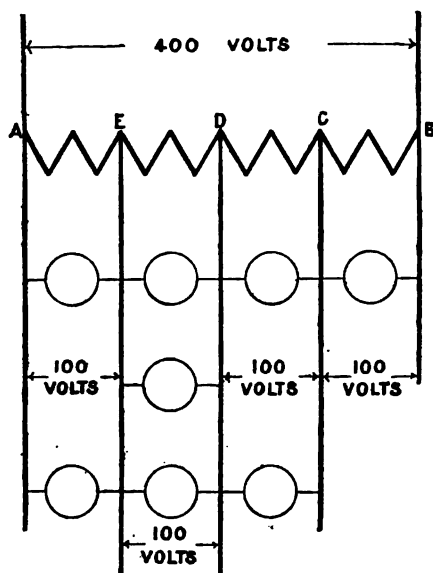
FIG. 20.

by the way, he calls an "auto-converter," as it consists of but *one* coil only, and no distinct primary and secondary circuits.

Fig. 21 shows one method of raising the potential by means of this "auto-converter." A B is a converter consisting of a single

coil, and as it is evident that one half of the coil acts to the other half as the primary of an induction coil to its secondary, the potential between A B is constant, and double that of A C. A current of, say, 500 volts could be applied between the points A C of the coil, and if A C were to equal C B, a current having a potential double that of A C could be taken off between A B— in this case 1,000 volts.
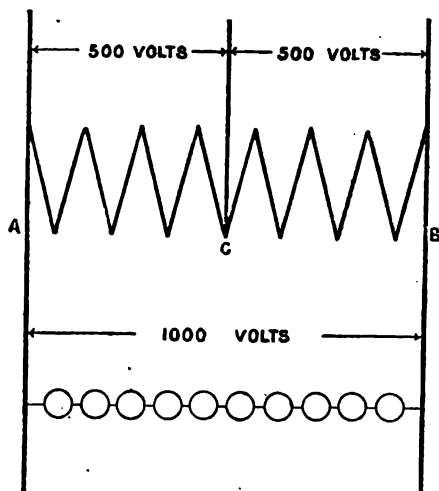
FIG. 21.

Fig. 20 shows theoretically an auto-converter reducing the potential. A B is a coil of one circuit only, and is divided into four equal divisions, viz., A E, E D, D C, and C B. If the potential between A and B be 400 volts, since the subdivisions are equal, they will each have a fourth part of the initial potential, and therefore may be employed individually to supply similar receivers as shown. If the resistance between adjacent divisions be equal, the arrangement becomes a series multiple coupling, and the coil A B has no work to do, consequently losing nothing by conversion. If, however, the resistance between any two adjacent divisions is varied—say, for instance, increased—then that portion of the coil A B included between those divisions acts as a primary to the rest of the coil, and the potential difference between all the branches remains constant.

From this it appears that the coil acts only to supply an

induced current to such resistances in any division as are less
than the resistance of the neighbouring branches, and therefore
the loss due to conversion is simply that due to the conversion
of the current flowing through the algebraic sum of the resistances
of the separate branches or divisions.   This form of converter
possesses the same merit in potential distribution that the three-
wire system does in the direct system.

I am not aware whether these auto-converters have been used
in practice, and I feel considerable doubt as to what the result
might be.   One difficulty which appears likely to stand in the
way of success with such a system of conversion is that as the
high-potential mains are directly connected to the lamp or other
receiver circuits, not only are such receivers placed at an equal
potential above the earth with the main, but any failure or
breakdown in the insulation of the receiver circuits would result
in a tendency to destroy the whole insulation of the system, not
only at the particular instrument directly connected to the fault,
but along the whole line.   When breakdowns occur in the
insulation between the secondary of an ordinary two-circuit
transformer and the earth, there is, at any rate, the insulation
between the secondary and primary existing as a safeguard
against putting the primary also to earth.

I will now conclude by referring you to Figs. 22 and 23,
which will graphically show you what the development of
electrical distribution by transformers or secondary generators
has done towards lessening the cost of a general electric supply ;
although, instead of "lessening the cost," we should, I think, say
"rendering electric supply *possible*."   The figures are taken from
Dr. F. L. Pope's article to the American journal before men-
tioned.   In Fig. 22, A, B, and C are drawn double the natural
size, but show proportionally the area of cross section of the total
mass of copper necessary to supply 5,000 16-c.p. glow lamps,
situated at a mean distance of 4,000 feet from the dynamo.   A
refers to the three-wire system, working at a potential of 200 volts,
with a fall of potential or loss of energy in the feeders of 10 per
cent.—the usual conditions under which this system is worked.
B shows the size of conductor required for the same work in an

installation based on the transformer system, allowing 2·5 per cent. loss in the supply mains—only one-fourth as much as in the direct system. If this loss were increased and made equal
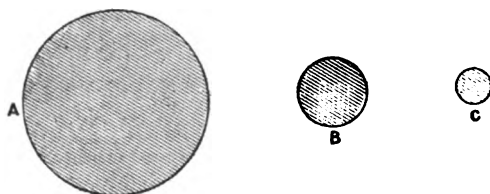
FIG. 22.

to that in the direct system, viz., 10 per cent., the size of conductor would be that shown at C.

Fig. 23 shows what the relative cost would be between each of the three conditions just named. This speaks for itself.
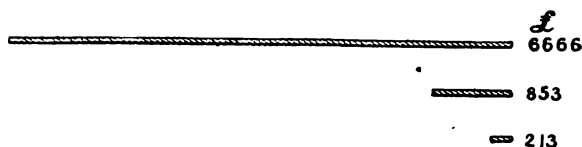
FIG. 23.

I believe you will agree with me that the introduction of transformers as applied to electric distribution is the greatest step forward that has been made since the great Electrical Exhibition in Paris of 1881.

I append a list of towns in which, as far as I can ascertain up to the present moment, alternating current and transformer supply stations exist, giving the approximate number of lights supplied; but it must of necessity be incomplete, through causes which those who have also tried to collect statistics can appreciate.

CENTRAL ELECTRIC LIGHT STATIONS ON THE TRANSFORMER SYSTEM.

Approximate Equivalent Output in 16-c.p. Lamps.

ENGLAND.

London (Grosvenor Gallery)		about 8,700...			Ferranti.
Brighton	...	...	...	...	...	Lowrie-Hall.
Eastbourne ...	...	...	...	...	,,

<div align="center">SCOTLAND.</div>

Cathcart	...	...	about 500	... Rankin-Kennedy.
Glasgow	...	...	... 600	... "

<div align="center">FRANCE.</div>

Tours...	...	...	about 2,000	... Gaulard-Gibbs.

<div align="center">GERMANY.</div>

Aschersleben...	...	about 1,000	... Gaulard-Gibbs.

<div align="center">ITALY.</div>

Tivoli...	...	...	about 450	... Gaulard-Gibbs.

<div align="center">UNITED STATES.</div>

Austin, Texas ...	1,300	... Westinghouse.
Beaver Dam, Wis.	1,300	... "
Galveston, Tex. ...	1,300	... "
Denver, Col. ...	7,000	... "
Steubenville, Ohio	1,000	... "
Ouray, Col. ...	300	... "
Tyrone, Pa. ...	1,300	... "
Charlestown W., Va. ...	650	... "
Morristown, N.J.	650	... "
Parkersburg W., Va. ...	2,000	... "
Trenton, N.J. ...	5,200	... "
New Orleans, La.	5,200	... "
Tampa, Fla. ...	600	... "
Springfield, Mass.	2,000	... "
Pittsburgh, Pa. ...	12,000	... "
" " (East End)	2,000	... "
Buffalo, N.Y. ...	2,000	... "
Greensburg, Pa.	1,300	... "
Torrington, Ct. ...	1,300	... "
Littleton, N.H. ...	650	... "
Phila, Pa. ...	2,600	... "
Stapleton, Staten Island	2,000	... "
Savannah, Ga. ...	1,300	... "
Portland, Me. ...	650	... "
Easton, Md. ...	650	... "
Colorado Springs, Col. ...	1,300	... "

Hartford, Ct.	...	...	...	2,000	...	Westinghouse.
St. Louis, Mo.	...	...	...	5,200	...	,,
Minneapolis, Min.		...	...	6,500	...	,,
Richmond, Va.	...	...	...	1,300	...	,,
Pittsfield, Mass.		...	...	1,300	...	,,
Sheffield, Ala.	...	...	...	650	...	,,
Cornwall, Ont., Canada		...	...	650	...	,,
Wheeling, W. Va.		...	...	2,000	...	,,
Chattanooga, Tenn.		...	...	1,650	...	,,
Pittsburgh, Pa. (S.S.)	...		...	—		
Allegheny, Pa.		...	...	—		
Philadelphia, Pa.		...	...	2,000	...	,,
Plainfield, N.J.	...	...	...	1,300	...	,,
Schenectady, N.Y.		...	...	1,300	...	,,
Philadelphia, Pa. (S. & C.)		...	1,300	...	,,
Carbondale, Pa.	...	...	...	1,950	...	,,
Lincoln, Neb.	...	...	...	2,000	...	,,
Flint, Mich.	...	...	...	650	...	,,
New London, Ct.		...	...	2,600	...	,,
Duluth, Minn.	...	...	...	1,300	...	,,
Bemington, Vt.	...	...	...	650	...	,,
St. Cloud, Minn.		...	...	650	...	,,
Fort Scott, Kans.		...	...	650	...	,,
Hillsdale, Mich.	...	...	...	400	...	,,
San Antonio, Tex.		...	...	650	...	,,
Springfield, O.	...	...	...	2,000	...	,,
Cheboygan, Mich.		...	...	650	...	,,
York, Pa.	...	...	...	650	...	,,
Oneonta, N.Y.	...	...	...	650	...	,,
Nashville, Tenn.	...	...	...	1,300	...	,,
Peekskill, N.Y.	...	...	...	650	...	,,
Cedar Rapids, I.A.		...	...	1,300	...	,,
Bath, Me.	...	...	...	650	...	,,
Port Huron, Mich.		...	...	650	...	,,
Stillwater, Minn.		...	...	650	...	,,
Marion, Kans.	...	...	...	650	...	,,
Junction City, Kans.		...	...	650	...	,,

Buffalo, Wyo. Terr.	...	...	400	...	Westinghouse.
Newton, Kans. ...	...	...	650	...	,,
Hoboken, N.J. ...	...	...	1,300	...	,,
Altoona, Pa. ...	...	...	650	...	,,
Havoac Tunnel ...	...	...	1,300	...	,,

A hearty vote of thanks was, upon the motion of the PRESIDENT, unanimously passed to Mr. Mackenzie for his paper.

The PRESIDENT : I will now ask Mr. Kapp if he will be good enough to communicate to us the object and substance of a communication which I understand Professor George Forbes left with him with the view to its being considered as his contribution to the subject we are discussing. Most of you are aware that Professor Forbes is absent from England, and it is very desirable that his communication should be introduced to-night, if possible, so that copies can be distributed to members who wish to refer to it before the discussion opens.

Mr. GISBERT KAPP then explained the substance of the following communication :—

FORMULÆ FOR CONVERTERS.

By Professor GEORGE FORBES, F.R.S. (L. & E.), Member.

The modern transformer for electric lighting is an induction apparatus in which the primary coil is of high, and the secondary of low, resistance, consisting of two ring coils of insulated copper wire enclosed in an iron sheathing, so subdivided as to prevent the formation of local currents in the iron when subjected to the influence of alternating electric currents in the coils. The terminals of the primary are kept at a constant mean difference of potential. The secondary circuit includes the lamp circuit, and is of varying resistance. The iron is so situated with respect to the coils that there is no free magnetism; this simplifies the mathematical treatment of the phenomena.

It is required to predict (1) the magnetic induction in the iron, (2) the work given off to the lamp circuit, (3) the whole work absorbed by the apparatus, (4) the waste of energy when

no lamps are in circuit, and (5) the effect which will ensue from varying the construction of the apparatus. The first of these has been approximately solved on the assumption that the permeability of the iron is constant.[*]

It is generally assumed that in such a system of electrical distribution the electro-motive forces, the electric currents, and the magnetic induction are simple harmonic functions of the time. This would not be the case if the phenomenon called by Ewing "magnetic hysteresis" is taken into account, and in a preliminary investigation this must be omitted. Its effect in practice is insignificant so long as the magnetic induction in the iron is not high.

The notation now adopted is as follows:—

p_1 = number of turns in the primary coil.

p_2 = ,, ,, ,, secondary coil.

r_1 = resistance of the primary coil.

r_2 = ,, ,, secondary circuit, including lamps.

c_1 = current in the primary circuit.

c_2 = ,, ,, secondary circuit.

m = integral of the induction over the cross-sectional area of the iron.

$\rho = \dfrac{\text{mean length of lines of induction in the iron}}{\text{cross-sectional area of iron} \times \text{permeability}}.$

$n = \pi \times$ number of alternations of E.M.F. per second ;

$= 2\pi \times$,, ,, complete periods of E.M.F. per second.

w_1 = work done in unit time by the primary current.

w_2 = ,, ,, ,, in the secondary circuit.

e = Difference of potential of primary terminals.

C.G.S. units are adopted throughout.

Assume $\qquad m = \text{M sin. } nt$ ... ... (1)

then $\qquad c_2 = -\dfrac{p_2}{r_2}\dfrac{dm}{dt} = -\text{M} n \dfrac{p_2}{r_2} \cos. nt$ ... ... (2)

and

$$w_2 = \int_0^1 c_2^2 r_2\, dt = \frac{\text{M}^2 n^2 p_2^2}{r_2}\int_0^1 \cos.^2 nt = \frac{\text{M}^2 n^2 p_2^2}{2 r_2}\int_0^1 (1 - \cos. 2nt)dt.$$

---

[*] J. Hopkinson, *Proc. Roy. Soc.*, 1887.

The mean value of the periodical part is zero. Hence

$$w_2 = \frac{M^2\, n^2\, p_2^2}{2\, r_2} \qquad \dots \qquad \dots \qquad \dots \quad (3)$$

Again, $M \sin. nt = \dfrac{4\,\pi}{\rho}\,(p_1\, c_1 + p_2\, c_2)\,;$

whence $c_1 = \dfrac{M\,\rho}{4\,\pi\, p_1}\sin. nt + \dfrac{p_2^2\, n\, M}{p_1\, r_2}\cos. nt \dots \qquad \dots \quad (4)$

$\qquad\qquad = C_1 \sin. (nt + a),$

if $C_1 \cos. a = \dfrac{M\,\rho}{4\,\pi\, p_1}$ and $C_1 \sin. a = \dfrac{M\, p_2^2\, n}{p_1\, r_2}.$

Again,

$c_1 = \dfrac{E}{r_1}\sin. (nt + \beta) - \dfrac{p_1}{r_1}\dfrac{dm}{dt}\,;$ if $e = E \sin. (nt + \beta),$

$\quad = \dfrac{E}{r_1}\cos.\beta \sin. nt + \left(\dfrac{E}{r_1}\sin.\beta - \dfrac{p_1\, M\, n}{r_1}\right)\cos. nt \quad \dots \quad (5)$

Equating coefficients in (4) and (5), we have

$$E \cos.\beta = \frac{M\,\rho\, r_1}{4\,\pi\, p_1};\ E \sin.\beta = M\, n\left(p_1 + \frac{p_2^2\, r_1}{p_1\, r_2}\right)$$

$$= M\, n\, p_1\left(1 + \frac{p_2^2\, r_1}{p_1^2\, r_2}\right);$$

whence $\dfrac{E^2}{M^2} = \left(\dfrac{\rho\, r_1}{4\,\pi\, p_1}\right)^2 + n^2\, p_1^2\left(1 + \dfrac{p_2^2\, r_1}{p_1^2\, r_2}\right)^2 \qquad \dots \quad (6)$

also, $w_1 = \int_o^1 c_1\, e\, dt = C_1\, E \int_o^1 \sin. (nt + a)\,.\,\sin. (nt + \beta)\,.\, dt$

$\qquad = \dfrac{C_1 E}{2}\displaystyle\int_o^1 \{\cos. (a - \beta) - \cos. (2\, nt + \beta + \gamma)\}\ dt$

$\qquad = \dfrac{C_1\, E}{2}\cos. (a - \beta) = \dfrac{C_1\, E}{2}(\cos. a \cos.\beta + \sin. a \sin.\beta)$

$\qquad = \dfrac{M^2}{2}\left\{ r_1\left(\dfrac{\rho}{4\,\pi\, p_1}\right) + \dfrac{n^2\, p_2^2}{r_2}\left(1 + \dfrac{p_2^2\, r_1}{p_1^2\, r_2}\right)\right\} \qquad \dots \quad (7)$

$\qquad = \dfrac{E^2}{2}\ \cdot\ \dfrac{r_1\left(\dfrac{\rho}{4\,\pi\, p_1}\right)^2 + \dfrac{n^2\, p_2^2}{r_2}\left(1 + \dfrac{p_2^2\, r_1}{p_1^2\, r_2}\right)}{\left(\dfrac{\rho\, r_1}{4\,\pi\, p_1}\right)^2 + n^2\, p_1^2\left(1 + \dfrac{p_2^2\, r_1}{p_1^2\, r_2}\right)} \dots \qquad \dots \quad (8)$

When the actual figures pertaining to a modern transformer are inserted, it is found that the quantity $\dfrac{p_2^2\, r_1}{p_1^2\, r_2}$ is less than ·01; that the first term of the numerator in (8) is small compared with the second; and that the first term of the denominator is very small compared with the second.

The efficiency is found by dividing (3) by (7).

When no lamps are used on the secondary circuit, the rate of work done is, by (7) (r_2 being infinite),

$$\text{waste} = \frac{M^2}{2}\, r_1 \left(\frac{\rho}{4\,\pi\,p_1}\right)^2.$$

It will be noticed that the efficiency of the apparatus depends on the first term in (7) being small compared with the second; hence n (or the number of alternations per second) may be diminished without loss of efficiency if ρ be diminished, or if the mass of iron be increased.

The PRESIDENT: The Council, foreseeing that in all probability time would not permit of the commencement of the discussion this evening, have arranged that an Extraordinary General Meeting shall be held on Thursday evening next, February 16th, to be devoted entirely to the discussion of the papers read this evening.

A ballot took place, at which the following were elected:—

Member:
Charles Hall.

Associates:

John Louis Balbi.	Arthur Frederick Guy.
Arthur Salisbury Baxendale.	Frank J. Lea.
John Coles.	Arthur E. Rossiter.
John West Curra.	Charles Henry Smeeton.
George Fletcher.	John Benjamin Verity.

Students:

Charles Henry William Gerhardi.	Douglas Potter.
Herman Koerner.	Percival S. Tasker.
William Vyvyan M. Popham.	

The meeting then adjourned.

An Extraordinary General Meeting of the Society was held at the
Institution of Civil Engineers, 27, Great George Street,
Westminster, on Thursday, 16th February, 1888—W. H.
PREECE, Esq., F.R.S., in the Chair, in the unavoidable
absence of the President.

The minutes of the previous Ordinary General Meeting were
read and confirmed.

Mr. KAPP, on the invitation of the Chairman, having pointed
out one or two typographical errors in the proof of his paper,
Professor Ayrton was called upon by the President to open the
discussion on the papers read by Mr. Kapp and Mr. Mackenzie on
the 9th inst.

Professor AYRTON : I think we ought to congratulate the authors Professor
Ayrton.
of these two papers on having given us two most interesting com-
munications on a subject of all importance at the present time.
I agree with Mr. Mackenzie that a good deal of credit is due to
Messrs. Gaulard & Gibbs, not for having invented a new method
of distributing electric energy, but for having drawn attention, in
an experimental way, to this method. At the exhibition close by
—in the Aquarium—in August, 1882, the system was worked,
but it certainly did not commend itself to many who saw it when
it was first shown. I think many of us who saw the secondary gene-
rators—or, rather, a secondary generator—at the Aquarium, had
no idea that in a few years the system would be employed on the
largest distribution of electric energy in this country. One reason,
I think, perhaps, why some of us at any rate did not appreciate it
was that the measurements shown us were made extremely inaccu-
rately, and we feared that possibly the system would be as unsatis-
factory as the methods employed to measure its efficiency. Of
course this in no way detracts from the advantages of the trans-
formers. I merely wish to refer to the impressions formed in
regard to the transformers when at work in 1882. For example,
the potential difference, or P.D., at the terminals of the trans-

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former was then measured by means simply of a high-resistance dynamometer, and that was multiplied by the current as measured by a low-resistance dynamometer; and the conclusion was at once come to that this product measured the watts put in, or the watts given out, as the case might be, and in this way an efficiency was obtained which might, or might not, have any connection with the true efficiency ; and therefore one felt more inclined in 1882 to be guided by the fact that the core of the transformer became extremely hot in concluding that the system was not practically good then, than by the results of experiments which were obviously incorrect. In attempting to measure the power given by an alternating current by the product of the readings of even a proper voltmeter, such as a Cardew voltmeter, and of a low-resistance dynamometer, exactly the same sort of error is made as if an endeavour were made to measure the power given out by a steam engine by measuring the product of the pressure as read by the pressure gauge into the speed as measured by the number of revolutions per minute, without taking any account as to whether there was any expansion of the steam in the cylinder, or what was the adjustment of the valves. Every mechanical engineer knows that if the mean pressure were so many pounds per square inch in the cylinder, and the engine were going at so many revolutions a minute, no scientific conclusion could be drawn from those two facts *alone* as to the horse-power the engine was developing. Now the volt measurements were not in 1882 made by the Cardew voltmeter, because it did not exist (as far, at least, as I can remember), but by a high-resistance dynamometer, which added another error, so that there was a collection of errors in the measurements. Nevertheless, the system is, in spite of this, an extremely good one, as shown by the fact that it is now used very extensively in London and elsewhere in Great Britain, and very largely used, as shown at the end of Mr. Mackenzie's paper, in the United States.

The next point that perhaps would suggest itself in this discussion is the question of safety to life. Of course you are dealing with a very high potential difference in the primary. How can we take care in this system to avoid danger when the

primary touches the secondary ? A very ingenious device, due to Professor Ayrton. Mr. Kent, was described by Mr. Kapp last time, and he also showed an equally ingenious arrangement of Captain Cardew's—a combination of electrometer and fuse. But it appears to me that there is a far simpler device, which merely consists in putting one wire of every secondary to earth. What we want, of course, is to ensure, not merely that the difference of potentials between the terminals of the secondary shall not exceed a certain value, but that the mean potential of the secondary circuit shall not differ from the potential of the earth by more than a certain value. It is not sufficient to know that the P.D. between the two ends of the secondary is never more than a hundred volts, but we must be sure that the potential of no point of the secondary circuit is some hundreds of volts different from the potential of the earth. Well, that condition, it seems to me, is most easily obtained by putting one of the terminals of every secondary to earth. If there be proper insulation between the primary and the secondary, the device I have suggested will not affect the insulation of the primary circuit in the least, and it ensures perfect safety in the secondary circuit. It also has this advantage (pointed out some time ago), that it prevents a discharge due to the electrostatic capacity of a transformer, which may be considerable, and which may give you a severe shock. Now this device of putting one terminal to earth is one that gets over that difficulty, and you get perfect safety with an arrangement which cannot possibly fail, and which, while simpler than the one due to Mr Kent, is less liable than Mr. Kent's to lead to leakage from the primary to earth.

I have spoken of the " potential difference," or P.D., at the terminals of the secondary. I think that is better than Mr. Kapp's expression of " electro-motive force." " Electro-motive " force " should not be employed when you are speaking of the potential difference between two points, because you are liable to confuse it with the electro-motive force of the whole circuit. " Electro-motive force " ought to be retained as an expression for the electro-motive force of the whole circuit. But the potential difference between the terminals of the primary, which is what

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Mr. Kapp employs in his calculations, should be called "potential "difference," and not "electro-motive force." Whether you like to employ the very convenient abbreviation P.D. is one thing, but calling it an E.M.F. is another, and is liable to lead to confusion in calculations, especially when they become complex.

It has been held by the authors of these papers that it is impossible to use transformers *in series* if we wish to maintain a constant P.D. at the terminals of the secondary when the resistance of the secondary is altered ; that is to say, it is impossible to maintain constant P.D. between the lamp mains of the secondary circuit if the number of lamps *in parallel* in the secondary circuit be altered. Well, no doubt, if you have nothing more than the mere primary and secondary, and if you keep a constant current through the primary circuit, which is all you can do if the transformers are in *series*, it is impossible to produce a constant P.D. between the terminals of the secondary. But there is a means of producing a constant P.D. between the terminals of the secondary, which, it appears to me, would lead to good results. I do not know whether it has been tried. The method is one analogous with the compounding of a dynamo. In a dynamo, compound-wound, we use, of course, a shunt coil ; but we have also a series coil on the field magnets, which increases the magnetisation of the field magnets when the current supplied by the dynamo is increased. Now we can get exactly the same thing with transformers. Supposing the secondary coil, S (Fig. 1), be joined in the ordinary way to the lamps, L, let there be in addition a second induction coil, S' T, the primary of which, S', is in series with S, and the secondary, T, so joined up that it adds a P.D. to the secondary circuit, and adds the greater P.D. the greater the current passing through the secondary. That arrangement is exactly the arrangement of a compound-wound dynamo ; the tertiary coil corresponds with the series coil of the compound-wound dynamo. That is to say, a constant current passing through the primary coil, P, will produce a constant electro-motive force in the secondary, S, and a P.D. will be added to the secondary circuit, the added P.D. being the greater the greater the current in the secondary circuit S. At any rate,

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my colleague and I believe it may be possible by such an arrange-
ment to do this, and so obtain transformers capable of working in
series and maintaining a constant P.D. at the terminals of each
secondary independent of the number of lamps in any secondary
circuit.
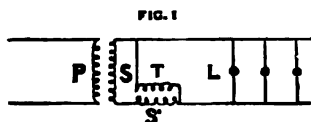
FIG. 1

Fig. 1 shows Ayrton and Perry's proposed self-regulating transformer for
use with transformers in series on the main circuit. P, primary coil, with
constant (mean alternating) current; S, secondary coil joined up to lamps, L;
S′ T, auxiliary regulating transformer; S′ the exciting coil in main secondary
circuit, acting as primary to coil T. When the P.D. at the terminals of S falls
off by reason of increase in the current through S, this increase of current
passing through S′ excites a larger E.M.F. in T, which thus adds a P.D. to the
lamp circuit, compensating for the falling off of the P.D. set up by the main
secondary coil S alone.

It is not necessary, of course, that these two transformers, P S
and S′ T, should be quite independent; all that is necessary is to
put a tertiary coil on the transformer, so that on each transformer
you have a primary, a secondary, and a tertiary coil. Some three or
four years ago my colleague and I endeavoured to work the thing
out mathematically, to find the exact condition of getting a constant
P.D. between the mains of the secondary for constant alternating
currents in the primary. But when you come to deal with
primary, secondary, and tertiary wires the mathematics become
extremely complicated, and we thought we should do better by
resorting to experiment. Apparatus was made; and it was for
these experiments that the little voltmeter we had the honour of
bringing to your notice last year was devised.* It was necessary
to measure a few volts accurately, and therefore we required a
voltmeter that measured four or five alternating volts accurately;
and as no such instrument was obtainable we had to devise one.
Just when the experiments were about to start, I came across an
account by Messrs. Gaulard & Gibbs, I believe in the *Engineer*,

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* "Portable Voltmeters for Measuring Alternating Potential Differences,"
Jour. Soc. Tel.-Engrs. and Elecs., vol. xvi., page 589.

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Ayrton. putting forward exactly the same device.   Imagining that, as they
were engaged on the Grosvenor Gallery installation, they had
more opportunity of making experiments than we had, we did not
pursue the matter further.   Whether they arrived at any definite
conclusion I do not know: I have heard nothing more about it
since.   I should be glad to hear from Mr. Mackenzie* whether this
system was seriously tried.   I can imagine that it did not give
proper results when first tried—just as the first compound-wound
dynamo was not perfect; but it would be interesting to know
whether it was ultimately successful.   Had we not come across
that account, published by Messrs. Gaulard & Gibbs at the same
time that we were starting on our own experimental investigations,
we certainly should have pursued the matter and endeavoured to
arrive at success.

The rationale of the device is quite simple: just as the
strength of the field produced by the shunt coil is increased
by the series coil in a compound-wound dynamo, so the
P.D. set up between the lamp mains in the house by the main
secondary circuit is increased by the P.D. set up by the tertiary
coil, and it is increased the greater the current passing through
the lamps; therefore it appears to me that the arrangement would
work satisfactorily if properly proportioned.

I am very glad to see that the idea of magnetic "lag," which
means retardation of iron to be magnetised, quite apart from the
production of Foucault currents in the iron, is gradually gaining
acceptance, especially in consequence of the admirable work that
has been done on this subject by Professor Ewing.   The idea of
a time-magnetic lag, as apart from Foucault currents, was put
forward some years ago in this room in our paper on "Electro-
"Motors and their Government;" and that the conception was
used in all our calculations was simply laughed at by some writers,
and our names in some quarters were held up to reproach from
our having brought out the idea of magnetic lag as something
different from the lag due to Foucault currents.   We believed

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* The answer given by Mr. Mackenzie, in his reply, to this question is not
to the point.   The arrangement he there refers to is in no way analogous with
a compound-wound dynamo.

then, as has since been proved, that there is a distinct delay in Professor Ayrton. iron to accept magnetism, or to lose it, quite apart from Foucault currents, and which cannot be entirely avoided by any amount of lamination.   Now, however, that the Greek name *hysteresis* has been given to the kind of phenomena which have long been known to exist, and which have been attributed to what was called the coercive force of iron, the classically educated Englishman is willing to believe in the existence of a *time lag* also.*

I have spoken about the wrong ways of making measurements on the efficiency and power given by transformers.   Let me say a word about right ways of making such measurements ; and I am interested in doing this because, through the kindness of Mr. Kapp, who lent us one of his transformers some time ago, experiments have been made by the third-year students of electrical engineering at the Central Institution, who have displayed, in connection with the subject, laudable perseverance.   There is only, so far as I know, one method for measuring the power given to a circuit which contains self or mutual induction, and which is *absolutely independent of the law connecting the current and the time.*   The method is one employing the quadrant electrometer, and is that which was adopted by Dr. Hopkinson in measuring the efficiency of Messrs. Gaulard & Gibbs' transformers on the Underground Railway some few years ago.   Let A B (Fig. 2) be the circuit possessing self or mutual induction, or both, the power given to which it is desired to measure.   Place in series with this a *non-inductive* resistance, B C, of known value r ohms, and send a current through the two by means of the alternating-current dynamo, **A.**   Join the pairs of quadrants of an electrometer to the points A and B, and the needle to C; then, if A, B, and C represent the potentials at any moment of the three points, all measured relatively to the outside of the electrometer, and if d be the steady deflection of the electrometer,

* The very striking experiments that have recently been conducted by Dr. Lodge, in connection with the subject of lightning conductors, place the existence of a *time lag* in iron beyond all further doubt.

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$$d = \frac{k}{2\,\tau} \int_0^{2\tau} (A - B) \left(C - \frac{A + B}{2} \right) d\,t,$$

where τ is the interval between two alternations. Next, join the needle to the point B, and let a steady deflection, D, of the electrometer be now obtained: then

$$D = \frac{k}{2\,\tau} \int_0^{2\tau} (A - B) \left(B - \frac{A + B}{2} \right) d\,t.$$

Consequently,.

$$\frac{D - d}{r\,k} = \frac{1}{2\,\tau} \int_0^{2\tau} (A - B) \left(\frac{B - C}{r} \right) d\,t.$$

Now the expression on the right-hand side is the mean value of the true watts given to the circuit $A\,B$, therefore $\dfrac{D - d}{r\,k}$ measures this mean value of the true watts.
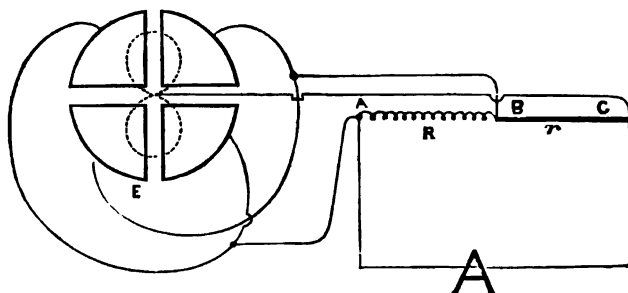
Fig. 2.—R, inductive resistance to which power given by alternating current is to be measured; r, non-inductive resistance ; E, electrometer; A, alternating-current dynamo.

This test may be varied by attaching, first, the quadrants to B and C, and the needle to A, obtaining a steady deflection d_1 ; next attaching the needle to B, leaving the quadrants as before, obtaining a steady deflection D_1. Then

$$d_1 = \frac{k}{2\,\tau} \int_0^{2\tau} (B - C) \left(A - \frac{B + C}{2} \right)$$

$$D_1 = \frac{k}{2\,\tau} \int_0^{2\tau} (B - C) \left(B - \frac{B + C}{2} \right)$$

$$\therefore \frac{d_1 - D_1}{r\,k} = \frac{1}{2\,\tau} \int_0^{2\tau} \left(\frac{B - C}{2} \right) (A - B) \, d\,t.$$

With either of the two methods of testing with an electrometer, the watts—the true watts—are found correctly independently of

any amount of self or mutual induction in the circuit *A B*, Professor
Ayrton. and *independently of the law connecting the P.D. with the time;* the P.D. may be a sine function, or any other function, of the time *without affecting the accuracy of this method of testing* the power given to the *circuit A B.* It is rather interesting, because the method occurred simultaneously to Dr. Fitzgerald, of Dublin, and myself, at the Paris Electrical Congress. We were listening to M. Joubert's paper on the measurement of alternating volts with an electrometer—of course a very much simpler problem—and when he was reading this paper it occurred to both of us to see whether it was not possible to do something more with the electrometer—that is, not merely to measure alternating volts, but to measure the power which is given to the circuit. A Cardew voltmeter will measure the former, or of course you can measure alternating volts in this way—by joining one of the pairs of quadrants to the needle, and then joining to the two pairs of quadrants the two points the difference of potential between which you desire to measure. If you want, for example, to measure the difference of potential between any two points *P* and *Q*, you connect the needle to one pair of quadrants and to *P*, while the other pair of quadrants is connected with the outside of the electrometer and with *Q* : then, as is well known now, the deflection of the needle is proportional to the square of the difference of potential. It was during M. Joubert's reading of this paper that the further device occurred to Dr. Fitzgerald and myself of employing an electrometer to measure what cannot be measured with a voltmeter and dynamometer, and after the meeting he came to me and said, " I have a method of measuring the power." I told him that I, too, had a method of doing the same thing, and I said, " If " you tell me yours, I will tell you mine." Well, our methods, or, rather, the method—for they are the same—is theoretically perfect ; but it has distinct practical objections. The first objection is that it requires that a non-inductive resistance, *B C* (Fig. 2), be put in series with that part of the circuit *A B* the power given to which you wish to measure. Of course there is a certain amount of power wasted in this wire *B C*. But

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that does not interfere with the accuracy of the experiment.. Though the introduction of the resistance $B\,C$ into the circuit wastes a certain amount of power, it does not destroy the accuracy of our measurement of the power given to $A\,B$; but if a dynamo were already being worked to its limit, giving power to, say, lamps $A\,B$, it would be inconvenient to have to introduce an additional resistance $B\,C$ into the circuit merely for the purpose of enabling the power to be measured.

There is, however, another and more serious objection, which I was not aware of until within the last couple of years; that is, this formula is worked out on the assumption that the ordinary law of the electrometer, as given, for example, by Clerk Maxwell, is true. The ordinary law of the electrometer, as given in text-books, is that the deflection of the needle is proportional to the potential difference between the quadrants into an expression containing the potential of the needle minus half the sum of the potentials of the quadrants.

Now, unfortunately, the experiments that have been conducted at the Central Institution have shown that, so far from this being the formula of the electrometer, it is the exception for the electrometer, *unless the quadrants are placed in a special position relatively to the needle*, to have this formula. I will not go further into that at the present time: it will form the subject of a subsequent communication. I merely mention it to warn you, if you use the electrometer for measuring the power given by an alternating current to a circuit, to remember that the method that I have described has been developed on the assumption that the formula given in text-books is correct; and that if you find that the method leads to wrong results it is not a proof that the method is wrong, but a proof that the formula for the electrometer is different from what it is usually assumed to be. I warn you, therefore, if you want to employ a very perfect method for measuring power, to be quite sure that you have a very perfect electrometer—at any rate an electrometer arranged so that this formula is true. We have found out that, as a rule, this formula is not true, and that a method thus carried out, pretty as it is, and perfect as it is,

is inapplicable. The third-year electrical students at the Central Institution have made any number of experiments with this method, and the results generally have been very unsatisfactory. There is only one other method I know of by which you can directly measure the power and efficiency of a transformer, and which, like the former, is absolutely independent of the law connecting the P.D. with the time. It is not an electrical method at all. It is this. We want to measure the power, of course, given to the transformer, and the power given out by the transformer. How shall we do it? The power given out by the transformer can be easily measured, if you only take care that the secondary circuit is giving a current to incandescent lamps. Suppose that a secondary circuit is arranged to give power to a *non-inductive* resistance like a set of incandescent lamps. Then the power given out by that circuit would be quite accurately measured, since there is no lag, by simply taking the square root of the mean square of the volts multiplied by the square root of the mean square of the ampères; that is, the reading of a Cardew voltmeter put as a shunt to the circuit, multiplied by the square root of the reading of a dynamometer put in the circuit, gives you the power given out by that circuit. I say square root of the reading of a dynamometer, since dynamometers are unfortunately not usually calibrated to read off ampères *directly*, and in this respect are far behind ammeters for direct currents. To avoid circumlocution, however, I shall assume in what follows that we are employing a *direct-reading* dynamometer— that is, one graduated so that the deflection gives at once the square root of the mean square of the current. You cannot get the power given to the primary by that method, in consequence of the self-induction of the primary and the mutual induction of the primary and secondary. Well, how can you estimate the power? Put the transformer in a calorimeter, C (Fig. 3). First put it in a water-tight box, then in another box not necessarily water-tight at the top. Arrange a definite non-inductive resistance, r_2, in the secondary circuit, and send a definite current through the transformer for a certain time, and allow water to enter, say, the bottom of the calorimeter and to

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flow out at the top, and measure the temperature of the entering water and that of the water which leaves with the thermometers T, T, and keep the current flowing until everything has become
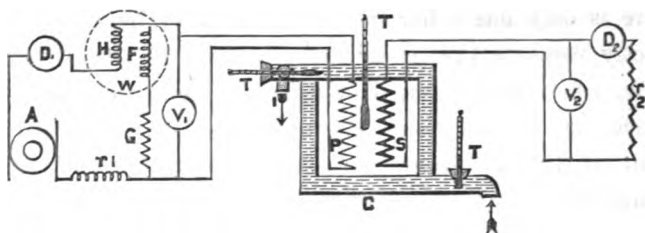
Fig. 3.—A, dynamo; C, calorimeter; T, T, thermometers; P and S, primary and secondary coils; D_1 and V_1, dynamometer and voltmeter in primary circuit; D_2 and V_2, dynamometer and voltmeter in secondary circuit; W, wattmeter—H being thick-wire coil, F suspended fine-wire coil of 2·16 ohms, and G the stationary non-inductive resistance of 895·5 ohms in series with F; r_1, adjustable resistance in primary circuit; r_2, non-inductive resistance in secondary circuit.

quite steady, until the temperature of the water as it flows out of the calorimeter ceases to rise, which in some of the experiments took as long as three hours. Then, quite apart altogether from the specific heat of the transformer and calorimeter and so on, we have at once the power wasted in the transformer. Suppose, for example, that W is the number of grammes of water that flow out in t seconds, and supposing T is the difference of temperature between the entering water and the leaving water, which I suppose to have become quite steady : then the power, in watts, wasted in the calorimeter, given out in the form of heat, is equal to W, the number of grammes, or cubic centimètres, of water, multiplied by the difference of temperature T, divided by the time t in seconds, into a constant, ·239. That gives you the power wasted in the transformer. That, added, of course, to the power given out by the secondary—which is obtained from the measurement previously described—gives you the power put into the primary, and you have then the whole measurement with a considerable amount of accuracy. Let me repeat, power given out by the secondary is accurately measured, provided that the secondary is attached to incandescent lamps or to something having no self-induction nor mutual induction, by the square root of the mean square of the volts multiplied by the square

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root of the mean square of the current—that is to say, is measured by the reading of the Cardew voltmeter, V_2, multiplied by the reading of the Siemens dynamometer, D_2. That is the power given out in watts. The power wasted is measured by the formula already given. The ratio of the watts obtained in secondary to the sum of the two is, of course, the efficiency of the transformer. Well, that has given us very fair results. We have also put in a voltmeter, V_1, and a dynamometer, D_1, in the primary circuit (Fig. 3), to see what sort of result you get arising from lag in the primary current. We had something else : we not only had the apparatus already described, but, for the interest of the thing, we had an ordinary wattmeter, W, in the primary circuit. That is, in addition to the dynamometer D_1, there was the thick coil, H, of a wattmeter in the primary circuit, the fine coil, F, of the wattmeter being a shunt to the primary circuit. Now you see the sort of results that are obtained with the different systems of measurement ; and the difference between the results is most interesting, as we know which are the results which are true. The experiment was carried on in each case for a sufficiently long time—the dynamo, A, being kept running at a constant speed—for everything to become perfectly constant. In the first case the secondary circuit, S, was broken altogether —*i.e.*, there was no current in the secondary circuit. This particular transformer was an exact *fac-simile* of a transformer which Mr. Kapp had on the table last time, and I may mention that Mr. Kapp's method of indicating the power of transformers is an extremely satisfactory one. Very often, when new apparatus is devised, the nominal power is stated (for reasons pretty obvious) in excess of what the real power is. Transformers, at least Mr. Kapp's, are on a par with the steam engine—that is, the nominal power is much less than the actual power that can be developed with them. I do not know whether Mr. Kapp's dynamos are equally good. If so, they are extremely good. This is a two-horse-power transformer, which does not, I am happy to say, give you only one horse-power, but three, and even nearly four horse-power without excessive heating.

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KAPP AND SNELL TRANSFORMER.

Experiments made at the Central Institution by Messrs. Dykes, Lamb, Priest, Smith, and Zingler.

Primary		Secondary		Calorimeter.	Watts given out by Secondary.	Watts given to Primary.			Efficiency.		
a Volts.	b Ampères.	d Volts.	f Ampères.	g Watts wasted in Heat.	$h = \sqrt{d^2 \times f^2}$	$g + h$ True.	$\sqrt{a^2 \times b^2}$	By Watt-meter. j	$\dfrac{100\,h}{g \times h}$ True.	$\dfrac{100\,h}{\sqrt{a^2 \times b^3}}$	$\dfrac{100\,h}{j}$
184·3	0·8	94·5	0	53·18	0	53·18	147·2	98·5	0	0	0
184·6	2·3	96·8	3·3	79·05	318·7	397·8	424·5	446·7	80·1	75·1	71·4
187·6	3·32	97·7	5·09	82·68	495·6	577·9	623·7	653·2	85·3	79·5	75·9
186·1	8·31	94·8	14·87	55·6	1,416	1,472	1,546	1,727	96·2	91·5	82
185·6	16·14	91·44	30·59	149·9	2,813	2,968	2,995	3,277	95·0	98·9	85·9

Each test given in the above table is the mean of five or six consistent results.

Efficiency $\dfrac{100\,h}{\sqrt{a^2 \times b^3}}$ neglects the lag due to the self and mutual induction of the transformer.

Efficiency $\dfrac{100\,h}{j}$ neglects the lag due to the self and mutual induction of the transformer ; also the lag in the fine-wire coil of the wattmeter due to its self-induction.

Time of an alternation in all the experiments was about $\frac{1}{1000}$th of a second.

Well, in the first case the volts of the primary circuit were 184·3. Professor
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The volts of the secondary (the circuit being open in this case)
were 94·5. Then the power in watts put in, as measured by—let
us take them in order—the wattmeter (the constant, of course,
being carefully determined, not merely being the constant as
marked on the instrument, which might be supposed to be correct
for power supplied for direct currents, but for the constant alter-
nating currents), was 98·5 watts, as given in column headed j. The
watts put in, measured by the product of the dynamometer and
voltmeter, were 147·2, as given in the column headed $\sqrt{a^2 \times b^2}$.
But the actual power put into the transformer was only 53·18
watts, as stated in the column headed $g + h$; therefore both of
the former measurements gave results enormously too large.
Why? Because when the secondary circuit was broken the
effective self-induction of the primary circuit, P, was very much
larger than when the secondary circuit was closed, and the errors
both of the wattmeter and of the voltmeter and dynamometer
became immensely magnified—the readings were altogether wrong.
The wattmeter says 98·5 watts put in. The product of the volt-
meter and dynamometer says 147·2 watts put in. But the real
watts, as given by the calorimeter (and of course in this case, as the
secondary was open, all the power was wasted in heat), were 53·2.
The electric measurements, then, indicated from two to three times
the actual number of watts given to the primary circuit. Of
course, as the secondary circuit is closed and less and less
resistance is put in, the readings given by a wattmeter and by the
voltmeter and dynamometer become more nearly the same and
more nearly equal to the truth, the truth being in each case given
in the column headed $g + h$. But even for the maximum power
they were *not* equal. For example, when 2,963 watts were really
being given to the transformer, which is about four horse-power,
the square root of the mean square of the volts into the square root
of the mean square of the ampères gave 2,995 watts, and the watt-
meter 3,277 watts. The transformer was worked in the next case
with about two horse-power, or, more accurately, 1,472 watts were
given to the primary, when the product of the voltmeter and
dynamometer readings gave 1,546 watts, and the wattmeter 1,727

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watts; the true efficiency in this case, given in the column

headed $\dfrac{100\,h}{g \times h}$, being over 96 per cent.

The differences in the amounts of power given to the transformer, measured in the different ways, do not arise from instrumental errors, but from errors in two of the methods arising from neglect of the effects of the self-induction of the primary circuit and the mutual induction between it and the secondary. It is interesting to observe that the readings in the column headed j are higher, not merely than those in column headed $g+h$, but, with the exception of the first value, higher than those given in the column headed $\sqrt{a^2 \times b^2}$. One would expect the product of the voltmeter and dynamometer readings would give an answer higher than the true watts given to the primary circuit, as this product neglects altogether the effect of lag in the primary. You might not at first sight have seen whether the watts as measured by the wattmeter should or should not have been higher than the true watts. In the published writings of Dr. Fleming[*] and others it is stated that a wattmeter used to measure the power given to a circuit by alternating currents reads too low; that is, the watts as measured by a wattmeter are less than the true watts. But experiment shows that this is not the case, for the watts as measured by the wattmeter are higher than the true watts; and not only are they higher than the true watts, but, excluding the first result, they are even higher than the watts measured by the square root of the mean square of the volts into the square root of the mean square of the ampères. That led me, in one of my lectures to the third-year electrical students, to investigate the question as to whether a wattmeter ought to give you a result greater or less than the true watts, and the result I have obtained is as follows. Take a wattmeter—one like a Siemens wattmeter, say—which has practically no mutual induction, but has some self-induction. If a wattmeter be employed to measure power in a circuit having self-induction, this seems to be what is obtained:—If τ be the time of an alter-

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[*] " Short Lectures to Electrical Artisans," page 149.

nation, r the resistance, and L the coefficient of self-induction of Professor Ayrton. the fine-wire circuit of the wattmeter, r' and L' the resistance and self-induction of the circuit the power given to which it was desired to measure, then the reading of the wattmeter, compared with the true power which they wanted to measure, has this value—

$$\frac{r}{r'} \times \frac{r^2 r r' + \pi^2 L L'}{r^2 r'^2 + \pi^2 L'^2} \quad \dots \quad \dots \quad (1)$$

It is possible, by putting but a few convolutions on the suspended fine-wire coil of the wattmeter, and using for the greater part of the resistance of the fine-wire circuit a stationary non-inductive resistance, to make L, the coefficient of self-induction of the fine-wire circuit of the wattmeter, very small; but unless it were made absolutely nought the term $\pi^2 L L'$ may be large in consequence of the largeness of L', and the expression would be the greater the greater L' was. So that, in other words, you might expect, when the circuit the power given to which you wished to measure had very much self-induction, that the wattmeter would give an answer far too high. In the wattmeter used at the Central Institution, the self-induction of the fine-wire circuit was made very small in the way just indicated, the resistance of the suspended coil having only $\frac{1}{150}$th of the value of that of the non-inductive stationary coil, but still the watts given to the primary as measured by the wattmeter were 100 per cent. too large when the secondary circuit was open.

If $\dfrac{L}{r}$ be small, the expression just given becomes approximately equal to

$$1 + \frac{\pi^2}{r^2} \cdot \frac{L}{r} \cdot \frac{L'}{r'} \dots \quad \dots \quad \dots \quad (2)$$

From this it is easy to see that as the resistance in the secondary circuit is made less and less, and L', the effective self-induction in the primary circuit, becomes less and less, the above expression becomes nearer and nearer equal to unity. And this is more or less borne out by the results of the experiments, for the ratios of the wattmeter watts to the true watts are—

Professor
Ayrton.

Ampères in Secondary Current.				Ratio.
0	...	...	...	$\dfrac{98 \cdot 5}{53 \cdot 18} = 1 \cdot 85$
14·87	...	...	...	$\dfrac{1,727}{1,472} = 1 \cdot 17$
30·59	...	...	...	$\dfrac{3,277}{2,963} = 1 \cdot 11$

the ratio becoming more and more nearly equal to unity as the current in the secondary circuit is made larger and larger.

The wattmeter, then, will always give *too high* an answer if the time constant $\dfrac{L}{r}$ of the fine-wire coil be small, but not nought; and the ratio of what you think you are measuring to what you ought to measure will increase as the coefficient of self-induction of the circuit the power given to which you are endeavouring to measure is increased; that is, the wattmeter will be least accurate when the secondary circuit has the highest resistance, and will be relatively most accurate when the secondary circuit is of least resistance.

The reason why the mistake has hitherto been made of assuming that a wattmeter reads too low instead of too high when used to measure the power given to a circuit with alternating currents is because only one of the errors affecting the wattmeter has been taken into account. In consequence of the self-induction of the fine-wire coil the current through it is diminished, and on this account, no doubt, the wattmeter reads too low; but self-induction has another effect, viz., that of retarding the phase of the current in this coil, and the result of this error depends on the amount of retardation that the current suffers in the main circuit the power given to which it is desired to measure. If the time constant $\dfrac{L'}{r'}$ of this circuit be very small, so that there is practically no retardation of the current wave in it behind the P.D. wave, then the second error of the wattmetter will be added to the first, and the wattmeter will certainly read too low. This is seen at once from the complete expression (1), which may be written in the form

$$\frac{\tau^2 + \pi^2 \dfrac{L}{r} \cdot \dfrac{L'}{r'}}{\tau^2 + \pi^2 \dfrac{L^2}{r^2}} \quad ... \quad ... \quad ... \quad (3)$$

and which is less than unity if $\frac{L'}{r'}$ be very small. If, however, the Professor Ayrton.
time constant $\frac{L'}{r'}$ of the main circuit be not nought there will be a lag of the current in the main circuit behind the P.D. wave. The power then given to the main circuit for a given value of the current and of the P.D. will be less than if $\frac{L'}{r'}$ were nought, and will approximate more and more to the watts as measured by the wattmeter. And when the time constant $\frac{L'}{r'}$ of the main circuit equals the time constant $\frac{L}{r}$ of the fine-wire circuit of the watt-meter the expression (3) becomes unity; that is, the two errors in the wattmeter balance one another. When, however, $\frac{L'}{r'}$ exceeds $\frac{L}{r}$, then (3) becomes greater than unity; that is, the lag error in the fine-wire coil of the wattmeter—the error which has hitherto been neglected—exceeds that due to the diminution of the current in this coil; that is, (3) becomes greater than unity, and (3) approaches (2) more and more as $\frac{L'}{r'}$ becomes larger and larger compared with $\frac{L}{r}$. The error, therefore, in the wattmeter reading—which may be either negative, nought, or positive, according as $\frac{L'}{r'}$ is less, equal to, or greater than $\frac{L}{r}$—is usually *positive*, since $\frac{L'}{r'}$ is usually much larger than $\frac{L}{r}$, and is not *negative*, as has hitherto been stated by writers; that is, the wattmeter reads *too high*, and not *too low*.

If we now plot the efficiencies which are given in the last three columns of the table as ordinates, and the values of the true watts given out by the secondary circuit of the transformer as abscissæ, we obtain the curves shown in Fig. 4; the upper curve representing the true efficiency, the next that obtained from the values obtained from estimating the power given to the primary circuit of the transformer from the product of the square root of the mean square of the volts into the square root of the mean square of the ampères, and the lower curve by

Professor
Ayrton.
regarding the power given to the primary circuit as being
correctly measured by the wattmeter. And from this we see in a
marked way that, in spite of the error arising from disregarding
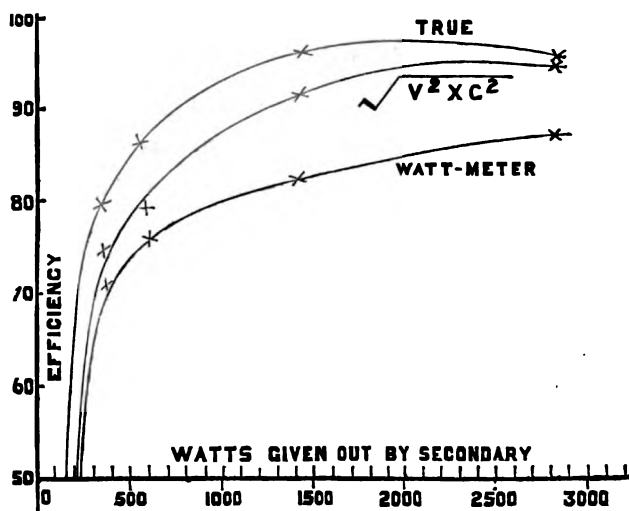
FIG. 4.

the lag in the primary circuit of the transformer which is made
when the power is calculated from the product of the voltmeter
and dynamometer readings, a still greater error is made in the
efficiency if the power given to the transformer be regarded as
being correctly measured by the wattmeter.

Captain
Cardew.
Captain P. CARDEW: As I had not the advantage of hearing
Mr. Mackenzie's paper, and have not yet had an opportunity of
studying it, I will confine my remarks to Mr. Kapp's paper,
merely expressing my sense of the deep obligation of all
electricians to Messrs. Gaulard & Gibbs, and the gentlemen
connected with them, for their perseverance in working out a
practical transformer system, in spite of every sort of discourage-
ment and hostile criticism.

I think Mr. Kapp's paper will be generally considered a
valuable addition to the rather meagre information at present
available on the subject. Still I do not anticipate for it such a
practical success as attended his paper on dynamo construction,
and this for two reasons—first, that the modern theory of

magnetism has considerably advanced, and is more generally Captain
Cardew. understood now, since the publication of Mr. Kapp's former paper and the very valuable ones by the Messrs. Hopkinson; second, because the construction of transformers for alternating currents is obviously not limited by so many conditions as that of dynamos, while those which do govern their construction are so very analogous.

There are two points in the construction of an efficient transformer on which I believe Messrs. Gaulard & Gibbs have always laid great stress—they were discussed, I understand, in Mr. Mackenzie's paper—viz., that the coils must have equal mass, and must be wound identically with respect to the core. I hope Mr. Kapp will give us his opinion as to the importance of these conditions.

There are two assumptions in Mr. Kapp's paper which I am afraid somewhat diminish the confidence one would like to feel in his beautifully simple graphical representation of the action in a transformer, viz., the sine function curve and the coincidence of the magnetisation with the effective exciting power. Recently published experiments are certainly adverse to the truth of both of these assumptions; and if the latter be true, what is the real meaning of hysteresis?

The chief theoretical novelty in this paper is the method given of making allowance for the power lost in heating the iron core. The difficulty for which a solution is here attempted is that this lost power affects to some extent the angles of lag; and, unless we can allow for this, the electrical power supplied cannot be exactly computed, except, perhaps, by a wattmeter. I do not feel quite happy about Mr. Kapp's solution. The assumption is that hysteresis produces an inductive effect always additive to that of the secondary coil, and if there be a true molecular friction this may be the case; but surely this is not the whole of the molecular force, and the molecular force as a whole opposes magnetisation and helps demagnetisation, while the friction alone opposes change of magnetisation. My ideas are probably very crude, but I look upon a molecule of iron under cyclic changes of magnetism as a straight spring, bent first one way and then the

Captain
Cardew.

other out of its natural position. When you bend there is opposing force; most of the energy is stored up and given out as the spring returns, but some is lost in molecular friction; and my idea of the relative curves of effective exciting power and magnetisation is something like this—

SUGGESTION OF RELATION OF MAGNETISATION TO EXCITING POWER IN TRANSFORMERS.
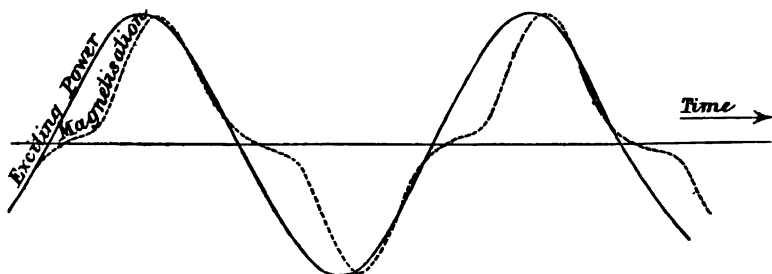
Now, although the molecular resistance, apart from friction, may not expend energy, it may alter the curves, and lags at different points; and as it is the rate of change of magnetism as a whole which affects the secondary current, I don't see how you are to separate the frictional effect from the force opposing magnetisation and helping demagnetisation, which at one part of the curve acts with the friction and with the secondary current to oppose magnetisation, and at another part acts against it.

The risk to life from the use of the alternating transformer system has been the subject of anxious consideration, and, apart from any question of Government interference, it is certain that any serious accident due to the unsuspected presence of a dangerous tension on the secondary circuit would seriously check the progress and development of this system.

The risk cannot be considered chimerical. We have in a transformer two conductors in close contact with each other and with an iron core. They are necessarily thinly, and in many cases practically very imperfectly, insulated. One of these conductors may acquire at times any potential difference up to 2,400 volts from the earth. I need not point out to this meeting that in the primary circuit, taken as a whole, there is certain to be a resultant

leak, and that therefore the potentials of all points in it at any Captain Cardew. time are in a certain definite relation with that of the earth.

The other conductor has also its resultant leak, and this is likely to offer a resistance of not many thousand ohms. We have here what will be recognised, I think, by all who have worked much with cables and condensers, conditions favourable to an eventual breakdown. Seeing the obvious importance of some safeguard, I waited in vain for it to be brought forward, and eventually attempted a solution myself, with the result you see before you. This iron box is a trap laid for the primary volts. This piece of aluminium foil (not platinum, as stated by Mr. Kapp; the lightest, not the heaviest metal) is the bait. You see it consists of two round discs united by a very narrow neck. This is laid flat in a shallow depression in the bottom of the box, which prevents any lateral displacement, and one of the discs is immediately underneath an insulated disc connected through this piece of fine fuse wire to the secondary circuit. The box is connected to earth. The insulated disc can be set to any distance above the foil by this screw, the distance being indicated by this ivory disc on a little brass scale. Should the potential of the secondary circuit ever acquire a difference from the earth exceeding a certain amount, varying with the distance of the charged disc, the foil underneath it will rise up, the other end remaining in contact with the box and earth. It will effect an instantaneous contact. An arc will start only to be immediately suppressed by the fusing of this thin wire, which releases this spring, short-circuits the primary coil, and causes the double-pole safety fuses which must be used in conjunction with it to be at once fused, thus cutting off the transformer completely.

I have here a foil which has been used, and has had on it a potential difference of 2,400 volts. You see it is alive to tell the tale, which is more than you or I would be. The only evidence of ill-treatment it shows is a little piece nicked out just here, where the contact with the insulated disc was made, and here, where the current passed to earth. It was so little damaged that it was used two or three times over; and, curiously enough, without any laborious calculation the distance first tried was exactly

Captain
Cardew.
right for the volts, and everything happened exactly as was
expected. A supply of foils is kept tightly squeezed together
between two brass plates, so that they should be perfectly flat
when taken out. I may mention that thin paper coated with
some sufficiently conducting substance may be substituted for the
metal foils.

Now I do not want to force my apparatus down people's
throats. It is not necessary for the working of the system, nor
does it generally increase its efficiency. It does practically no-
thing until wanted, but it at any rate does not materially increase
the strain on the primary insulation and the chance of a break-
down.

Professor Ayrton has suggested that any safety device is
rendered unnecessary by simply putting the secondary circuit to
earth. This solution is obvious, and I ventured to propose it my-
self when asked my opinion as to what should be done. I was
informed, however, that there was a serious objection to this in
the additional strain to which the primary insulation would be
subjected. The insulation throughout is far from being perfect,
and by earthing the secondary circuit it is practically reduced by
one-half.

Mr. Kapp has described another device with the same object
as mine. I therefore feel a delicacy in criticising it, and will only
say that as a customer I should be quite satisfied with it, unless
I had to provide my own transformers; as a maker of transformers
I should hail it as a boon; but as a supplier of light I should
distinctly object to its use.

Mr.
Bernstein.
Mr. BERNSTEIN: We have, in addition to the very excellent
paper of Mr. Kapp and the interesting remarks of Mr. Mackenzie,
now a paper by Professor Ayrton on the theory of secondary
generators, for which we are very thankful. There is now an
embarras des richesses of interesting questions on which one might
be induced to enter. I shall confine myself, however, to a few
points—those in which I feel most interested. One of these
questions is that of putting transformers either in series or in
parallel. Mr. Kapp has already said that where light has to be
distributed over a large area it would be advantageous to put the

transformers in series. But, in addition to this, I should like to Mr. Bernstein.
call your attention to some other very important facts. In a
system of transformers where we use high-tension alternating
currents, the question of insulation becomes a very serious one.
If you use transformers in parallel you are obliged to have two
main conductors between which there is a very high difference
of potential, say 2,000 volts, all along the line; and you are
obliged to put these conductors near to each other, so as to prevent
induction on other wires. This makes the insulation under these
circumstances exceedingly difficult. When you use transformers
in series, the wires having the same relative position towards each
other, you only get the highest difference of potential at the
dynamo end of your circuit, and you reduce the difference of
potential on the line, so that there is a mean difference of potential
only half as large as the maximum; and, from a theoretical point
of view, one would say that the tendency to produce a leak would
be reduced to one quarter. This is a serious matter in the con-
struction of these cables. But, in addition to this, the series
system offers an advantage of even more importance in the trans-
former itself. In a transformer on a parallel system you may have
a difference of potential of 2,000 volts between the ends of the
primary coil, and, as you cannot use a thick insulation on the wire
of this coil if you want an efficient transformer, there is a great
liability to leakage. In a series transformer this difference of
potential may be reduced to 100 volts or 50 volts, and this
enormously reduces the tendency to leakage. These are two very
important advantages which the series system has over the
parallel system.

There is one question I would like to ask Mr. Kapp. We
have been told repeatedly—although there is hardly any distinct
evidence—that the efficiency of a transformer may be very
high when the maximum amount of power is used, but that
it is reduced considerably when the number of lamps is decreased.
I do not see why it should be so very considerably reduced, but
the statement has been made. I am inclined to think that in
this respect the series transformer will offer an advantage over
the parallel; but this is a matter on which I shall be glad to
have some information from Mr. Kapp in his reply.

Mr.
Bernstein. I shall now turn to another point in the paper, namely, the devices for preventing the current from going from the primary into the secondary. First of all, you will see at once that where there is a tendency to short-circuits in the primary, as there would be with a potential difference of 2,000 volts, there is a tendency to heating. This destroys the insulation materially between the primary and secondary, and then there is the leak. Where this difference of potential is very much reduced the tendency hardly exists. However, we will suppose that it does exist, and consider the devices for preventing the leak itself or its dangerous effect on the secondary. Such a device as Mr. Kent's may work very well, but at the same time may be a source of considerable trouble if proper precautions are not taken, because there is always a considerable resulting leak to earth on the primary. There is almost such a thing as leading an electric current into temptation. Let me quote an example. There are in the United States a very large number of arc light circuits, several miles in length, of overhead wire. After these had been introduced, it was considered advisable to put lightning arresters in the circuits. Careful observation has revealed the curious fact that formerly it was only a rare case that the lightning took its way through the wire, but as soon as the lightning arresters were put on it became a rather frequent occurrence. These lightning arresters, in fact, acted to a great extent as lightning inducers; and the remedy was only found by much increasing the distance between the points, so as to give the lightning current less temptation of selecting the wire for going to earth. Now, in making such devices as that of Mr. Kent, proper precautions must be taken not to give the current too easy a chance to leak from the primary to the metal plate, and from there into the earth. Professor Ayrton has mentioned another device, which consists in connecting the secondary directly to earth. Now, in regard to this, I should like to mention something on behalf of Professor Forbes, who is not present. When I first showed to the members of the Council of this Society, in my laboratory, the contact plug used in my system of series lighting, with which you are familiar, Professor Forbes at once made the

remark that this contact plug would work exceedingly well to Mr.
Bernstein. protect the secondary line, and would be much better than a direct connection to earth. The contact plug would normally have a very high resistance—as much as 5,000 ohms—but as soon as the difference of potential increased, as would be the case in the event of a leak, then it would at once get into action and make a short-circuit.

Mr. PREECE : Will you, Mr. Bernstein, please describe your contact plug ?

Mr. BERNSTEIN : I thought it had been described so often in electrical papers that it might be sufficiently known by this time.

Mr. PREECE : It is so very pretty that it will bear description over and over again.

Mr. BERNSTEIN : I am sure I am very thankful to you. I will endeavour to describe it without making any drawing. The contact plug has been designed in order to prevent interruption of a circuit in which incandescent lamps are placed in series, in case one of these lamps should break. You can at once see that under these circumstances the cut-out device has to offer another path for the current, and it must be placed as a shunt to the lamp. But any device acting as a shunt to the lamp must be of comparatively high resistance, otherwise it would absorb too much of the current. There have been several devices proposed for this purpose, all of them containing an electro-magnet placed in shunt, and therefore wound with fine wire. In the exhibition in Vienna I have myself shown such devices, which seem, however, to have been entirely overlooked, for I have never seen any description of them in the electrical papers. But all such devices are exceedingly cumbersome : they require adjustment, and their application would make series lighting with incandescent lamps perfectly impracticable, because they are too expensive ; and several of the advantages of the system would be counterbalanced by their use. In order to overcome this difficulty, and to do away with all mechanical devices, I have succeeded in perfecting another method, which is realised in the following device. Imagine that we have in a shunt round the lamp two pieces of metal with a substance between them which is normally of

Mr.
Bernstein. high resistance, but which becomes of low resistance if the current through the shunt should increase. This last takes place in case the lamp should break. We therefore want a substance the resistance of which is not independent of the current passing through it, but the resistance of which will decrease if the current increases. In order to obtain such material, I mix an oxide of metal—for instance, oxide of mercury—with some carbon powder. The first is a non-conductor in itself, but by adding carbon powder the specific resistance of the mixture can be regulated to suit requirements. The two metal pieces which I have mentioned before contain a very small amount of this mixture between them, and the resistance is adjusted to be about 200 times that of the lamp. Under normal circumstances only a very small part of the total current passes through this mixture, and this is not affected in any way. But as soon as the lamp breaks the current through the shunt increases very materially, and the increased current causes heating at the point of highest resistance: this is in the mixture. The result is that the mixture is decomposed, metallic mercury is formed, and a by-pass of practically no resistance is established around the lamp, thereby preventing any break in the circuit.

This action takes place very quickly, and does not in any way affect the light in any of the other lamps.

These cut-outs, which are generally made in the form of a plug, but can be made in a variety of ways, have now been in operation in a great number of installations for over a year, and I can state to you as a fact that we have never known a case in which the plug has failed to do its duty.

Now, gentlemen, I have made a diversion from what I was going to say, and as it is rather late I think I shall somewhat shorten my remarks. I only wish to say in regard to Mr. Mackenzie's paper that I fully agree with one of the observations he made, and that is, that we should never put electric light fittings on to gas fittings, especially in a system on which high-tension currents are used. It is an enormous mistake to do so, and generally leads to fault. If my information is correct, it is distinctly prohibited in the United States wherever high-tension

currents come into play, and I hear from some of the gentlemen Mr.
Bernstein. connected with the Edison Company that even on their low-tension circuits they do not like it. It is altogether a bad practice, resorted to to make things cheap, but it ought to be abandoned.

Mr. ATKINSON: The few remarks I shall make this evening Mr.
Atkinson. will have reference principally to that part of Mr. Mackenzie's paper which deals with alternate-current machines. I agree with him that the greatest improvement has been the introduction of iron into the armature. He, however, at once passes on to give as his latest type of machine the Ferranti, which, I believe, contains no iron in the armature; and this seems to be rather a contradiction. Although the introduction of iron is a great improvement, it is very essential not to saturate the core to the same degree as is customary in direct-current machines. The builders of multipolar disc dynamos have found by experiment that with more than six poles there is great difficulty in keeping down the heating of the iron in the armature, due to the more rapid change of magnetisation. I ran out roughly a calculation to see how many lines of force per square inch we might take in the armature core of an alternate-current machine having a period of $\frac{1}{80}$th second without danger of heating from magnetic hysteresis. As a basis, I assumed that in a six-pole continuous-current machine running at 600 revolutions the greatest density is about fourteen lines to the square inch, taking Mr. Kapp's system of measurement. I believe this is about the maximum they can work at when of that number of poles and speed. Assuming that there be no greater hysteresis in the armature of the alternate-current machine, you could not use more than about nine lines to the square inch to produce the same amount of heat, and this points at once to a considerable reduction in the intensity of the magnetisation of alternate-current machines. In the machine which Mr. Mackenzie has described and illustrated he places on the magnet cores alternate coils, one set forming the exciting circuit, and the other set the working circuit in which the alternating currents are generated. Now it appears to me that in this arrangement there would be

Mr.
Atkinson.
produced equally in both of these coils an alternating electro-
motive force, and that when the machine is excited by a con-
tinuous-current dynamo there would be produced rapid fluctuations
in the strength of the exciting current.   Generally speaking,
continuous-current dynamos are not constructed to stand that,
and it would produce a rather curious effect.   I do not know
whether he has had any experience in that respect.   I have
brought here a photograph of an alternate-current dynamo
designed by Mr. Kapp and constructed by Messrs. Goolden.
This has also iron in the armature.   It has an output of 30
ampères at 2,000 volts, and weighs 2½ tons.   The armature con-
sists of a rim of steel or cast iron, on which is wound iron tape.
On the outside of that is wound iron wire, because the poles come
over the top, and it is therefore necessary to laminate in all
directions.   On the core thus built up are secured driving spurs to
hold the copper wire.   The armature is 40 inches in diameter, and
there are sixteen pairs of magnets.   The speed is 600 revolutions.
The arrangement of driving spurs for holding the wire in position
is mechanically good.   When running at 1,500 volts, and at full
speed, the terminals were accidentally short-circuited, which
pulled up the engine; but there was no damage done to the
armature, and no wires were shifted in the slightest degree.   The
machine of Mr. Kennedy has the advantage that you can attach
these " inductors " direct to the fly-wheel of his engine.   I do not
know what his experience of engine builders may be.   I have
always found them very unwilling to alter any part of their
engines, and I doubt whether they will agree to substitute his
inductors for the ordinary fly-wheel.

The author appears to attach great importance to the possibility
of coupling the armature circuit in various ways.   I do not think
that anyone will attempt to alter the coupling of a machine of
that kind giving 2,000 volts whilst running; and, generally
speaking, it is only for experimental purposes that such an
arrangement is required.   In practical work we know beforehand
what pressure and current we must employ, and we build our
machines accordingly.

The only other point I would refer to is the remark of Professor

Ayrton on the compound winding of transformers. He admits _{Mr. Atkinson.} that he has not gone fully into the question. I have devoted some time to its consideration, and have come to the conclusion that somewhere or other there is a fallacy in it. I cannot see how the result is to be produced. In his diagram he shows a coil placed in parallel with the main coil, and draws the analogy with a compound-wound dynamo. Now it seems to me that we cannot raise the volts on the terminals by putting one coil in parallel with another; for, if that coil will produce two volts for every volt of the other coil, it will at once begin to work back through the other coil. I am inclined to think that compound winding of transformers cannot be done.

Professor AYRTON: Mr. Atkinson is mistaken in his idea that _{Professor Ayrton.} two generators put in parallel cannot raise the volts at the terminals without the added one having a higher E.M.F. than the one previously sending the current. Of course it is the simplest experiment in the world. Here is a cell sending a current through a wire and maintaining a certain P.D. at the terminals of the wire. Now, by putting two cells in parallel, you can raise the P.D. at the terminals of the wire. The E.M.F.'s of these two cells may be the same; or, for the matter of that, the E.M.F. of the added generator may even have a lower E.M.F. than the one previously sending the currents, provided that the resistance of the wire be low.

I do not say that this in any way proves that our proposed system of compounding transformers for use in series will work satisfactorily; all I say is that the illustration that I have given disposes of Mr. Atkinson's theoretical objection to the proposed system of compounding transformers.

Mr. ATKINSON: That result is only produced by reducing the _{Mr. Atkinson.} output of the original cell—that is to say, practically substituting the second cell for the original one. If the original cell is still doing the same work the potential difference will remain the same as before.

Dr. FLEMING: I should like, in a very few words, just to _{Dr. Fleming.} describe some experiments I made in 1885 on a couple of Zipernowski transformers which were brought over and exhibited

Dr.
Fleming.
at the "Inventions" Exhibition. In that year Mr. Zipernowski brought over two of his well-known life-buoy-shaped transformers and placed them in the "Inventions" Exhibition. We took the opportunity to make some experiments on them. I may mention that they were wound with the intention of being arranged in parallel. They were worked from a self-exciting alternating-current machine at some distance away; and in order to give an opportunity of seeing how the method adapted itself to the transmission of electric energy over a great distance by means of small conductors, we placed the alternating machine as far from the transformers as the conditions would permit, and I believe these two transformers were placed 1,000 feet away from the current-generating machine. Each was intended to work 200 16-candle lamps, and, if I recollect rightly, they were connected by No. 10 wires with the machine. I may mention that the primary current was operated by an E.M.F. of about 1,000 volts. At that time Dr. Hopkinson had just drawn attention to some possible danger that might result from the electrostatic capacity of transformers, and it occurred to me that this might be obviated, or diminished, by leading the secondary circuit to the earth. In these transformers, exhibited in 1885, the secondary circuit was connected to an earth-plate. After this, Professor Elihu Thompson, of America, wrote to one of the technical journals, and stated that he had previously suggested that method; and thereupon, I believe, Professors Ayrton and Perry wrote also to the same journal that they had suggested it before Professor Elihu Thompson ; and thus the matter rested.

I may also mention as an interesting fact in connection with these transformers that Mr. Zipernowski had arranged a method of self-regulation, the exact details of which will be found in one of his specifications of the year 1885. The principle of that method was the use of an additional transformer, so arranged that when additional lamp load was thrown on the transformer the terminal volts of the secondary circuit were kept up by this additional transformer to the normal amount.

Professor AYRTON: I said Gaulard & Gibbs, but I think it was Zipernowski. Did the plan answer?

Dr. FLEMING : Fairly satisfactorily. It certainly did effect an improvement. Then our attention was naturally directed to some method of determining the efficiency of these transformers. Mr. Zipernowski had brought over with him a couple of wattmeters for this purpose, and the intention in designing them was to get rid, as far as possible, of the self-induction of the fine-wire circuit. For that purpose the movable frames were large, and the whole of the fine wire was not put into the movable frames; it was put partly outside the movable frame in the form of a non-inductive resistance in series with it. If that was the frame of the watt-meter, then (without drawing the connections) there was a non-inductive resistance in series with it, which formed the bulk of the high resistance, and, as in the Siemens wattmeter, there was no mutual induction between the fine-wire and the thick-wire circuits. Provided with a couple of these instruments, one placed in the primary circuit and the other in the secondary, we made some measurements which we hoped would give us something approximating at full load to the efficiency of the transformers. Then, taking the respective readings of these wattmeters, we drew a curve in which the horizontal ordinates were the power as measured in the external or lamp circuit in watts, and in which the vertical ordinates were the ratio of the readings of these two wattmeters expressed as percentages of the lowest-reading watt-meter, namely, that of the secondary circuit. These figures were plotted down, and showed a curve gradually rising upwards and finally getting nearly flat at the extremity, and the highest ordinate indicating about 92 per cent. efficiency. It is obvious that these figures cannot be correct, but it would now be interest-ing with the apparatus referred to by Professor Ayrton to ascertain how far the readings taken by a wattmeter built on this plan differ from the real efficiency of the transformer.

It is very important that attention should be paid, in con-nection with transformers, to the question of the magnetic lag. There is no question that magnetic lag is a reality, and that there is a lagging behind in the time at which iron reaches its maximum magnetisation as compared with that in which the magnetising force comes to a maximum. It follows from this

Dr.
Fleming.
that we shall have to modify the method of treating the magnetic
resistance of magnetic circuits to assimilate it with the method
which we have to adopt in considering the resistance of electrical
circuits when dealing with alternating electro-motive force. We
know perfectly well that we do not obtain the average current in
a circuit working under an alternating impressed electro-motive
force by dividing the average value of impressed electro-motive
force by the resistance of that circuit, but that you have to
substitute for the resistance that quality which has been called
by Mr. Oliver Heaviside the "impedance."   The impedance is that
quantity which takes into account the quality of the conductor
in virtue of which there is a lagging behind of current compared
with E.M.F.; and when we are dealing with magnetisation it would
follow from this that in the case of rapidly reversed magnetisation
we must substitute for magnetic resistance a quality which might
be called the "magnetic impedance" of the magnetic circuit.
The undetermined quantity in all the theoretical investigations
at present published on the subject of transformers is this
magnetic impedance.   Special experiments should be made in
order to ascertain what are the values of this magnetic resistance
corresponding to the same magnetising forces operating at
different periodicities.   The experiments I should like to see
carried out on the subject would be directed to obtain this
magnetic impedance or resistance with alternating magnetising
forces working with the same average magnetising force, but
operating at different periodicities.

With regard to the question of safety, it cannot be doubted
that in course of time very many different devices will be
designed for obtaining security and preventing the E.M.F. of
the primary circuit getting into the secondary circuit.   It would
no doubt be possible to arrange something on this plan that
should be tolerably simple and yet work effectively.   If this is
the secondary circuit, and these mercury cups interrupting the
secondary circuit, then, if these cups are connected by two bars
fixed to an insulated support pulled up by a spring and held
down by what practically would be a fine-wire safety catch in
connection with the two opposite sides of the circuit, that safety

catch may be so arranged as to stand the normal difference of ^{Dr.}
_{Fleming.}
potential between these two mains, but as soon as that rises
beyond a certain amount the circuit would be automatically
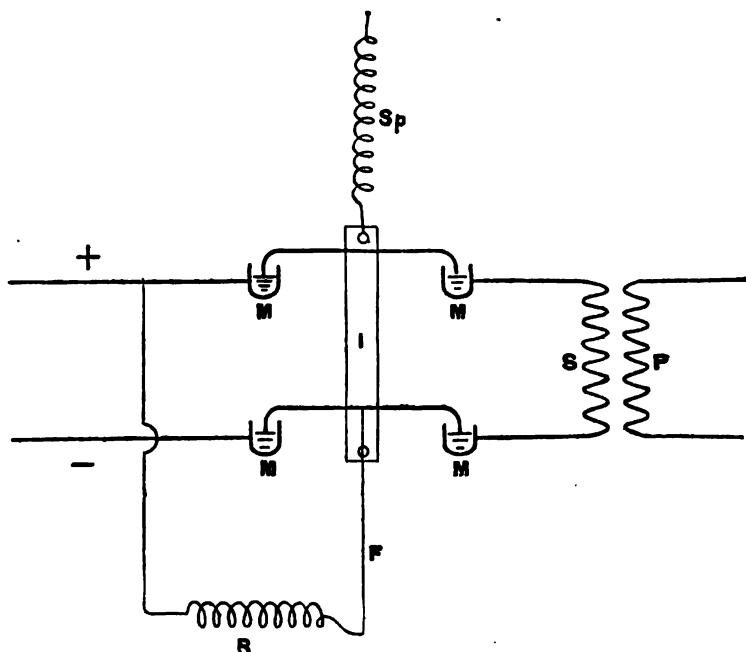
TRANSFORMER CUT-OUT.

M, M, mercury cups; I, insulating distance piece; R, resistance coil;
F, fine fusible wire; P, S, primary and secondary of transformer; Sp, spring.

broken.   Say that were set to stand 200 volts : then it would give
way if the difference of potential were to rise to, say, 400, and the
circuit would be broken automatically.

Professor AYRTON : Might I remind you that the wattmeter we ^{Professor}
_{Ayrton.}
used gave an error of 100 per cent. when there was a very small
current in the secondary circuit ?   The greater part of the
resistance of the fine-wire circuit was non-inductive, and was
placed in a separate resistance-box.   The suspended coil was fairly
large, and contained comparatively few convolutions.   From what
Dr. Fleming says, I think that the instrument must have been
made by Mr. Zipernowski.   I bought it second-hand, and be-
yond that I knew from its appearance that it was of foreign

Professor Ayrton.

construction I was quite ignorant as to where it came from; but from what Dr. Fleming has said I think it must be one of Mr. Zipernowski's, and similar in construction to the wattmeter Dr. Fleming himself employed.

Mr. Crompton.

Mr. CROMPTON: I have a good deal to say on this subject, but not from a mathematical point of view. I propose rather to deal with the question from a commercial standpoint, and in order to do this I propose criticising Mr. Mackenzie's paper on one or two important points, and should feel obliged if he would come prepared to answer me. I shall be glad to hear from him what is the total actual efficiency of the system of engines, dynamos, conductors, and transformers. We have heard a great deal about the efficiency of transformers taken by themselves, but not sufficient as to the total efficiency of the whole system. I also wish to hear whether the high efficiency claimed for transformers is maintained when they work at a low rate of output.

Mr. Preece.

Mr. PREECE: I adjourn this meeting until next Thursday, when this discussion will be renewed. There are several gentlemen present who are eager to speak, but I have held them back in order that we may have a continuation of this discussion next week.

The meeting then adjourned till Thursday, the 23rd February, 1888.

The One Hundred and Seventy-fourth Ordinary General Meeting
of the Society was held at the Institution of Civil Engineers,
25, Great George Street, Westminster, on Thursday, February
23rd, 1888—Mr. EDWARD GRAVES, President, in the Chair.

The minutes of the Extraordinary General Meeting held on
February 16th were read and approved.

The names of new candidates were announced and ordered to
be suspended.

The following transfers were announced as having been
approved by the Council:—

From the class of Associates to that of Members—

       William Henry Davies, F.R.G.S., F.S.S.
       Captain Henry Capel Holden, R.A.
       John Rymer-Jones.
       Frederick Ward.

Donations to the Library were announced as having been
received since the last meeting from Sir Frederick Abel, C.B.,
F.R.S., Past-President; W. Anderson, Esq.; the Director-General
of Telegraphs in India; Dr. W. Jacques, Foreign Member;
Herbert Tomlinson, Esq.; and Messrs. E. & F. N. Spon; to all
of whom the thanks of the meeting were unanimously voted.

The adjourned discussion upon the following papers was then
resumed:—" On Alternate-current Transformers, with Special
" Reference to the Best Proportion between Iron and Copper,"
by Gisbert Kapp, A.M. Inst. C.E., Member; " The Distribution
" of Electricity by Transformers," by J. Kenneth D. Mackenzie,
A.M. Inst. C.E., Member; " Formulæ for Converters," by Professor
George Forbes, F.R.SS. (L. & E.), Member.

The PRESIDENT: At the suggestion of Professor Ayrton, I will ᴛʜᴇ
ᴘʀᴇsɪᴅᴇɴᴛ.
ask Mr. C. G. Lamb, of the Central Institution of the City Guilds,
to explain more fully the diagrams which Professor Ayrton briefly
referred to at the last meeting.

Mr Lamb.   Mr. C. G. LAMB, B.Sc., gave a more detailed description
of Professor Ayrton's experiments and the diagrams which
exhibited the apparatus and results.   The transformer was
placed in a calorimeter; in the primary circuit was a dyna-
mometer, a new Ayrton and Perry voltmeter, and a wattmeter;
in the secondary there was a similar dynamometer and a Cardew
voltmeter as described by Professor Ayrton.   The wattmeter
was like that mentioned by Dr. Fleming, having an induction-
less high resistance in series with the shunt coils.   Each
number in the table was the mean of several observations.
From it, it appeared that when the volts in the primary circuit
were kept constant the volts in the secondary remained constant,
the variation being about as much as in an ordinary compound
dynamo.   The primary current was increased from ·8 ampère
to 16 ampères, the first being when the secondary was on open
circuit, and the last when it was giving out about four horse-
power: this rise of primary current is due to the decrease in
the practical self-induction.   The efficiency curves showed how
quickly it rose as the secondary power was increased, till at
the full working power of the transformer it was over 96 per
cent.   Though the efficiency was low at low powers, the actual
loss was of small importance.

Mr. Gordon.   Mr. J. E. H. GORDON, B.A.: I have listened with the greatest
interest to the very able papers that we have been favoured with
at the meeting before last, and to the very interesting discussion
that has followed upon them.   I think nothing is more wonderful
than the extraordinary ingenuity which has been lavished on
these transformers during the last few years, and the extra-
ordinary development in working from the modified Ruhmkorff
coils that we used to experiment with many years ago.   But
while fully appreciating the scientific interest of these things,
and fully appreciating the great practical use that has been made
of them, I cannot help having a feeling that it is a pity to see so
much ingenuity, so much skill, and so much labour lavished on
what, in my opinion—possibly I may be wrong, but really the
feeling seemed to come from the speakers themselves—can only
be intended for a makeshift, enabling us to go on until something

better is obtained—that is, a perfect storage system. It seems a Mr. Gordon.
pity to spend time and money in improving the details of that
which is not to go on in the future. If I may be pardoned in
putting into a scientific discussion a word almost of auto-
biography, I would say that I am not altogether incompetent to
speak on the development of alternate-current machines, for I
suppose few people have been so deeply committed to them as I
have been, or have had to deal in actual practice with such large
ones ; and yet I lately, even at the expense of abandoning every
patent that I hold, have been so far convinced of the superiority
of the method of distribution given by storage batteries that I
felt compelled to abandon what was identified as my own system.

We heard with much interest about 42-feet machines driven by
5,000-horse-power engines. It is not quite a new thing to think
of such machines, although it is a new thing to give the order for
one. I made calculations for one, and went as far as getting out
working drawings two or three years ago, and was much tempted to
go on with it. It would be delightful to see such a machine running,
and to watch the movement of such a mass of machinery going
round ; but there is an old French expression, " *C'est magnifique*
" *mais ce n'est pas la guerre.*" The delight would be more felt, I
imagine, by the engineers than by the shareholders. I am not
speaking of the abandonment of alternating-current machines as
if they had been failures, or as if they had not worked economically.
Our Paddington installation has worked now for two years, and in
one year we supplied 838,000 units of electricity : I am not going
to speak of the price they were supplied at, but it was an extremely
low price, and one we should not shrink from, and it is only from
reasons which I will give you in a moment (which I hope may con-
vince you) that I think the other thing is better. It is not
because alternate-current machines are bad, but because I think
something else is better, that I determined to abandon them. The
first consideration is the space that is occupied by these machines.
We know that experiments have been made by coupling a number
of small alternate-current machines together, and at the South
Foreland they were successful, but that was because they were
working on arc lamps. Many of us have tried them, and they

Mr. V.
Gordon.

will, on trial, work together, no doubt, but they do not work to-
gether till they have run for three or four minutes; they will in
that time jump, and that jumping will take months of life out of
the 40,000 lamps.   That alone is rather a serious difficulty in
coupling machines together, and I think we may take it, in prac-
tice—I am not speaking about the laboratory or experiments—we
do not couple machines; and we must have very big machines if
we are to have alternating-current machines to do big work. These
take up a considerable amount of space. I do not think that 10 feet
is an out-of-the-way diameter for a machine to give 200 to 250
units per hour, which is about what the Paddington machines are
doing.   Mr. Crompton is making me machines for 100 units with
an armature 16 inches diameter, so that two or three of these on
one set do not occupy so much space as one of our alternating-
current machines, or anything like so much. You must, with alter-
nating-current machines, practically put them outside your town,
and bring your wires and your mains in.   But with the modern
direct-current machines, and with modern engines such as my friend
Mr. Willans is making for us, you can pack your machinery so
close that you can get an immense power into a small space and
none of your neighbours be any the wiser.   We are at this moment
putting in plant for 10,000 lights, but with room in the engine-
house to extend to 20,000 lights, not a quarter of a mile from
this room. The engine-house is 86 feet by 40 feet, which includes
engines, boilers, batteries, and everything else. No doubt it
may be remarked that 20,000 lights only represent a drop in the
bucket of London lighting, and we have to face the question as to
whether we are going to establish very large stations, or be
content with stations of about 20,000 lights. We all know that
one of the great advantages of central station lighting is the great
economy in management and working that is obtained by in-
creasing the number of lights worked from one station. But,
gentlemen, that increase does not go on indefinitely. If you have
about 500 lights your working cost for wages will, roughly, be
about 10s. per lamp per year.   When you go up to 10,000 lights
that average is decreased to about 1s. 2d. per light per year; but
after 20,000 lights you do not get any further decrease of the

average wages per lamp, because then more machines and men are Mr. Gordon.
required. If you have a number of stations working from 10,000
to 20,000 lamps, each with a direct current, there is the advantage
that the stations can be coupled together, one working at one end
of the main and the other at the other, so that if anything goes
wrong at one end the other can help it out. There are certain
definite reasons which I think will always make a number of
small direct-current machines more economical in working than
any big machine, and I will go briefly into them. The first
is a reason which I can explain better by a diagram that I
have on the wall here (Fig. 1). The demand for light is of

WIRED FOR 20,000 LAMPS

15000 LAMPS

LAMPS

DEMAND

7500 „

4550 „

12 HOURS SUPPLY

2275 „

24 HOURS SUPPLY

A.M 6 7 8 9 10 11 12 1 2 3 4 5 6 P.M 7 8 9 10 11 12 1 2 3 4 5 6 A.M
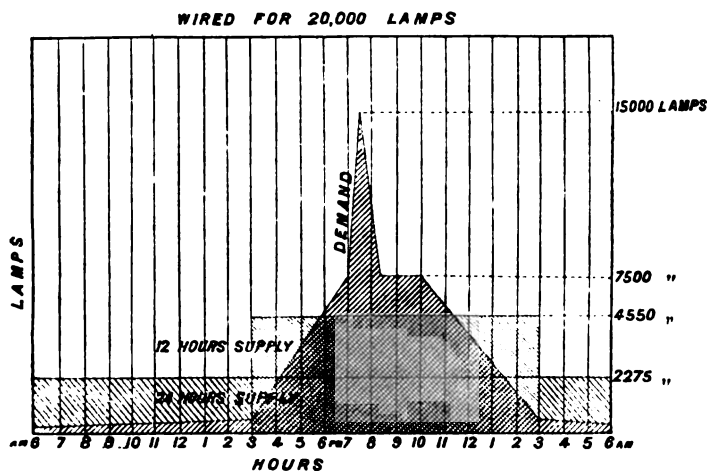
HOURS

Fig. 1.

course, as you know, not constant : it is very small at certain
hours and very large at other hours ; the maximum demand is,
in fact, very much greater than the average demand. We have
here a curve for a purely residential district, taken from actual
figures. Hotel districts and club districts vary. The vertical
ordinates show the number of lamps on while working. The in-
stallation is wired for 20,000 lamps. The number of lamps going
is small till about 3 o'clock, when it gets dusk on a winter after-
noon ; then it rises steadily and regularly till about 7 o'clock, when
it goes up with a rush (I will explain the reason for this in a
moment) ; then it goes down, and remains pretty nearly constant

Mr.
Gordon.

from about 8.30 p.m. to about 10 p.m., which is the time a good many people go to bed—the first batch. Then it sinks gradually—you see there is apparently a great line of demarcation of virtue—about 10 o'clock; people who sit up after 10 o'clock seem to get reckless of hours and morality, and drop off one by one to bed until about 2 in the morning: then the number gets less again.   Now the reason for the great rise and fall between 7 and 8 o'clock is that at that hour people go to dress for dinner, and at the same time the servants are busy preparing the dining rooms, and consequently bedroom and dining room lights are required together.

We must further note that, whatever the percentage loss is in the storage batteries, *it has only to be calculated on the part of the supply and demand curves, which do not overlap, not on the whole area;* and this is a very different matter.

Well, assume we work such a district with alternating–current machines. We have 20,000 lamps wired.   In order that there may not be a breakdown at the busiest time, the machines must be powerful enough for 15,000 lamps, which means that engines, boilers, dynamos, and engine-house must all be arranged for that size.   We supply by meter; and if the supply be by meter or by fixed contracts based upon meter, a great portion of the machine is only earning revenue for about $1\frac{1}{2}$ hours in the 24.   Half the power of the machine is wasted for 12 hours out of the 24, but the interest on the cost of that machine is going on for the whole 24 hours.   Now look what this means.   If you have a storage, and work the engines for 12 hours, the maximum current they have to furnish can only be that for 4,550 lamps; and therefore for 20,000 lamps, with occasionally 15,000 lamps on at once, it is unnecessary to have machinery of a greater capacity than 4,550.   If, however, you desire to work your engines for 24 hours, which may in some circumstances be advisable, you must halve that: you then have something extremely small.   I suppose, in practice, what we shall do in designing installations will be to so arrange that the engines can be worked for 12 hours, and then as custom comes in the engine power can be increased, or the battery power increased and the

engines worked for longer hours. This point only deals with the
capital cost of the plant. The next point is the efficiency of the
dynamo. Now, as Mr. Mackenzie knows—perhaps better than I
do—and as we all know, the efficiency of an alternating-current
dynamo is quite different when it is fully loaded to when it is
half loaded. If an alternating-current dynamo is used that will
give 4,000 electrical horse-power, and give a very good efficiency,
say 85 or 90 per cent.,—whatever it may be, such efficiency will
not be got, or anything like it, when the dynamo is working at
half, or a third, or even a quarter of its power. There will be a
very wasteful action in the dynamo, and not only wasteful, but
a mischievous action. But there is a much more important
point than that. Alternating-current machines, when used
direct, or even more so when they are used with transformers,
require for their successful use that the number of alterna-
tions per second shall be sensibly constant: the speed of the
engines or dynamos must not be greatly varied; the engine must
run at very nearly the same speed whether worked at full power or
only a fraction of that power. We are "electrical engineers" now,
and not entirely electricians, so I will ask the engineers present
what the diagram of a "quadruple expansion 5,000-horse-power
"engine" will be when working at 1,500 horse-power, and at
full speed—perhaps two cylinders will be working one way and
pulling the other two along. The diagram would be a very
peculiar thing. But, putting the diagram aside for a moment,
experience perfectly shows us that a compound engine working at
full speed at a third of its power burns at least twice as much
coal per horse-power as when working at its full power. With
these smaller units that we use for storage, where we can easily
couple them together, or even with larger machines and with
storage, we work our engines at their full power until we get as
much as we want out of them, and stop. There is no working at
half power: it is either full power or stop ; and therefore we are
able to get a far greater economy than we can by working big
compound engines under their power. There is still another
reason, which I said I would allude to, for having scattered
stations placed where the engines will not be a nuisance. London

Mr.
Gordon. is now being filled with great buildings, residential flats, great hotels, and buildings of that sort; and the best site for a station is somewhere near one of these, where, on account of special demands, we can, for the first time, meet the gas companies on their own ground, *and utilise our waste products.* In our Whitehall installation we are selling every morsel of waste steam that we are generating, and we are not sending any fraction of it up the chimney, because we have a contract to supply the whole of the hot water required for warming the corridors and for the baths in the building. I do not think others who supply their current from a distance could dispose of their waste steam in the same way. I hope Mr. Mackenzie will not think that I am trying to disparage the excellent work which his company are doing, because there is plenty of room for all of us; but we must remember certain points. Statistics of the lighting in London are not very trustworthy, but, as far as I can get them, they show that there are 125,000 gas jets to a square of a mile. Now we are all friendly rivals, and whoever of us proves not to be successful will have the pleasure of educating the public up to the use of the electric light and getting them to wire their houses, and then later on they can adopt the most successful system. But there is no rivalry here among ourselves between these two systems of distribution; these are two honest systems, both endeavouring to do their work as in their knowledge is best. There is another system which perhaps should be referred to. We have heard a good deal lately about a system for distributing electricity which I think none of us will wish to assist to develop more than we can help—that is, the system of distributing electricity from house to house by means of circulars in halfpenny wrappers.

Mr.
Crompton. Mr. R. E. CROMPTON: My friend Mr. Gordon has, I fear, rather wandered from a strictly scientific discussion of Mr. Kapp's paper. Perhaps you will check me if I wander too far in the same direction. There is some temptation to do so, as this subject of transformers, viewed from the purely scientific point, as Mr. Kapp has viewed it, is somewhat of a dry nature; and I propose to enliven it by entering on the vexed question which I believe will be the

question of the future—whether transforming currents of high Mr.
Crompton. E.M.F. to low E.M.F. can, after all, be done best by means of the alternating-current systems now under discussion, or by means of batteries of accumulators used as transformers.

With this preamble, I address myself to Mr. Mackenzie's paper. I consider that the questions he has raised at the present moment are of supreme importance. The company that he has been more particularly connected with (the Grosvenor Gallery Company) have done a great work, and one for which everyone in the electrical profession must thank them most heartily. They have educated the people of London to like the electric light. But the failures which they have made, although of great assistance to my present argument (as I believe them to be inherent to the alternating transformer system), in no way detract from the value of the services rendered by that pioneer company; for they have shown to a large circle of customers that the advantages and conveniences of the electric light are so great that the users can afford to put up with failures and interruptions which, although very unpleasant, have not apparently diminished the popularity of the light. Mr. Mackenzie has explained some of the causes of these failures, which I do not propose to severely criticise. He has told us that with the alternating transformer system, as at present arranged, each user is to a certain extent dependent on what his neighbour does: any accident to his neighbour's transformers affects his lights, although the machinery at the central station may be in perfect order. This great defect must of itself necessarily increase the percentage of the failures of a large system; and it is a defect from which the direct-current system worked from accumulators is absolutely free. I have been long working at the development of the latter system. As most of you know, I have carried out several central stations in which currents at 400 volts have been transformed down to currents at 100 volts to suit the lamps usually employed in incandescent lighting, the transformation being effected by mains carried from the terminals of four groups of accumulators coupled up so as to be charged in series, the charging and discharging being carried on simultaneously. I have very full data in my possession taken from these installations, and which are

Mr.
Cromptor.

of considerable service to the present discussion. I do not propose
here to introduce questions of the cost of management of the two
systems, but to confine myself to discussing the comparative cost
of generating and distributing—that is, so far as cost of fuel,
labour, first cost, and subsequent upkeep of plant employed;
comparing also the steadiness, regularity, and freedom from break-
downs inherent to each system. Before I commence on this I wish to
point out the enormous influence Mr. Westinghouse has had in
the development of alternating-current transformers. He has
done for them what Mr. Brush did for arc lighting. You must
all of you recollect how Mr. Brush in 1880 astonished us all by
sending over well-considered sets of arc lighting plant, these
sets being so complete in every detail that they could be set to
work at a few hours' notice by unskilled workmen. By these simple
means he attracted public attention to arc lighting, and to such
an extent that it led to the disastrous speculation in electric com-
panies. People did not then stop to consider how large a share
of the first success of Mr. Brush's machinery was due to excellence
of system, and how much was due to the completeness of detail
which I have above referred to. At the present time I cannot
help thinking that people are being similarly led away to consider
that the alternating transformer is a complete solution of the dis-
tribution of the electric light. They hear that Westinghouse has
his shop full of orders, and that enormous numbers of lights on
this system are being supplied in the United States. Mr.
Westinghouse is an extraordinary man. He is one of the best
organisers of machine labour in the world; further, he has extra-
ordinary powers of pushing a new thing, as witness his celebrated
brake. I am told that the design and workmanship of the plant
that he turns out at his works are perfection. He adopts the same
system that Mr. Brush did of carrying out every detail of the
work in the shops in a most complete manner. This makes it
evidently fitted for American requirements, where, as you know, the
public is content with the roughest style of fixing, such as would
not pass muster in this country on a most second-rate installa-
tion. I admit at once that the alternating transformer system
carried out in this manner has a large field for its employment in

such countries as America, or our Colonies, or, in fact, for any new Mr.
Crompton. country where wires can be run overhead, and where temporary interruptions of the light are of secondary importance ; but I do dispute the suitability of the system for the lighting of large cities such as London.

As I have already said, the system has served us a good turn as a pioneer system. It enabled a station to be put down from which overhead wires could be run in any direction, and thus a number of isolated customers could be supplied who otherwise would never have even tested the electric light. I see both Mr. Mackenzie and Mr. Kapp virtually admit that when a large proportion of the houses are lighted a network of mains must be laid down in the streets. I wish to know in what way these mains can be laid down cheaper for the alternating system than for the other. We know that a network of distributing conductors can be laid down in the streets at a cost of from 14s. for the smallest useful size of conductors up to 18s. as the largest size necessary for 100-volt direct distribution. When people talk of copper as if copper were the only thing that has to do with the cost of distribution, they are talking nonsense. This is not the place for me to go into long comparative estimates for the two systems. This I am prepared to do at another time. It is sufficient to say that I have now figures in my possession by means of which I can prove that the capital cost of the two systems of distribution is nearly identical. I do not take the extreme figures taken by Mr. Mackenzie, who talks of the distribution of 1,500 ampères at 100 volts as if this were the only method possible for systems other than the alternating transformer one ; but I take the 500 volts we actually use at Vienna. I find, then, that so long as the generating station is not situated more than one or two miles distant from the centre of the district to be supplied, the capital cost of the two systems is approximately the same. There is no saving in using the alternating transformer system. Next, I propose to attack the alternating system as an efficient one. We have heard a great deal about the efficiency of these transformers, and there has been a fight for a few places of decimals. They have shown very high efficiency at full loads, but I do not believe

Mr.
Crompton.
that these efficiencies can be maintained at small loads. For London lighting the transformers used would have to be large enough to work up to the peak of the diagram shown by Mr. Gordon; but when working in the daytime the load would be so small that efficiency would drop to 20 per cent. Further than this, these transformers are worked by very large dynamos. Mr. Mackenzie talks about 5,000-horse-power dynamos, 42 feet in diameter; and as we are elsewhere told that the full capacity of the station is to be 200,000 lights, it appears as if the whole work of the station could be divided over four dynamos. I should like to know how many tons of coal per lamp these dynamos will use if they are used for working a few lamps in the daytime. I have made many experiments in connection with the efficiency of engines and dynamos at low loads, and I know that there is not a more extravagant machine on the face of the earth than a compound or triple expansion engine worked at less than a third of its full output. As Mr. Gordon has said, the low-pressure cylinders are mere drags on the engine when it is lightly loaded. I think, if you ask Mr. Willans, he will tell you this much better than I can. I put it to you in this way. These big engines and dynamos use nearly half of the coal when nearly empty that they do when they are running full load; that is to say, if they are using 20 tons a day when fully employed, they will take 10 when they have only one lamp on. Everybody who has worked central stations knows these figures. Mr. Mackenzie could corroborate them if he chose by the figures from the Grosvenor Gallery. I can tell you that they are now burning five times as much coal per lamp at the Grosvenor Gallery as they are burning at Kensington or Vienna, and solely for the reasons I have given above—that is, that they have to run a big engine long hours on light loads; whereas when we are using accumulators we can run our engines at their best efficiencies. If Sir Coutts Lindsay had asked me to supply him with a battery system two years ago, he would have saved enough money on his coal bill alone to have paid for two new sets of batteries. The diagram shown by Mr. Gordon is almost an exact copy of a series of diagrams which Dr. Fleming has shown to me for the first time this

evening. They come from the Edison Company in America, and Mr. Crompton. refer to various installations. All these diagrams point out that there are peaks of maximum lighting which lasts for a little over an hour, and during which the demand is nearly double that of the mean demand during lighting hours, or something like ten or twelve times the average demand for the 24 hours. I believe the whole pith of the economical question lies in the consideration of load diagrams of this kind. A great deal turns on the question that has been raised by Mr. Mackenzie whether it is possible to run alternating machines in parallel. If this could be done in practice, it would knock the bottom out of my arguments as far as they apply to the want of economy of using large engines for light loads; but I have taken great pains to ascertain whether such parallel running has been actually carried out in practice. It has been done in England and America, but not otherwise than experimentally. Until alternating-current dynamos can be used as continuous-current dynamos are now used—being switched on to the circuit as the load rises—they can never approach the economy in working which can be obtained from direct-current dynamos, more particularly if the latter are used in combination with accumulators. I think that Mr. Ferranti has practically admitted the impossibility of putting these machines in parallel by designing the huge machines above mentioned for his latest station.

Mr. W. B. Esson : Speaking after a most energetic repre- Mr. Esson. sentative of the anti-transformer party, I wish to express my indebtedness to Mr. Kapp for the most interesting and instructive paper he has brought before the Society.

In dealing with this subject Mr. Kapp has introduced graphic methods, which are always of great value to the engineer, inasmuch as he can see at a glance exactly what is taking place, and is able also to comprehend better the modifications in results due to alterations in the proportions of the apparatus. Through-out his investigations, however, Mr. Kapp assumes that the E.M.F. impressed on the primary coils, or, rather, the E.M.F. generated by the dynamo, is a sine function of the time; but surely in modern machines with iron cores in their

Mr. Esson. armatures this is not quite the case. I can believe that in a Siemens dynamo the curve would be a sine function; also in the Ferranti dynamo the deviation might not be great; but in a dynamo having iron in its armature I should say decidedly that it would not be a sine function, nor would it be anything approximating a sine function. I have endeavoured to map out the magnetic field in a dynamo such as Mr. Kapp's with a disc armature having poles on each side, and the results are shown by the diagram on the wall, where A is a curve of sines, and B the sort of curve you get from the machine. There is a flat part in the middle. That part may be a little too long in the diagram, but its length depends entirely upon the construction and proportions. I do not say that the deviation from the true curve of sines detracts from Mr. Kapp's method, but it certainly modifies the results he has obtained. Both in the mathematical and geometrical treatment something must be assumed as a basis in the absence of experimental data. Mr. Kapp assumes the curve of sines; and the question arises, To what extent are the results modified by deviation from that curve? My own view is that no reliable comparative tests can be made without constructing the different types of transformers and actually trying them on the circuits.

The point as regards the degree to which the cores of transformers can be saturated is most interesting. From Mr. Kapp's investigations it seems that the number of C.G.S. lines per square centimètre is only one-half of what can be employed for saturating the armatures of continuous-current dynamos. I should like Mr. Kapp to tell us what is the usual weight of transformer per 60-watt lamp; also how many volts he gets per foot of conductor. In the Westinghouse transformer I believe they get 2 volts per foot.

There seems to be very little difference in the construction of the various transformers exhibited. In some the object appears to be variation rather than improvement. I should think the transformer of Mr. Kapp has an advantage over others, inasmuch as it can be easily dismounted, the cover simply requiring to be taken off for the coils to be taken out and fresh ones slipped on. This obviates the necessity for taking the whole arrangement to

pieces, which has to be done when the iron parts are interlaced Mr. Esson. with the copper parts.

Leaving transformers, and coming to distribution, it seems' that there is here some little room for discussion. I understood Mr. Bernstein last time to say that he advocated the series system; and in his paper Mr. Mackenzie showed himself rather favourably disposed towards it. From a table given in Mr. Mackenzie's paper it appears that having a constant current in the primary you have a current which is practically constant in the secondary, and that therefore the arrangement is admirably adapted for lighting lamps in series. So far, then, as self-regulation goes, there is no difference between the two systems. But when you come to the question of leads, I think the cost would be greater in any case in the series than in the parallel system. You have to keep the same section of conductor right through the circuit: if you go down 100 yards to light a single lamp, you must carry the heavy conductor with you wherever you go; and although Mr. Kapp says that in sparsely lighted districts the cost will be less, I cannot quite make out how this is. Perhaps Mr. Kapp means to say that copper can be saved at the expense of power which, if cheaper, does not in a series system affect the regulation. But, of course, if there is the same total weight of copper in the conductors, and the same electro-motive force at the central station, there will be the same amount of power lost in both systems if the current-density is similar.

As regards the dynamo of Mr. Kingdon, I am afraid I do not think very much of it, and for a reason already referred to by Mr. Atkinson. The magnetic circuit should not be subject to abrupt changes of section as the machine is working, but the flow of lines should be uniform, otherwise Foucault currents will be generated in the magnets. I see that Mr. Mackenzie is shaking his head, but I fancy that the machine will get hot. I should be very glad to find it was not so, but the heating can only be kept down by thorough lamination, which will make the machine expensive.

I am rather puzzled by the last sentence of Mr. Kapp's paper, and I am not sure whether it is intended to be prophetic or not. If Mr. Kapp believes that we shall really get secondary batteries

Mr. Esson. sufficiently improved to render lighting from them reliable, it would be a great pity if, having the conductors all right, it was found that at the generating station he had dynamos of the wrong kind, and that the transformers were also useless. Therefore I advise Mr. Kapp to give up the alternating-current system, to put in at the generating station continuous-current high-potential dynamos, and to use continuous-current transformers at the sub-stations. He would then be ready for the contingency of a perfect accumulator arising. He could charge his accumulators direct from the central station, or he could charge them through his transformers. He could light his lamps from the transformers or from the secondary batteries; under ordinary circumstances it would be from the transformers. I do not say that the system could be worked at such high efficiency as the alternating trans-former system; nor would it be so low as working from accumula-tors alone, as advised by Mr. Crompton; but it would be somewhere between the two, and the accumulators would be simply used as a stand-by, being charged occasionally just to make up the leakage, while in the absence of any casualty the lamps are fed from the transformer.

Mr. Evershed. Mr. SYDNEY EVERSHED : Mr. Esson has anticipated me in his protest against the employment of the sine function. There is one very good reason for employing it, however, and that is, that it is the only one which can be worked mathematically. It is rather remarkable that the sine function is, so far as I can see, the only one which is reversible—*i.e.*, that the secondary current will also be a sine function of the time. Mr. Kapp says he has reason to think that, whatever shape the curve of E.M.F. may be, the current will, after filtering through two or three trans-formers, come out a true sine curve. Of course Mr. Kapp, with his beautiful geometrical method, hoped that it would do so, but I am afraid the facts are against him. I think the effect of filtering it through transformers (if the original curve departs in any way from the sine curve) will be to make it depart still more from that curve. However, as Mr. Esson said, that does not really detract from the merit of the *method* of Mr. Kapp's paper in the least. But it does somewhat detract from the

interest of the *results* obtained. Mr. Esson has been good _{Mr.} _{Evershed} ·
enough to give us a diagram which shows what a complicated
law a modern dynamo may follow. The Westinghouse dynamo
is also said to give, not a sine curve, but more nearly a zigzag of
straight lines. The only way I can see of attacking such a
function is to assume that it is a hyperbolic curve, and I can
only echo Mr. Kapp's wish that some one with a mathematical
tendency would investigate the subject.

If I may, I should like to say a word about a wattmeter for
use with alternate currents. Dr. Fleming described a *large*
wattmeter which apparently did not give very accurate results.
There is no need for a wattmeter to be large. If Dr. Fleming
were to make a wattmeter like those beautiful instruments
which he showed to the Society last year, and employed in them
a very few ampère turns (say 2 or 3) on the pressure coil, and a
large number (100 or so) on the current coil, we should have an
instrument in which there was practically no lag in the pressure
coil, and consequently it would give the true power.

Professor J. PERRY: Might I be allowed to say a word upon _{Professor}
this matter of the sine curve, which is really all I have to speak _{Perry.}
about, as I have come here as a learner, merely, on the subject of
transformers? It has been said by Mr. Esson and by Mr. Ever-
shed that the curve is not by any means a true sine curve, and
that therefore Mr. Kapp's theory must be wrong. In this room
already I have made corrections of such statements. I understand
that Mr. Esson measured his field, and so obtained his curve for
electro-motive force in the primary circuit. Now it is easy to show
that the primary current will be more nearly a pure sine function of
the time than the E.M.F., and the secondary will approach the sine
curve more nearly still. The effect of self-induction and mutual
induction is to destroy the minor ripples, and this I will explain
on the board. [*It was here explained that any periodic function
is made up of a fundamental sine function and its harmonics,
which are like ripples on a larger wave of water, and that self-
induction causes the quicker ripples to diminish more rapidly.*]
Mr. Esson's curve has certain ripples every one of which is toned
down, and the quicker ripples tone down very much more than

Professor
Perry.

the slower ones; and I do not see that it is possible by any
method of measurement of the secondary current from a trans-
former to get anything very different from a true sine curve.
Mr. Evershed was certainly quite wrong, then, in saying that if
the E.M.F. did not follow exactly a sine law the current would
tend to depart more and more from a sine law. The reverse is
the case. At the same time it is to be remarked that the
harmonics will not entirely disappear, in comparison with the
fundamental, however great the self-induction may be. I have
assumed here that the self-induction is constant.

As this is really the subject of the paper—the investigation of
the transformer—I am not so much out of order as I thought, Sir,
I should be when I got up to speak.

The time is very late, but I should like to add a few words—
I will not take up more than two minutes—to mention to the
meeting something that suggested itself to Professor Ayrton and
myself some years ago. I always mention this to my students
when we are discussing Fourier's theorem, as it is an admirable
illustration of that theorem. Mr. Esson, Mr. Evershed, and
others have said that the current does not follow a true sine
curve. Well, there is a way of testing it, like Helmholtz's
method of investigating sound. Let us take a Siemens dyna-
mometer through one coil of which the current is passing. Let
us pass through the other coil, say the fine-wire coil, a current
from a battery which may be made to vary in a definite way, as
by the method adopted by Professor Hughes in his induction
experiments. Professor Hughes worked with a battery and a
make-and-break that could be controlled so as to vary the rate of
alternation, and this rate could be measured. Fourier's theory is
of enormous mathematical importance, and I think that it can be
put in a beautifully simple way to electricians. We have had so
much to do ourselves that we have not taken any steps to apply
the method I am describing. There is the main current in
coil A with any amount of ripples in it; here is the coil B through
which we pass a current with a known rate of alternation. Now,
when the rate of alternation in B reaches the rate of alternation
in A, you will have an attraction between the coils, and not till

then. Hence, by changing the rate of make-and-break—quicken-
ing it gradually from a slow to a rapid rate—a reading is obtained
for some one rate of make-and-break, and when this is the case
you have got the mean alternation of the current without any of
the ripples. You now continue to increase the rapidity of your
make-and-break, and just when you get to twice the old rate you
will see, probably, some indication—if it is small, it means a small
ripple—of twice the ordinary alternation: measure what it is. Then
go quicker and quicker until three times the old rate: you will
then probably get another indication. You can now combine the
measured fundamental and its harmonics to obtain the actual law
of variation of the current. It will be observed that if the coils
are at right angles mutual induction will have no effect.

Mr. MORDEY said the opposition to transformer distribution
by Mr. Crompton and Mr. Gordon rather reminded him of a cer-
tain historical person who told the tide to advance no further;
but with this difference, that whereas King Canute knew the sea
would not obey him, the gentlemen who had spoken did not. He
thought there was no doubt whatever, from what had been seen,
on the other side of the Atlantic at any rate, that transformers
had a very great future before them. Mr. Crompton said that
the bottom would be knocked out of his argument if it could be
shown that small alternating-current dynamos could be used and
could be put in parallel. It was easy to knock the bottom out of
the argument. In the United States machines were being run
in parallel not only in one large station, but it had been done
across rivers. A station on one side of a river is supplying mains
connected with a network on the other side, as well as on its own
side; and when it is necessary to put more power on the mains, the
dynamos on the other side of the river are switched in. He did
not think there was any serious difficulty. There was one objec-
tion he would like to make to the conclusion of Mr. Kapp's paper,
and that was as to the use of low-tension networks with trans-
formers. It seemed to him that the great advantage they had in
transformers was that they got rid of the enormous cost of a low-
tension network. Everybody had known that the cost of the
copper in a low-tension distribution system had been a very

Mr.
Mordey
serious item on the whole. It seemed to him that as transformers
regulated so perfectly, and as they could be made to regulate even
more perfectly than they did just in their simple state, and as
they were undoubtedly very efficient indeed for small loads, and
further, as loss in the main conductors could be at a practically
small cost rendered of no practical effect whatever,—it seemed to
him that they got rid entirely of any necessity for the distribution
network. They only wanted a distribution network that would
help one part from another when the wires of any one part were
not sufficient to carry a full current. He also disagreed with Mr.
Kapp as to the danger of having separate transformers for every
house. It might not be necessary to put them in every house,
but he certainly thought that if some simple arrangement were
made for cutting out a transformer when anything went wrong
with it, it was the very best thing they could do. They might
put a number of transformers right along the mains, and just one
house would be cut out and no more when the transformer in that
house went wrong. He even went so far as this, that in a big
house like a hotel he would run the mains round outside the
buildings; he would take the wires in at two or three places from
the outside, and put transformers at those points. To that
extent, perhaps, a network system might be used—that is, with
the inexpensive primary rather than the expensive secondary
conductors.

With regard to the employment of series or of parallel systems
of connection, he thought the advocates of the series connection
of transformers forgot two difficulties. One was that the
regulation of the constant-current system with an alternating-
current dynamo was not at all easy. He did not know that that
had been done yet. They could regulate and maintain a constant
current with a direct-current system, but for an alternating system
this problem had not been solved in practice. The regulation of
the transformer alone only solved one part of the problem. They
must have a constant current in the first place. When they had
got a constant current it had been shown in two ways—by using
lamps in series, and by using Mr. Kent's very ingenious arrange-
ment of the impedance coils—that it was possible to regulate the

transformer. But they had got to start with the dynamo at the Mr. Mordey. station, and they had not got over that preliminary difficulty yet. Then the second difficulty was that in a series or any other system what limited the engineer was the E.M.F. They could only work up to 2,000 or 3,000 volts. When they had got there they must stop and begin to increase their leads. He thought it was just as well to start with the parallel system, in which there was no problem that would have to be solved as they went on. But even if the constant alternate-current dynamo were forthcoming—and he felt that if such a machine were necessary to progress it would be produced—the self-regulating powers of the series system would be seriously hampered by the want of pliability found in the arrangement and connection of the lamp leads. If the simple series of lamps of large current and low potential were used, the conductors would be large and troublesome; if groups of ordinary lamps in multiple series were employed, there were all the old complications of multiple-series working; but single lamps of a group could be switched off by using impedance coils, though only with further complication and expense. Besides all these connections, the series system would sometimes land us in the ludicrous position of having introduced a higher difference of potential in a house than we had outside of it. We should actually have to transform up instead of down from our high-tension mains. This would be particularly the case with a primary current of any considerable amount.

With reference to the running of the two mains close to-gether, any necessity for so placing them applied equally to parallel and series work. It was thus probably not advisable to have a large open loop of a single main out and home, as was often done in continuous-current work. This whole question of series or parallel connections demanded more than a passing notice, and the present was the time when the advocates of either system should justify their position. While admitting that for very small work (in which, however, it would be better to employ continuous currents) transformers in series might be used with advantage over transformers in parallel, he thought that for general distribution parallel connections would become almost universal.

Mr.
Mordey.

He was very glad to see Professor Ayrton making use of the calorimeter method of testing the transformer. He believed that method, even roughly carried out, could be made very accurate. About a couple of years ago he tried an experiment of this sort in the works of the Brush Corporation. He took a box—an ordinary packing case—and made it water-tight, put a resistance coil in it, sent water through it, and sent a current through that resistance coil. He wished to measure the energy; the current and the difference of potential, which he did not know, being taken by independent observers. Using a bucket to measure the amount of water passing through this rough calorimeter, a couple of thermometers to take the temperature of the incoming and outgoing water, a result was obtained which worked out to within 4 per cent. of the actual energy as shown by the independent volt and ampère readings. That method could be easily applied as an absorption method of testing alternating currents. Unfortunately, it could not be used as a transmission method. It would thus be quite possible to carry out an electrical test without any other instrument than a thermometer. He might remark that it was advisable to allow the water to rise to a fairly high temperature, in order to get a large difference of the thermometer readings: this reduced the margin of error.

It had been remarked by a previous speaker that it was not possible to decrease the speed of alternating-current dynamos for small loads because the alternations must be kept the same. Might he be allowed to mention a method by which it was possible to decrease the speed and to keep the alternations the same? He did not know that it was a very practical method, but at any rate it would work on paper. If they took an ordinary Gramme armature, and put a laminated field to it, and sent an alternating current at a given rate of alternation around the field, they would get from the ordinary continuous-current machine an alternating current with the same rate of alternations. If they kept a constant field they would get an electro-motive force which would be directly proportional to the speed. Thus it would be

possible to vary the speed without changing the rate of alternation,
and to vary the current or E.M.F. by a combination of speed
and field regulation. The only thing that would be necessary
would be to have a small machine, which could be separately
driven, to excite the fields at a given rate of alternation.

He might, perhaps, mention another experiment that he had
made. Mr. Kapp with his diagrams could at once see what was
going to take place in a transformer, but some of them could not,
and they liked to be able to see it if they could in a practical
way. He wanted to be able to get something that he could look
at that showed the lag in a transformer, and he should like to
submit his method for criticism. Perhaps Mr. Kapp would tell
him if it was a wrong method. If they were to take two trans-
formers and join, say, the high-tension coils in series and send
an alternating current through the low-tension circuit of the
first transformer from a dynamo, as in Fig. 1, then, of course,
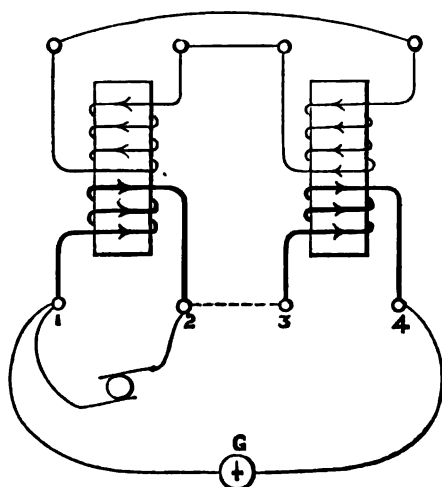
Fig. 1.

they would get a current in the low-tension circuit of the second
transformer, which might be used, say, for running glow lamps,
and the momentary currents in the two coils would at the same
time be (roughly) in the same direction, as shown in the figure.
Suppose, then, that a certain difference of potential is maintained
at the terminals 1 and 2 of the first transformer, as shown by a

Mr.
Morday.

Cardew voltmeter, then there would be practically the same potential difference between terminals 3 and 4 of the second transformer, as would be shown by the voltmeter. The readings would not be quite alike, on account of the small loss due to the double transformation up and down. Now, if terminals 2 and 3 were connected as in the first figure, and the voltmeter were placed across the terminals 1 and 4, it would, if the waves absolutely coincided, indicate the sum of the two previous readings; but if the waves did not coincide in phase the reading would be less than the sum of the two first readings, and the amount of this reduction might be made a measure of the difference of phase. Instead of connecting the two coils as in the first figure, they might be joined together and to the voltmeter, as in Fig. 2,
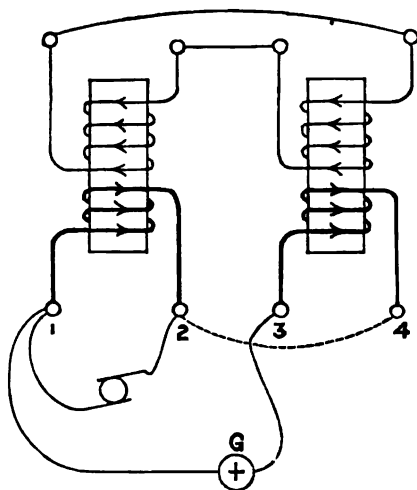
Fig. 2.

when it would be seen that the indication would be zero if there was no difference of phase, any indication on the voltmeter showing that the waves were not coincident. He had tried the experiment not only with two but with six transformers, connected in the same manner, in a series, and using the first and the last coils of the series connected as shown, as well as with the two windings of a single transformer, and in neither case had he been able to find any indication of lag, not even when there were many steps in the transformation processes. This showed that

the maxima and minima were reached simultaneously in the two
windings of a transformer.

Owing to the very sad news (which he was sure they all deeply regretted) of M. Gaulard's mental illness, he was a little reluctant to say anything about the theories put forward in Mr. Mackenzie's paper, many of which were open to question. He thought, however, one could not, without protest, pass over the statement that it was not till the views of Faraday had been proved to be erroneous that it was found possible to make any advance in transformer work.

Mr. P. W. WILLANS: I do not know, Sir, that I can say anything upon this subject, except that I can fully corroborate what Mr. Crompton has said as to the wastefulness of working large engines at very low power. The decrease in efficiency is not quite so marked in the case of condensing engines as it is in that of non-condensing engines; but it is of the utmost importance in both cases to work the engines at their most economical load, which, in the case of compound engines driving dynamos, is almost always at full power.

Mr. T. H. BLAKESLEY: I should very much like to say some- thing upon this subject, if I may be allowed. My remarks may not be in chronological order, but I would first refer to the attempts at measuring the phase difference of two alternating currents. Some years ago I gave a method of precisely measuring this phase difference, which has met with such approbation from Signor Galileo Ferraris that he has done me the honour to claim it as his own. Perhaps I may point out to you exactly what it is. An ordinary dynamometer, as you know, is composed of two coils, one of which is fixed, the other being movable. If those two coils are used in series you can measure the square of your alternating current. You can apply this method first to one of your coils and then to the other of your coils, and then, if you please, you can send one of the currents through one of the coils and the other through the other. These three readings give you, without the slightest difficulty, the cosine of the angle of phase difference. Nothing is simpler, and nothing is easier to prove that it is. If Mr. Kapp would try this method on the primary

Mr.
Blakesley. current and the secondary current in his transformer, I think,
Sir, his mind would be enlightened as to some facts which he
has put forward in connection with this subject. He has drawn a
line representing the phase of magnetisation in his case, and he
has made that correspond in phase with the resultant of the fields
produced by the currents in the secondary coil and in the primary
coil. In his Fig. 4 you will observe that O P is this resultant.
I say this altogether fails if hysteresis is a fact; if hysteresis is
not a fact, I have nothing more to say on the matter. But Mr.
Kapp himself owns that there is a considerable degree of hys-
teresis involved in the reversal of the polarity of a mass of iron.
I will point out what I mean. O J^1 and J^1 P together make up
something which is in the same phase as O P. Compounded, they
make up the magnetic field due to the currents—that I will allow;
but that the magnetisation phase corresponds with this resultant
I deny, and I deny it on this ground : If the magnetisation phase
coincides with O P, the phase of the increase of magnetisation is
at right angles to O P, and therefore its projection on O P equals
nothing at all; and therefore, as this projection should be one of
the factors of a product (representing the power of the hysteresis)
of two things, viz., the magnetic field and the increase of the
magnetisation in the magnetic field, the hysteresis itself would
be nothing. With definite hysteresis the magnetisation must
follow the field of magnetism produced by the currents. In fact,
Mr. Kapp has left out a certain line representing *an induced
magnetic field due to the increase of magnetisation itself*—an effect,
I think, not very often, if at all, recognised; and if he puts that
in (in a way that perhaps it is rather too complicated to go into
now), without much difficulty he will work out his complete
figure. Inversely, without the knowledge of this one thing, viz.,
the coefficient of this hysteresis—which is, in reality, what he is
seeking—if he applies the dynamometer method to find the phase
difference of the two currents, the result will be that his figure
is again completely determinate, and that the hysteresis comes
out as a function of those dynamometer observations.

I think it is very right of Mr. Kapp to indulge in these
geometrical figures—I have done so very largely myself—but, in

my opinion, he is wrong in tracing in the same diagram things of Mr.
Blakesley. an entirely different sort—fields, E.M.F., and so on. I do not know whether those are the two that he has mixed up, but I think he has not kept all his lines of the same sort in that diagram. If he had been successful in his representations, perhaps I should have had to quarrel with him on another ground, because I think I started this geometrical method of representing phase difference of electrical magnitudes before he took it up. But I must entirely disclaim that triangle J^1 O P as representing the phases of the magnetic stress and the resultant magnetisation.

Mr. A. WRIGHT: I should like to say a few words with regard Mr. Wright. to Mr. Kapp's statement about the superiority of the core transformer over the shell transformer. I think in this he has lost sight of the fact that, as nearly all the heat of the transformers is produced in the iron, the iron, being outside in the shell form, is better able to lose its heat than in the core transformer; and on taking that into consideration I think he will find that the shell is very much better than the core form.

When the iron is inside, the heat, before it can escape, has first to warm the copper and thus increase the loss in volts; and the *sine quâ non* of a good transformer is a small loss in volts in the primary and secondary, consistent with having a very small current passing through the primary with the secondary circuit open. In central station work it will be most important to have a very small current indeed passing through the primary when the secondary is open, because in the case of all-day and all-night supply that current will mount up when there are many houses connected on the system. I think it is very desirable to fully investigate which form of transformer is the better, on the basis of comparing the through current: by "through current" I mean the current passing in the primary when the secondary circuit is open.

With regard to safety instruments connected to the secondary circuit of a transformer, perhaps the simplest plan would be to connect the secondary circuit to earth through a very high resistance inductionless coil, and when the tension of the secondary circuit got dangerously high the current through this

Mr.
Wright.

coil could make a tumbler fall which would short–circuit the secondary terminals and so cause the primary fuses to go and cut out that transformer from the rest of the circuit. That is simpler than the electrometer method, and, I think, perhaps safer than Professor Ayrton's proposal of putting the secondary circuit straight to earth.

The practicability of putting converters outside houses is a most important question, and I think the inherent difficulty ought to be got over by putting the converter in cast-iron boxes with water-tight joints, and placing a very small phial of sulphuric acid to condense any moisture that might arise inside the box. I think there may be danger in having a high-tension converter inside a house, and probably the above plan would succeed, as it does in other cases, such as in electrometers, where it is absolutely necessary to get very high insulation.

With regard to Mr. Crompton's challenge about the cost of the unit at all loads, if Mr. Mackenzie is not going into those figures, I shall be very happy to give the figures that we have worked out at Brighton; and I hope he will take into consideration the enormous cost per unit for depreciation of batteries and for attendance.

With regard to measuring the efficiency of a transformer by means of a calorimeter, this seems to me hardly practicable with large apparatus, as it will be very difficult indeed to put a 6-unit or 7-unit transformer into a vessel of water without losing a great deal of heat by radiation from the surfaces of the calorimeter.

As to Mr. Kapp's query as to the advisability of having one transformer to every house, or one to many houses, *this*, I think, depends very much upon how the houses of the consumers are situated. In systems of electric lighting it will be very difficult for the next two or three years to get a number of consumers close together. If two or three consumers can be got close together, it would then be well to put one converter for that group; but for many years to come I do not think this will generally be the case, and we shall have to put separate converters for every house for some time yet.

Mr. W. L. MADGEN: Mr. Mackenzie has stated that it was
not until M. Gaulard had discovered that induction apparatus
based upon the theory founded by Faraday were not suitable
that success crowned their efforts.   Perhaps he will say more
particularly what he meant by that.   The reason I refer to it is
that some time before, I understand, Messrs. Gaulard & Gibbs
commenced their experiments Jablochkoff had done a very great
deal in connection with this question.   Mr. Mackenzie went so
far as to admit that Jablochkoff, Sir Charles Bright, and a
few others had given some attention to the matter; but I
think he hardly did Jablochkoff sufficient justice.   There is a
drawing before the meeting of Jablochkoff's transformer of 1877.
It is not very clear, because it was hastily prepared; but it will be
seen that it combines various very important points, among them
being the closed magnetic circuit.   In that year the closed mag-
netic circuit was considered by him the best form—long before
the later period of 1885 referred to.   All the modern types of
transformers seem to be very much alike.

Mr.
Madgen.

Referring to the Westinghouse transformer, several good
things have come from America; and not unfrequently, I suppose,
one may go from here.   The reason I mention that is because
of the strong resemblance between the Westinghouse and the
Kapp transformer.   Probably the most ingenious modern appli-
ances in connection with transformers are the choking coils of
Messrs. Sharp & Kent.

A perfect secondary battery was referred to by Mr. Esson
as being a "contingency."   That, taken in connection with Mr.
Crompton's remarks, sounds rather peculiar.

Professor SILVANUS P. THOMPSON: I have read Mr. Kapp's
paper with very much pleasure.   I cannot pretend to discuss it
at this late hour, but I would like to say that I very much
appreciate the particular way in which Mr. Kapp has handled
those geometrical diagrams; and it seems to me that Mr. Blakesley
did not quite understand one point in those diagrams—as to what
the line O P represented: it did not represent current at all, but
it represented the resultant magnetisation due to the two cur-
rents.   Although I very highly appreciate the good work that Mr.

Professor
Thompson.

Professor
Thompson. Blakesley did three or four years ago in applying this problem
for alternate currents, I think we ought to acknowledge our
indebtedness to a still older source for the use of this kind of
diagram to represent the composition of harmonically varying
quantities, for many of us learned the method from Thomson and
Tait's treatise on "Natural Philosophy" many years ago.

I cannot quite agree with Mr. Kapp in dividing transformers
into "core" and "shell" transformers. Where does one class
begin, and where does the other end? When does a core become
a shell, and when does a shell become a core?

It seems to me that, given the one essential fact that the
iron circuit must interlace through the copper circuit, there may
be any number of intermediate forms between the one extreme,
where all the exterior surface of the iron is covered with copper,
and the other extreme, where all the exterior surface of copper is
covered with iron. I suppose the single iron ring inserted
through the copper circuit, as in Mr. Kapp's Fig. 1, is a core
transformer. What is it if there are two iron rings? Does open-
ing apart the two iron rings at right angles to one another change
the core into a shell? I cannot see that such transformers as
Westinghouse's or Mordey's are of the shell type rather than of
the core type. I must confess that the attempt at drawing a
distinction is a little foggy. All the same, I wish to express my
indebtedness to Mr. Kapp for the excellent way in which he has
put this subject before us, and has added solid matter to our
knowledge of the subject, and for the very impartial manner in
which he has distributed his favours to the various rivals in the
race for improvement.

I wish I could say the same of Mr. Mackenzie's paper. I
must confess that I find in it much to severely criticise. That
statement that the theory founded by Faraday had to be
abandoned is simply untrue.

Mr. MACKENZIE : Excuse me ; a misprint has occurred : instead
of "*and* exemplified by Ruhmkorff," it should read, "*as* exempli-
"fied by Ruhmkorff."

Professor S. P. THOMPSON : Then what does Mr. Mackenzie
mean by talking of the "Ruhmkorff *and* Faraday type" only two

Professor
Thompson.

lines further down in his paper ?   What can it mean, when we all
know that the " secondary generator " depicted in the very first
patent specification of M. Gaulard is simply an induction coil of
Ruhmkorff type ?   However, I do not wish to dwell too long on
that point.

Again, what is the result of dividing the things into a
class of Gaulard type and one of non-Gaulard type ?   I see here,
in Mr. Mackenzie's paper, that " Gaulard type " means that the
secondary is placed " in an identical position in regard to the iron
" core with the primary ; " and, as far as Mr. Mackenzie's drawings
go, they show that there should be one turn of the primary
between each turn of the secondary wire.   If that is not meant,
and if one turn of primary is put to ten of secondary, or *vice versa*,
I do not see any distinction from anybody else's form of trans-
former.   I used to be told that here was the difference between a
secondary generator and an induction coil: that in an induction
coil the coils were wound in the usual Ruhmkorff way, but in a
secondary generator there was a series of flat copper washers for
the primary, lying each between a similar set for the secondary.
That was a distinction in construction, but now it seems that that
is not to be the distinction, and it is not at all clear what is the
distinction between M. Gaulard's form and any other forms.

There are other points which I do not like to criticise too
severely, but I must go into one point.   It is stated that " when
" Gaulard & Gibbs first exhibited their system at the Westminster
" Aquarium, in 1883, only one transformer was there shown at work,
" and this was consequently either in series or parallel with the
" dynamo."   What does " in parallel " mean when applied to two
things electrically connected ?   It means that the current has
alternative routes, and may divide at a certain point, part of the
current flowing through both these things, and then reuniting at
a point beyond both.   Do you mean to say that when one dynamo
is joined to one transformer they are in parallel ?   They are in
series.   It is a misleading statement to say that such a system
can be in parallel.   With that sample of the paper I will leave it.

Now I come to the remarks of Mr. Crompton, that I really feel
ought not to go unchallenged.   There is a curious and misleading

Professor
Thompson. assumption underlying his arguments. He says we must work at something under 500 volts. All his argument is based on that assumption. Why does he not take 5,000 volts or 10,000 volts? Because his dynamos will not do that. Fancy 2,000 volts on an ordinary commutator made up of parallel bars, insulated with half-millimètre mica. We know it may last for a trial run, but for how many years will it last? We have to find out some other means for working with a potential of 5,000 volts or 10,000 volts. Mr. Crompton has good reasons for limiting it to the potential of distribution that he adopts.

Mr. J. E. H. GORDON: How about the Brush machine?

Professor S. P. THOMPSON: The collector is quite different. So is that of the Thomson-Houston machine; so are those of the alternate-current machines that will work at 2,000, 3,000, or 5,000 volts. Mr. Crompton limits them to 500 volts.

Mr. R. E. CROMPTON: Why should you not use that?

Professor S. P. THOMPSON: Because it is obvious that the main reason for transforming is to take advantage of the economy of distribution at a high potential.

Mr. J. E. H. GORDON: Everything is as the square of the volts, and you have got the limit already.

Professor S. P. THOMPSON: Yes; but it is found possible to go further with alternating-current machines. There is a second assumption underlying Mr. Crompton's argument—that alternating-current machines are not to be compared with continuous-current ones: they take up so much room, they are not so economical, and will not work in parallel. I am afraid that Mr. Crompton is not up to the latest information about alternate-current machines, or he would not say what he does. I happen to know that there are new alternate-current machines —and we may hear of them in this room soon—as much better than the common alternate machines as the new continuous-current machines are better than those of five years ago. The same kind of improvements have been introduced, with the same happy results. As to machines not working in parallel, we have had indications enough on this point in this room to-night that this not an insuperable difficulty.

As to the sine function, I have pointed out on more than Professor Thompson. one occasion that the question of the curve obtained as the curve of induction in the machine is a question of the shape of the pole-pieces. It is perfectly true that the self-induction of the circuits, in every part of them, acts, as I have said before, as a " harmonic " fly-wheel," to flatten down the ripples, and tends to make the wave approximate more nearly to a true harmonic period. With all due deference to Mr. Esson's curve, it is perfectly clear, if the machine gave that curve for its curve of induction, that its pole-pieces might have been better designed.

I would like to say that I have no great love for alternating currents. If I have differed from Mr. Crompton to-night, it is not because I do not think storage is itself, in the abstract, a great deal better ; but I do not, in this state of things, want to see any difficulties unnecessarily put in the way of the fullest and most complete trial being given for the system of distributing alternating currents by transformers.

Lastly, I am very sorry that I have not heard to-night any mention made of continuous-current transformers : I mean motor dynamos. They appear to me to have obtained a real success. I am sorry we have not had some contribution from Messrs. Paris and Scott in this debate, to tell us. how they carry out their system, because we know so little about these machines. One extraordinary property they have is that when they are doing their work properly the currents in the incoming coil of the armature and that in the outgoing coil neutralise one another's self-induction. There is a mutual induction between the two, and, as a result, here, as in the alternate-current transformers, the self-inductions of the two separate coils are practically wiped out by the mutual induction between the two. The consequence is that these machines hardly spark at all when doing their heaviest work.

It is clear that in all directions distribution by means of transformers is the order of the day, whether it be by alternate-current machines on the one hand, or by accumulators on the other, or, thirdly, by motor dynamos. It is clear that we have to study this question of transformation in all its bearings

Professor
Thompson. for the basis of immediate economic distribution of electric
energy.

Mr. Money-
Kent.   Mr. J. M. V. Money-Kent: There are many points in Mr.
Mackenzie's paper that I do not understand, but particularly I
should like to ask him what he means when he says that "the
"value of the secondary circuit [presumably the current] varies
"directly as the square of the primary current." This surely is
not the case.

As regards my device for shielding the secondary circuit from
the primary, perhaps I had better explain that this shield is not a
bare copper band connected to earth inviting the primary to leak
into it, but a heavily insulated band, and so arranged with respect
to the two coils that, should there be any *tendency* on the part of
the primary to leak, it would leak into the shield before it got to
the secondary coil. Suppose such a leak does take place, what
happens? Why, if there is any other leak on the circuit, an
excessive current flows through the two leaky transformers, the
cut-outs melt, and the transformers are cut out of circuit. Next
suppose the primary leaks into the shield and also into the
secondary. Well, then we only arrive at Professor Ayrton's
suggestion of earthing the secondary. And yet he thinks his plan
simpler! Sir, I consider the practice of earthing the secondary
would be very reprehensible.

As regards supplementary devices, I may say that I have
sketched out several to be inserted in the earth wire—that
is, between the shield and the earth—some of which might be
made to indicate a leak or ring an alarm if a leak occured.

Coming now to Mr. Kapp's paper, I do not at all agree with
him as regards series working. I thoroughly endorse all Mr.
Bernstein has said on the subject. There are three distinct ways
of obtaining self-regulation when the primary coils of the trans-
formers are in series—

1st. By putting all the lamps in series on Mr. Bernstein's
system.

2nd. By the use of what have been called our "choking" coils,
or what we prefer to call "induction" coils.

3rd. By building up the transformer in sections, so far as

the core of the secondary is concerned, and connecting Mr. Money-
Kent. each lamp or group of lamps to its separate section.

The first and third of these methods is admirable for small or medium sized transformers, and the third will do for any size. All three are perfectly self-regulating. As the second method has been described elsewhere, I will not take up your time with it here.

From our experiments we find that an induction of 5,000 is quite high enough for satisfactory working.

The PRESIDENT: The time is very late, and it is necessary to The
President. close the discussion. Perhaps Mr. Kapp will be able to now give us his reply?

Mr. GISBERT KAPP, in reply, said: In the first place, I will Mr. Kapp. answer Professor S. P. Thompson's question as to the distinction between core and shell transformers. The explanation of these terms, as already given in the paper, is this: A shell transformer has one set of coils and two cores or more; a core transformer has one core and two sets of coils or more.

Mr. Wright remarked that the shell transformer is the better type. I quite agree with him, and that is the reason why I have adopted this type for my own transformers. The original Zipernowski transformer has, however, the worst possible arrangement of shell and coils, and is actually inferior to an ordinary Gramme ring. As at first introduced it was a shell transformer of circular ring shape, and has now been abandoned even by its inventor, who, in his modern transformers, puts the iron inside and the copper outside.

A very interesting experiment was described by Mr. Mordey, showing that there is practically no lag in transformers, and thus confirming the statement in the paper that the angle a in my diagram, when the transformer is worked with a fairly large output, is very small—perhaps one or two degrees—so that the cosine of this angle is very nearly unity. Even when a large number of transformers are coupled together, no appreciable difference of phase is obtained. There must, of course, be a certain difference, but the errors of the measuring instruments seem to have been greater than this difference, and therefore Mr. Mordey appears

Mr. Kapp. to have obtained the exact sum of the separate electro-motive forces.

Mr. Mordey objects to a secondary network of mains on the ground of expense, and would prefer to bring the light high-tension wires right on to the premises of subscribers. I object to putting transformers into the houses simply on account of safety. I hold that, whatever we do, we must study safety before the cost of installation, the coal bill, and every other consideration. As long as you have high-tension wires and transformers in the houses more or less within reach of the inmates, they are not safe. That is the reason why I advocate putting transformers outside of the houses, and arranged to feed a secondary low-tension network of mains. We would also have the further advantage that, when one transformer broke down, the houses ordinarily supplied by it would not be put in darkness. In any system of direct supply you are bound to have a network, but instead of using the huge mains which Professor Forbes brought before us some years ago, and which are necessary where the feeding points are far apart, we need only have mains of comparatively small section, because we can multiply the feeding points to any convenient extent. It will thus be quite possible to use Mr. Crompton's mains, which he told us to-night cost only 14s. 6d. a yard. To sum up, these are the two reasons why I advocate distribution by a low-tension secondary network : First, absolute safety ; and next, the possibility of arranging for constant pressure without excessive weight of copper. There is, however, also a third reason, viz., if you have the network once laid down, you can use it when the happy day arrives that you have a perfect storage battery.

A question was asked by Mr. Esson about the weight of transformers in relation to their output. That depends entirely upon the size. This transformer now before you weighs 1½ lbs. for every lamp; the transformer which Professor Ayrton tested weighs 2 lbs. for every lamp; and a 500-lamp transformer would weigh a little over 1 lb. per lamp.

I now come to the remarks of Mr. Gordon and of Mr. Crompton. I must confess that when I heard those two gentlemen speak I began to feel very doubtful whether, after all, we

were not wasting labour on a hopeless problem. I was, how-
ever, comforted by the idea that, supposing even such men
as Edison, Brush, or Westinghouse, had been sitting here,
their case would not have been much better. If the arguments
in favour of storage batteries could have carried conviction
to their minds, they would have gone home resolved never to
put up another central station again on the direct-supply system.
But I think we may take it for granted that their previous
experience would have saved them from falling into this despon-
dent mood, for what are the facts? How many battery stations
are running in the world, and how many direct-supply stations?
I know of a very small battery station in London and of two larger
ones on the Continent—one in Berlin and the other in Vienna.
On the other hand, the number of direct-supply stations now at
work is very large. I cannot give you the exact number, but
think in America there are something like 1,000, and on the
Continent of Europe there are perhaps 100 or 150. In England
there are about 14. All these are working without batteries, and
are therefore open to the criticism that their machinery must be
capable of supplying the extra demand which we see in Mr.
Gordon's diagram. But this diagram illustrates a bad case. You
can only have such a condition of things when you have to light
the same class of houses throughout the district. But in London
we never have districts so defined : we have residential mansions,
offices, warehouses, shops, and humbler dwellings all more or less
within the area which would have to be supplied from one station ;
so that, although in this diagram the condition of supply looks
very unfavourable, you will scarcely ever have to meet such a case.
But, granting that so exceptional a case should be met with in
some London district, in what way is Mr. Crompton better off
than we are? He has to supply this extra amount of light for a
short time, and must therefore provide battery power accordingly.
My argument is simply this. To meet any exceptional demand
we have to put up more machinery, and he has to put up more
batteries. Now, inasmuch as for equal output batteries are more
expensive than boilers, engines, and dynamos, he would be rather
worse off than we should be.

Mr. Kapp. I am not against batteries in any way: I would hail the day with delight when we got a good battery; but all the arguments that were used to-night by Mr. Crompton and by Mr. Gordon might have been used with equal consistency three years ago. We had batteries then; we had very good direct-acting steam engines and dynamos: why did not the advocates of batteries come forward then and use them for central station lighting?

Mr. CROMPTON: We had no good battery.

Mr. GISBERT KAPP: Have we a good battery now? Most of us have seen the batteries at the Colonial Exhibition, and the report on their performance. These batteries were supervised by an efficient staff, and yet gave only an efficiency of 60 per cent. No transformer need work at so low an efficiency, because we can always arrange to work the transformers at a reasonable output, say not less than one-third of the maximum. If the consumption of current in the secondary network decreases beyond a certain point, all we have to do is to switch off the current from certain of the primary feeders, leaving the work to be done by the remaining feeders and transformers.

The point about running dynamos in parallel has already been answered, and that disposes of the argument about having a big compound engine to do little work. You would subdivide the engine and dynamo power into convenient unit sets, and use as many sets as correspond to the consumption of current at any time.

I should like to say a few words about the question of dynamos. I am afraid you will go away with an erroneous impression regarding the size of alternate-current machines if you take Mr. Gordon's figures. He mentioned a machine for 200 kilowatts, with an armature 10 feet in diameter. Now the machine a photograph of which was shown last Thursday is a 60-kilowatt machine, and has an armature 40 inches in diameter by $3\frac{1}{2}$ inches wide. If you build a machine on the same lines with a 5-foot armature, it will give 200 kilowatts, and would weigh about 80 lbs. per horse-power, which is less than a continuous-current machine.

I come now to the discussion of last Thursday.

The calorimetric measurements of Professor Ayrton are very

interesting, and, as far as I know, they are the first experiments Mr. Kapp. of this kind that have been made with transformers. There is one point to which attention should be paid. The temperature of the room in which the calorimeter is used should be about the mean temperature of the incoming and outflowing stream of water, so that the radiation of heat to or from the calorimeter may be nothing. That can be always done, because you can regulate the rate of flow. Suppose the room is 15° C.: if the incoming water is 10° C., the flow can be regulated so that the outgoing water has a temperature of 20° C.   Then the mean temperature of the water in the calorimeter would be the same as that of the room, and no heat would be given to, nor heat abstracted from, the calorimeter.   I am told by Professor Ayrton that that precaution was adopted.

Several speakers to-night, and also last Thursday, dealt with the question of magnetic lag. They say there is magnetic lag. I think there is not. My reason for so thinking is that if there were magnetic lag it is difficult to see how any telephone could articulate. If there were lag, the lower the voice going through the telephone the less distorted would be the articulation. Now, as a matter of fact, the telephone transmits high voices better than low voices, notwithstanding the fact that high voices have quick waves and should therefore be more distorted by the lag.

Objections were raised by Captain Cardew and Mr. Bernstein to the safety device which is put between the two coils on our transformer, and Mr. Bernstein characterised it as something which leads the primary current into temptation. Now what is this temptation ?   It consists in the presence of a metal sheet outside the primary coil, but between this sheet and the primary coil there is insulation.   Now in what way does this differ from the temptation which exists in a transformer having no such sheet ? You have metal in the secondary coils, and you have insulation between it and the primary, which is just the same thing; and in what way is the strain brought on the insulation of the primary greater on account of this dividing sheet than the strain which is already occasioned by the presence of the core itself?   You are bound to have iron somewhere, and whether you have it on the

Mr. Kapp. inside or outside the strain on the insulation is just the same. For these reasons I believe Mr. Bernstein's objection is really a sentimental one.

A very interesting question was asked by Mr. Bernstein as to the comparative efficiency of series and parallel transformers when the output is varied. That depends entirely upon the ratio between the waste in the iron and in the copper at full output. In a transformer for constant pressure—that is, for parallel working— the magnetisation of the iron is a constant, and therefore the heat generated in the iron is a constant for all loads, but that generated in the copper decreases with the load. When transformers are worked in series, the magnetisation, and therefore the heat generated in the iron, is proportional to the output, but the heat generated in the copper is a constant. If we have two transformers—one series, the other parallel—in which the heats generated in copper and iron at full output are equal, then the efficiencies will also be equal for full load and approximately equal for all loads; but, inasmuch as the heat of iron is generally greater than the heat of copper, the series transformer will be slightly better in efficiency for small loads.

Dr. Fleming suggested that experiments should be made to ascertain the permeability of iron at different periodicities. That would be interesting in itself, but a much more important investigation would be that dealing with the heat generated in iron at different degrees of magnetisation and different periodicities. It is this question of viscous hysteresis which regulates the design of transformers. If we only could magnetise our core more than we do now without producing more heat, we could make transformers which would be exceedingly small for their output.

In conclusion, I would refer to the general question of storage batteries against transformers. I advocate at present the adoption of alternating plant simply because I have no confidence in batteries. If secondary batteries were perfect, or even fairly reliable, I should never dream of putting down alternating-current transformers; and to provide for the day when batteries will have become sufficiently improved for use in central station work I think it will be wise now to put down our alternating-current

plant in such a manner that most of it can be used later on. Let Mr. Kapp. us see what, under the programme I have sketched out in the paper, we would have to discard, and what we could retain. We could retain our boilers, engines, and station buildings, the whole of the feeders, the whole of the secondary distributing network, and all the wiring and fittings within the houses. We would have to discard the alternating-current dynamos and the transformers, both of which together will not come to more than 10s. a lamp fixed. The fitting of a lamp in private houses will cost from 30s. to 35s., the conductors in the streets would cost something like 30s. per lamp, and the station buildings, boilers, and engines another 30s. at least. The total outlay may be taken as about £5 per lamp fixed, in which sum that portion of the plant which might eventually have to be discarded figures with only 10s.

You will see I do not advocate the adoption of a system which will be altogether wrong in a few years. I advocate a system not only workable for the present, but which, with a very slight sacrifice, will be good for the day when storage batteries have become sufficiently improved to be fit for use in connection with long-distance central station lighting.

Mr. J. KENNETH D. MACKENZIE, in reply, said: It is so late Mr. now that I shall not be able to reply to half the questions and Mackenzie. remarks that have been raised on my paper.

On Thursday last Professor Ayrton asked for some information about the regulating coils that he believed we used at the Grosvenor Gallery for the purpose of maintaining a constant potential in the secondary when we were running the transformers in series. Several were made, and tried at different parts of the outside circuit. They consisted of fine-wire coils, with a core formed of a bundle of fine iron wires, which could be pulled out more or less by hand as the lamps were turned off and on, and were placed as a shunt in some instances across the poles of the primary, in others across those of the secondary; but the general result was to burn the coils up, and they were done away with. We were in rather a fix at the time, as constant complaints were being made regarding the variation of the lamps as they were turned off and on; and any suggestion made

Mr.
Mackenzie.

was tried in order to get some sort of regulation, so long as the generators were kept in series. The regulating apparatus at the central station for keeping the current constant on the line was also incomplete, and as we were getting ready to run the transformers in parallel, any expedient that might have helped us to " run along " was tried.

Mr. Bernstein has suggested that his fusible contact plugs would serve as a means of putting the secondaries to earth should the potential rise beyond what was proper, and I think that such an arrangement might be quite feasible, and act, perhaps, quite as well as Captain Cardew's contrivance, which I think most ingenious, and excellent for the purpose for which it is intended ; but I do not at all hold with the plan of putting secondaries to earth, for many reasons already brought forward by some of the previous speakers, as I think it a most dangerous proceeding.

Mr. Atkinson said that he did not know that Ferranti's dynamos had any iron in their armatures, or that alternating-current machines ever employed iron for this purpose. I myself have not seen Ferranti's largest dynamo, but I was told not very long ago that there was a considerable amount of iron in it, and that, in fact, all the machines now made for central stations by that maker contained a very large amount of iron in their armatures. Certainly it is so used in the Westinghouse dynamo, but as I have never worked with an alternating dynamo having iron in the armature, I cannot say what the results would be regarding regulation, as I have before remarked in my paper.

Mr. Atkinson also made some remarks about the Kingdon dynamo, and I expected Professor Silvanus Thompson would have mentioned it as well, since it is a modification of an idea of his. The results that we have had so far with the dynamo Mr. Kingdon is having built to show the idea, have been very satisfactory ; and although someone—I think it was Mr. Esson—said he was afraid that heating would result, we have not experienced any ; in fact, it is the coolest dynamo I have ever seen, probably on account of its great size, and the amount of air space between the bobbins : everything is so open and free of access to the air,

and all the iron acted upon electrically is laminated as much as Mr.
Mackenzie. possible.

Mr. W. M. MORDEY: What is the size?

Mr. KENNETH MACKENZIE: 50 horse-power.

Mr. W. M. MORDEY: For what?

Mr. KENNETH MACKENZIE: It is a 3,000-volt and 12-ampère machine, designed for central station lighting. It is, of course, so far experimental; but we wanted to see on a practical scale what the idea is worth, and put it into the hands of an engineering firm, not electrical engineers, to build.

I think, when we have to use dynamos of any size in central stations, that they should not be quick-running machines, and that the idea of a wireless armature is a good one.

Mr. Kingdon, in stating that the fly-wheel of an engine would form part of his machine, did not intend to convey the impression that existing engines were preferably to be so altered, but that in designing a dynamo for a central station the fly-wheel of the engine—necessary in any case to ensure steady running—would be made to form part of the dynamo, and thus combine two functions in one. The object of arranging the machines into various circuits, so as to admit of alterations in the coupling, is obvious, since bobbins can be cut out of circuit as the output diminishes, thus working the remainder at their maximum. There are many reasons why it would be advantageous to be able to alter the connections of the bobbins, but the want of time prevents me going into the matter.

In answering Mr. Crompton's request to give him information regarding the efficiency of the transformer system, I think that Mr. Crompton is at present asking for rather too much. As regards the saving of copper used by the employment of this system, reference to the diagram on the wall (Fig. 22) will make this clear; and I think that many of the objections that have been raised by Mr. Crompton have been met and successfully answered by several speakers this evening. Mr. Crompton, in his remarks, stated that if it were possible to run alternating-current machines in parallel, working only a few when the supply was small, and throwing fresh machines into the line to meet the increased

Mr.
Mackenzie.

demand, then "the bottom was knocked out of his argument."
Mr. Mordey quite rightly stated that this was done, and done in
practice with perfect results; so Mr. Crompton may consider his
argument to be bottomless! Generally speaking, the method
adopted in the States is as follows:—Two glow lamps are, on the
switch-board, connected to the two circuits, one to the dynamo or
dynamos supplying the line, and the other to the machine to be
thrown in. If the phases of the two supplies are dissimilar, the
lamps will at a given moment be of unequal brightness, each
varying from full brightness to nothing—out, in fact—and their
periods of variation will be closer or farther apart according as
the speed of the two supplying circuit machines. Now, depending
upon the poles of the dynamos and how they are to be connected,
all that is necessary is to wait until the two lamps are either both
out or both at full brilliancy at the same time, and at that period
they show that the phases are equal; and the fresh machine, on
being switched into line, drops naturally and quietly into its
proper position, without, I believe, a "blink" on the lamps. This
is a rough idea of how it is done, and I hope Mr. Crompton will
now think a little better of the capabilities of the alternating-
current transformer system.

There is no gainsaying the fact that the transformer system
has made tremendous way, and this would never have been the
case had not the system possessed merits placing it before all
others for the general distribution of electricity. I quite expect
to see Mr. Crompton before very long becoming as warm an
advocate of the transformer system as he was not many years ago
an opponent of electrical distribution by secondary batteries.

Mr. Gordon has gone very fully into the relative merits of
the two systems, but he has been answered by several gentlemen
who have joined in the discussion this evening, and I am afraid
I cannot altogether agree with the conclusions Mr. Gordon has
arrived at.

Professor Silvanus Thompson, as well as Mr. Mordey and
Mr. Madgen, have called attention to my statement on page 1
regarding the Faradaic theory. I wish to point out that the
words " *and* exemplified by Ruhmkorff" should be " *as* exem-

" plified by Ruhmkorff;" M. Gaulard never having dreamed of Mr. Mackenzie.
denying the Faradaic theory of induction, as such would have
been absurd on the face of it ; but he distinctly laid down the
dictum that the Ruhmkorff coil, more or less modified, formed
the basis upon which all previous experimenters had worked,
and had consequently. been utterly unsuccessful.   Regarding
Mr. Madgen's mention of Jablochkoff, I deny that that electrician
ever conceived the idea of a transformer system as it now exists.
I quite recollect the apparatus exhibited by M. Jablochkoff in
Paris at the Great International Electric Exposition of 1881 ;
but that system then shown at work as a "transformer system"
was no more—either theoretically or practically—like what
Gaulard discovered than a Wimshurst machine is like a dynamo,
and I do not think the subject worth discussion.

Mr. Kent asked for an explanation of my statement that
" the secondary current increases as the *square* of the ampère
" value of the primary."   This was the conclusion arrived at by
M. Gaulard in his early experiments, and it was upon his
authority that that assertion was put forth.   If such be proved now
not to be the case with existing transformers, it may be due to
the fact that there was less iron in the original Gaulard apparatus ;
and what iron there was, was magnetised to saturation.

I referred in my paper at that time to transformers working
in series, and although Mr. Kent has questioned my statement,
he has not said *how* I was wrong.   I believe myself that it
depends upon the number of ampère *turns* in each coil, but
I would be glad to know what is the true state of the case as
proved by recent investigation.

Professor Thompson has objected to my statement that when
one transformer only is connected to a dynamo it may be
considered as "either in parallel or series ;" but I do not see how
anyone can well prove the contrary, in spite of the argument
brought forward by the Professor, since, like a "quarrel," it takes
more than *one* to make a parallel or series circuit.

Seeing the hour is so very late, I am unable to say more in
answer to the remarks ' made by the numerous gentlemen who
have joined in the discussion.

The PRESIDENT: Gentlemen, votes of thanks have already been given to the authors of these papers, and, considering the extreme lateness of the hour, I will only say that we are very much obliged to them for having called attention to a subject the importance of which cannot be exaggerated, and to which our attention has not been previously called by the reading of a paper. The papers have given rise—as such papers are sure to do—to an interesting discussion, and have brought out differences of opinion from which, perhaps, much good may be afterwards gained.

A ballot took place, at which the following candidates were elected:—

Member:
Major-General E. Festing (late R.E.), F.R.S.

Associates:

Sir Archibald C. Campbell.

Edward Tremlett Carter.

Edwin James Howell.

J. T. Niblett.

William Henry Patchell.

Frederick Pollard.

Percy James Rea.

Bernard P. Scattergood.

Students:

Robert Carpenter Clay.

Leonard Joseph Healing.

Edward Loraine Heelis.

Walter Langdon-Davies.

Ernest Gordon O'Kell.

Walter Alfred Poynton.

Herbert Walter Ridley.

Alfred D. Williamson.

The meeting then adjourned until March 8th.

ORIGINAL COMMUNICATION.

EARTH CURRENTS IN INDIA.

By E. O. Walker, Member.

An endeavour was made in 1884 to account for the existence of these currents by taking simultaneous observations of temperature at the opposite ends of a telegraph line, the hypothesis being that the absolute difference of temperature of the junctions of the earth plates and the ground at any moment caused the electro-motive force. A paper contributed to the *Journal*, under date the 16th September, 1884, gives the results of the writer's observations up to that time. He now contributes the results of fresh observations, taken at Calcutta on the telegraph line running mainly north-west to Allahabad—distance, 577 miles. It
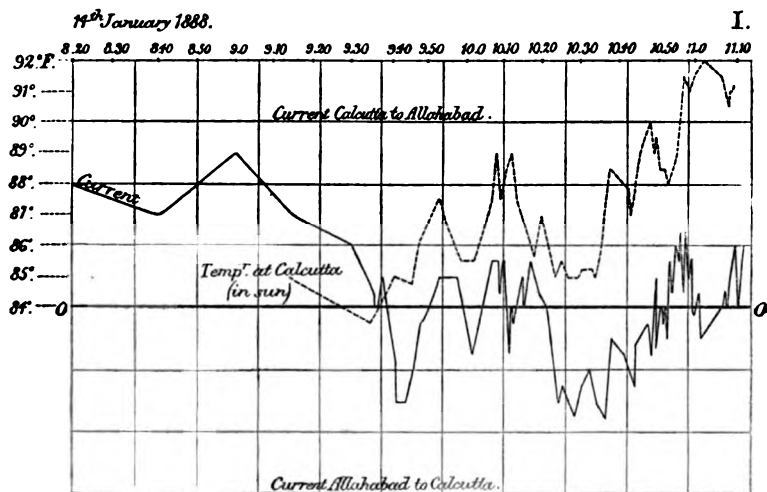
FIG. 1.

was recognised that, to be of any real value, observations should be as nearly continuous as circumstances would allow, and Diagram No. II. gives results of observations taken every half-minute. Continuous photographic traces taken at the two ends of a line

synchronously would be invaluable, but unfortunately the appli-
ances do not exist in telegraph offices in India to carry this out.
Thermometrical and earth-current observations should of course

FIG. 2.

be simultaneous, judging from the observations set forth in the
annexed plates.

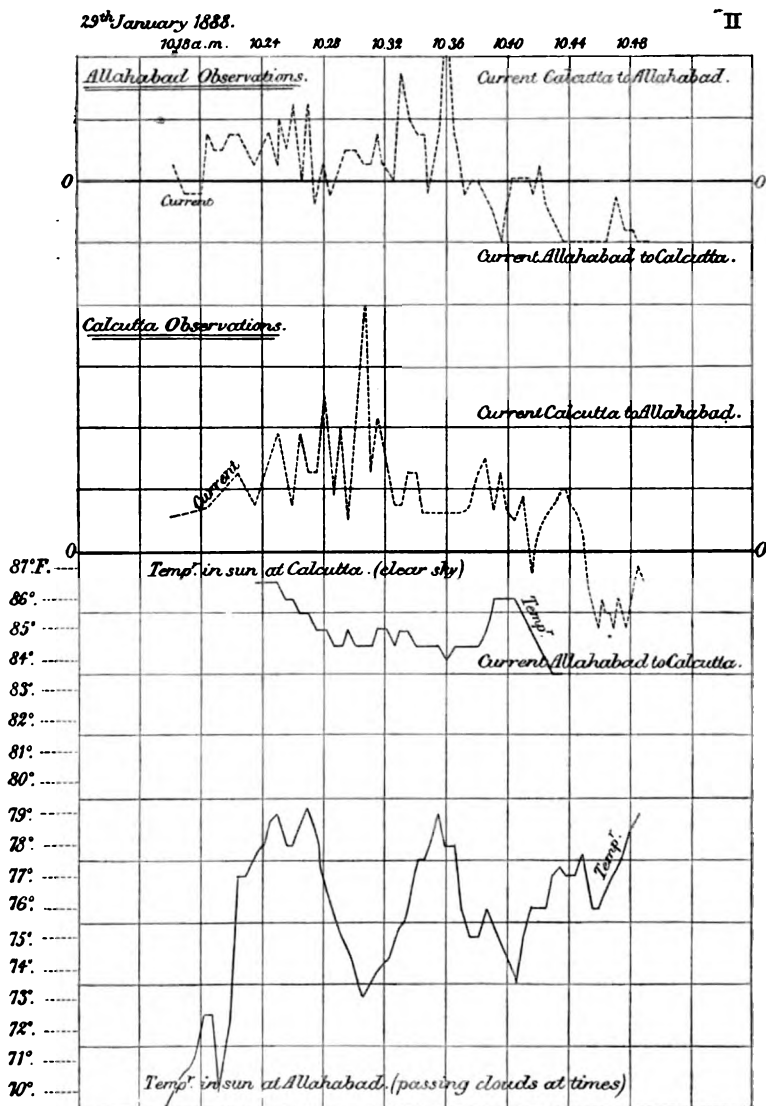It must have come under the notice of many who have observed these currents that they will flow in opposite directions from the place of observation to places, say, east and west, some perplexity being thus introduced when it was attempted to account for their origin on the theory of magnetic induction by the sun, or by the lagging behind of magnetic matter during revolution of the earth on its axis. Such perplexity is very much cleared up when the relative atmospheric conditions of the places of observation are studied, and when it is seen that these currents are within limits purely local. The condition necessary for their generation seems to be that two places should have different densities of atmosphere—a condition which, to a great extent, always exists between places far apart east and west. The writer had shown how marked and regularly recurrent the currents were between Belgaum (on an elevated plateau, 2,500 feet high) and Vingorla, on the coast line, in the paper above referred to; and there is no doubt that, had minute and continuous thermometrical observations been taken at those places, the theory now suggested would then have been brought out.

The generation of the electro-motive force seems, then, to lie, not in the absolute difference of temperature at any moment, but in the *difference of the rate of change of temperatures*. This difference, which represents a changing molecular condition of the wires, earth plates, and soil in the two places of observations, appears to determine the direction and amount of the current. It can be conceived that since a current itself is capable of producing molecular strain in wires—indeed, in all materials through which it is passing—which relieves itself on the cessation of a current, and is demonstrated in what is known as reverse E.M.F., so that precise molecular condition may be produced by different rates of vibration of heat rays throughout the length of a telegraphic circuit, and an electro-motive force thus set up. Absolute difference of temperature will of course always be productive of some current, but it must, in ordinary conditions of temperature, be very small. The maximum electro-motive force usually observed on the Calcutta-Allahabad lines is 3 volts. (Each $\frac{5}{16}$ of an inch in the diagrams, taken vertically, represents approxi-

mately 1·5 volt.)   On this thermal hypothesis a lunar influence
(brought out in Mr. Adams' papers, vol. xii., No. 46 of *Journal*)
is admissible, and the changes of direction of the current in
submarine cables with changes of the tide are susceptible of
explanation.   In the latter case the relative density of the
medium through which the heat rays pass would of course
influence the electro-motive force.   Should this direct conversion
of heat into electricity actually take place in the case of telegraph
circuits, a little consideration must show that it will also exist in
the crust of the earth and in the atmosphere.   The variations
of the magnetic elements will then be, to a great extent,
explained.

Observations in the early morning and forenoon in India, so
far as the observations of the writer go, seem to warrant the
assertion that over a considerable area currents flow in the
telegraph circuits from east to west, and that for a portion of the
afternoon at least they flow from west to east, though from
a limited and contracted research this statement cannot too
confidently be made.   Here, as elsewhere, large disturbances
are observed in connection with earthquakes and sun-spots,
and would, if the earth currents have a thermal origin, be
expected.   Accurate records would witness to the fact of ther-
mometrical variations, no doubt, simultaneous with changes of
the currents.

The observations plotted in the diagrams were taken with a
tangent galvanometer of the Indian Telegraphs pattern.   It was
thought that the inertia of the needle might be such that a
sudden change of the current in direction might not be noticed :
a Thomson's reflecting galvanometer was therefore inserted for
one day's observations, and this supposition was found to be
wrong.   The change of force is gradual, and allows time for
observation even with a sluggish needle.   The rate of change was
found to move the needle through 5° in one second.   (The Indian
Telegraph tangent galvanometer is here referred to ; it is super-
fluous to give the dimensions of its parts here.)

The needles of the observing instruments are in constant
vibration, and it is impossible by aid of the eye to record all the

pulsations they experience, nor were the thermometers delicate enough to show the same, but it is quite easy to record all their marked changes. Not so easy was it to obtain exact synchronism in the observations at Allahabad and Calcutta recorded in Diagram No. 2, and the writer is aware that many small discrepancies must have crept in from difference in the time of the watches used, and from errors of observation on the part of the four observers engaged. For these he must ask to be excused, and would conclude by suggesting the great importance that exists of establishing a concurrent series of photographic trace observations of earth currents and of thermometrical readings at a few places telegraphically connected, to evidence more clearly and thoroughly the very close relation that exists between the two, and to place the theory of the origin of these currents on an intelligible basis.

CALCUTTA, 31st *January*, 1888.

NOTE.—Mr. Syme, Telegraph Master, assisted at Allahabad.

ABSTRACTS.

H. LORBERG—NOTE ON THE THEORY OF DYNAMO MACHINES.

(Annalen der Physik und Chemie, Vol. 32, Pt. 3, No. 11, 1887, pp. 521–26.)

In a previous paper the author had calculated the currents, induced in the mass of the armature of a dynamo, which, where the armature core and coils are rigidly connected, are induced partly by the reversal of the current at the two neutral points of the winding, partly by the fixed electro-magnets; or, if the armature core alone rotate, which are induced by the electro-magnets alone.

The expressions for the impressed E.M.F. in the two cases are quite different, and the author concluded that the law of Clausius, viz., that the impressed E.M.F. when armature core and coils are rigidly connected is equal to the E.M.F. when the core alone rotates, was incorrect.

Clausius has reasserted the correctness of his view, and says if the coils alone rotate there is a mean value of the E.M.F. $= E'_x + E''_x$, where E'_x depends on the rotation, and E''_x on the change of direction of the current. The mean value of the E.M.F. induced in one single winding must be equal to zero for a complete revolution; consequently, therefore, $E'_x + E''_x = 0$, or, since $_J = - E_x, \therefore - E_x + E''_x = 0.$

A consideration of the laws of induction as given by Maxwell would show, however, that the conclusions of Clausius are correct.

G. QUINCKE—ABNORMAL PHENOMENA IN DIELECTRIC FLUIDS, ESPECIALLY IN COLZA OIL.

(Annalen der Physik und Chemie, Vol. 32, Pt. 4, No. 12, 1887, pp. 529–45.)

The paper relates to four points, viz.—the different values obtained for the same fluid with different methods of measurement; the peculiarities in the specific inductive capacity of colza oil; the electric refraction of chloroform; and the departures from Maxwell's law of electro-optics.

The specific inductive capacities of fluids can be measured in any of the following three ways :—

(a) By comparing the capacity of a condenser the plates of which are either in the fluid or in air.

(b) By comparing, by means of the electric balance, the attraction of two condenser plates in the fluid under observation and in air.

(c) By noting the increase of pressure of an air-bubble, which touches both horizontal condenser plates in the interior of the insulating fluid, as soon as they are charged to a known potential.

Except in the case of colza oil, the methods (b) and (c) gave the same values; but (a) came out rather lower. In this method, as Hopkinson has pointed out, it is necessary to take into account the capacity of the connecting wires and of the charge-key. Making this correction, the author finds that the specific inductive capacities of insulating fluids come out to the same value, whichever of the three methods is adopted for experimenting.

As already stated, colza oil is a remarkable exception to this uniformity. Each method gives a different value for the specific inductive capacity: the pressure method gives the highest value, the electrical balance the smallest, and the condenser method a mean value. This anomaly was noted in experiments with more than one kind of colza oil. It cannot be explained by the assumption that a slight electrolysis of the oil takes place. The products of decomposition on the condenser plates could not alter the capillary pressure of the common surface of air and oil, as measured on the bisulphide of carbon manometer. It is also very improbable that by such a decomposition the potential difference of the condenser plates could be altered from 2,700 to 4,000 volts.

It was found to be very difficult to get chloroform to insulate well; but the experiments made show that chloroform has a refractive index which is almost as great as that of bisulphide of carbon, but is of opposite sign.

According to Maxwell's electro-magnetic theory of light, the specific inductive capacity of an insulating body should be equal to the square of the index of refraction for the ultra-red rays. The author has attempted to measure the index of refraction of pure ether for the ultra-red rays by means of a thermopile, by the method of Becquerel and Lommel, and by an arrangement giving two refractions and one reflection, as is the case in a rainbow. He finds that pure ether has a specific inductive capacity of 4·3, and for ultra-red rays a refractive index less than 2; consequently this liquid does not follow Maxwell's law.

JAMES MOSER.—THE SEPARATION OF THE E.M.F. OF CELLS INTO THEIR POTENTIAL DIFFERENCES.

(*Beiblätter, Vol.* 11, *No.* 11, 1887, *p.* 788.)

The E.M.F. of a cell consisting of zinc, dilute zinc chloride, concentrated zinc chloride, zinc was found by direct measurement to be 0·15 volt. By means of electrodes consisting of drops of mercury the absolute value of the potential difference at the common surface of the metal and dilute solution was found to be 1·10 volt, and between the metal and concentrated solution 0·68 volt. The potential difference between the two liquids was 0·27 volt. Hence the total calculated E.M.F. is 1·10 − 0·27 − 0·68 = 0·15 volts, which agrees with the value found by direct measurement.

In a Daniell cell the potential difference between zinc and zinc sulphate = 198; between zinc, zinc sulphate, copper sulphate = 232; between copper

and copper sulphate = 40; therefore the total E.M.F. is 232 — 40 =192, the value obtained by direct measurement.

A Latimer Clark cell gave the potential difference of zinc and zinc sulphate = 183; of mercury, mercurous sulphate, zinc sulphate = 62; therefore the total 183 + 62 = 245, or 1·43 volt, which agrees very closely with the exact value.

G. GUGLIELMO—ELECTRIC LEAKAGE IN MOIST AIR.

(Beiblätter, Vol. 11, No. 11, 1887, pp. 793–95.)

The experiments were made with a Leyden jar with large knob 3 cm. in diameter. The jar, having been charged to a certain potential by means of a unit jar, was left in the air or in steam for an equal length of time, and then tested by a spark-discharger with micrometer gauge. When left in air the length of spark only decreased a few tenths of a millimètre, but in steam it decreased from 1 to 2 millimètres. The same result was obtained if the knob was moderately warmed; but if warmed to 300°, the loss of charge in the steam was scarcely perceptible.

Experiments made with a Coulomb's torsion balance, in which the fixed sphere was replaced by a brass rod capable of being surrounded by steam, seemed to prove that the moist air insulated as well as the dry air up to a potential of 600 volts; at higher potentials the loss of charge in moist air is greater the nearer the steam is to its point of saturation.

HEIM—INTENSITY AND CONSUMPTION OF THE ORDINARY SOURCES OF LIGHT.

(Bulletin de la Société Internationale des Electriciens, Vol. 5, No. 44, Jan., 1888, pp. 23–34.)

The results of the experiments are of great value, being made by one person with the same apparatus.

The unit of light adopted for comparison was the standard English candle, with a flame 45 mm. high. Various petroleum lamps were used as intermediate standards; their constancy having been first well proved, and all precautions taken for their regular and equal burning. The intensity of the lights used was measured under various angles by means of a Krüss mirror, the absorption being carefully determined for light of various colours.

Each light was generally examined on three different days. The comparison with the standard candle was made at the beginning and end of each experiment.

1. *Petroleum Lamps.*—The oil used was in most cases the refined kind, so-called " Imperial petroleum; " the specific gravity being 0·796 at 18° C. The amount consumed was determined by carefully weighing the lamp before and after each test.

TABLE I.

NAME.	Diameter of Burner.	Angle with Horizon	Standard Candles.	CONSUMPTION.		Remarks.
				Per Hour.	Per Hour per C.P.	
	mm.			grm.	grm.	
Ordinary round ...	25	0°	16·1	54·2	3·37	
Do. ...	25	45°	12·3	53·6	4·36	
Round, with flame spreader, small	30	0°	19·2	63·4	3·30	
Do. ...	30	45°	11·1	61·1	5·51	
Do. large ...	62	0°	67·3	229·0	3·40	
Do. ...	62	45°	33·9	228·0	6·70	
Cosmos-Vulcan ...	30	0°	22·9	84·9	3·70	} American petroleum.
Do. ...	30	45°	17·8	85·5	4·80	
Do. ...	30	0°	22·8	81·1	3·59	{ Imperial petroleum.

From this table the following conclusions may be drawn:—When the light is measured horizontally, all petroleum lamps are about equally good, whatever may be the differences of construction, and increase in the size of the burner does not increase the intensity for equal consumption. If the measurements are made at an angle of 45°, the intensity is less the greater the diameter of the burner. The best efficiency is given by the ordinary round burner with a flame 70 to 80 mm. high; at an angle of 45° the consumption of oil per candle per hour is only 29 per cent. more than for a horizontal measurement, whilst in case of the bigger burners at 45° about twice as much oil per candle-power is wanted as for horizontal measurements. The superiority of the Cosmos-Vulcan lamp under an angle of 45° is due to the flame being much spread. The use of refined oil does not seem to have an appreciable influence on the efficiency.

The following small table shows how the efficiency diminishes if the ordinary round-wick lamp is not burned at its full power. The measurements were horizontal.

TABLE IA.

Candle-Power.	Consumption per Hour.	Consumption per Hour per C.P.
18·9	58·4 grm.	3·09 grm.
16·8	56·0 ,,	3·33 ,,
15·0	54·8 ,,	3·65 ,,
12·7	50·7 ,,	3·99 ,,

At the present time there is no lamp in which the petroleum is so economically consumed as in the regenerative gas-burners.

2. *Gas-Burners.*—The quantity of gas burned was determined every quarter of an hour by means of wet meters. The regenerative burners were burned for half an hour at full power before being measured; the small burners long enough to reach a normal degree of heat. All experiments were made early in the day, so as to avoid variations of pressure in the mains. The gas used

was that made at the Hanover Works, which is very constant in its composition. Due care was taken to ensure efficient ventilation.

TABLE II.

NAME.	Angle.	Candle-Power.	CONSUMPTION IN CUB. FEET.		Remarks.
			Per Hour.	Per Hour per C.P.	
Fishtail	0°	16·9	8·864	0·5227	(So-called 6 cub. feet per hour.)
Do.	45°	17·2	9·041	0·5262	
Argand...	0°	21·9	8·441	0·3849	
Do.	45°	19·5	8·511	0·4379	
Auer's Incandescence ...	0°	14·4	3·359	0·2331	} Supplied } by Pintsch.
Do. do. ...	45°	10·5	3·662	0·3489	
Siemens' Regenerative, No.3	0°	65·3	16·245	0·2490	
Do. do. do.	45°	66·9	16·104	0·2737	
Do. do. No.1	0°	222·0	57·247	0·2578	
Do. do. do.	30°	162·0	57·000	0·3518	
Do. do. do.	45°	132·0	56·646	0·4309	
Wenham, No. 2	0°	28·4	8·794	0·3097	
Do. do.	30°	44·5	9·076	0·2038	
Do. do.	45°	45·8	9·041	0·1971	
Do. No. 4	0°	99·0	24·191	0·2444	
Do. do.	25°	152·0	24·226	0·1593	
Do. do.	45°	170·0	23·909	0·1406	
Do. do.	65°	200·0	24·191	0·1208	
Do. do.	90°	202·0	23·697	0·1176	

The flat burner was placed across the photometer axis. The use of the regenerative is clearly to increase the efficiency, as is shown by the last column of figures; the Argand is better than the fish-tail, the Siemens better than the Argand. It would seem that the efficiency of the Siemens burner is not increased in the larger sizes. The loss of effect as the horizontal position is departed from in the case of the Argand and Siemens burners is due to a portion of the flame being masked by the body of the burner. The contrary effect is of course noticed in the Wenham lamp, where the flame is below the body of the burner; its greatest brilliancy is vertically downwards, but the decrease of intensity is not very considerable until after passing the angle of 45°.

3. *Arc Lamps.*—In order to obtain a uniform arc, the necessary current was supplied by a compound-wound machine having an E.M.F. of 100 volts, a resistance being introduced to absorb the excess. The upper carbon was in each case a core carbon, the lower one being solid

TABLE III.

Name.	Normal Current	Diameter of Carbons.	Length of Arc.	Angle with Horizon	Standard Candles	Watts.	Watts per C.P.	C.P. per H.P.
Piefer	4	More than 6·7 mm.	mm. 2	0°	126	160	1·270	433
Do.	4	Less than 5·0 mm.	2	45°	377	153	0·405	1,360
Piette-Krizik ...	8	10 ,,	4	0°	220	414	1·880	293
Do.	8	10 ,.	4	45°	1420	413	0·291	1,890
Siemens ...	20	14 ,,	4·5	0°	575	918	1·600	344
Do. ...	20	14 ,,	4·5	45°	3830	912	0·236	2,310

The numbers in the last column are calculated on the assumption that the dynamo had an efficiency of 75 per cent. The measurements made under an angle of 45° are the only really reliable ones, owing to the great fluctuations in the light as measured in a horizontal plane. The table is a confirmation of the generally received opinion that small arc lamps are less efficient than large ones. The efficiency may be increased by increasing the length of the arc, but this is at the expense of the steadiness of the lamp.

4. *Glow Lamps.*—The experiments were made with the types most commonly used in Germany; the size chosen being the 16-c.p., with 100 volts potential difference. The measurements were only made in a horizontal plane, as the diminution in brilliancy is small up to 45°. The plane of the filament was placed at right angles to the photometer bank, except in the case of the old-type Edison lamp.

TABLE IV.

Name.	Actual C.P.	Watts.	Watts per C.P.	C.P. per H.P.	Lamps per H.P.
Edison, old type	16	72	4·50	122	7·6
Do. new do.	16	60	3·75	147	9·2
Swan, old type	16	66	4·13	133	8·3
Do. new do.	16	56	3·50	157	9·8
Siemens & Halske	16	52	3·25	169	10·6
Bernstein	16	56	3·50	157	9·8

5. *Magnesium Lamps.*—An experimental lamp of a new type was submitted for testing. As these lamps are not much known in England, it may be well to say that they consist generally of a small clockwork which unrolls the coiled-up magnesium ribbon and advances it through the centre of a reflector at a rate which can be regulated to equal the rate of consumption. The lamp tested could burn any number of ribbons up to eight, their size being 2·5 mm. wide by 0·1 mm. thick. The light is not quite steady, but the variations are

gradual, and not sudden as in an arc lamp. A ventilator was provided for the escape of the white fumes of magnesia produced by the combustion of the metal. The lamp, as is usual, was provided with a fixed reflector; but, in order to test the power of the flame alone, a dull black paper was placed over the reflector in some of the tests. The consumption of magnesium was determined by weighing the lamp before and after each test. A few measures made at 33° below the horizontal plane showed a decrease of 25 per cent.

TABLE V.

No. of Ribbons.	Candles without Reflector.	Candles with Reflector.	Candles per Ribbon without Reflector.	Consumption of Magnesium per Hour per Ribbon without Reflector.	Consumption per Hour per Ribbon for 100 C.P.
1	150	3,200	150·0	16·7 grammes	11·14 grammes.
2	237	5,880	118·7	,, ,,	14·10 ,,
4	450	8,000	112·5	,, ,,	14·80 ,,
6	700	11,300	117·0	,, ,,	14·15 ,,
8	950	17,000	119·0	,, ,,	14·03 ,,

The candle-power per ribbon does not seem to vary whether two or eight are used; the tests with four ribbons probably contain some error.

The price of magnesium ribbon may be taken at 19s. 6d. per lb. With eight ribbons burning the lamp consumes about 4¾ oz. per hour, at a cost of 6s. per hour; the 100 standard candles, without reflector, would cost about 8d. per hour. In the lamp tested the ribbon was advanced at the rate of 32 mm. per hour; but as good an effect can probably be obtained by burning 24 mm. per hour. On the other hand, the price of magnesium is likely to fall to about 13s 6d. per lb., in which case the price of 100 standard candles would be about 4d. per hour.

H. VILLENEUVE—AN EXTRAORDINARY LIGHTNING–STROKE.

(Annales Télégraphiques, Vol. 14, July—Aug., 1887, pp. 346-81.)

The damage occurred on the 24th April, 1887, at Mortrée (Orne), at the junction of four cross-roads. The telegraph wires running along the north-south road were entirely destroyed for a distance of 150 yards, the centre of the damaged portion being just opposite the east-west road. The poles and insulators were not damaged; and only very slight marks could be found on one of the elm trees which border the road on either side. The line had been pegged to earth at the telegraph station, but an explosion happened in the battery.

The lightning also struck a corner house, and pieces of plaster were projected across the road against the opposite house. Behind the latter a man was milking a cow in the stable when he observed a fire-ball enter the stable, pass between the legs of the cow, and disappear without doing any material

damage, though it would seem that the moral damage was great to both man and cow.

A quantity of fragments of incandescent stone fell at the four cross-roads; some of these could be crushed between the finger and thumb, and gave out a smell of sulphur.

LAGARDE—TELEGRAPHIC LIGHTNING-DISCHARGERS.

(*Annales Télégraphiques*, Vol. 14, Sept.—Oct., 1887, pp. 419-27.)

A Ruhmkorff coil is connected to a battery of six Leyden jars, the inner coatings of which communicate, through a spark-discharger provided with knobs, with a Riess air-thermometer and the lightning-discharger under trial, the two being in parallel, and the circuit being completed to the outer coatings.

When the knobs of the spark-discharger are at a fixed distance apart, the same quantity of electricity will always pass. The passage of the current through the air-thermometer alone heats the platinum wire to a certain degree; when it passes also through the lightning-discharger, the platinum wire is heated to a less degree; so that the difference between the two readings is a direct measure of the efficiency of the lightning-discharger.

Eight different dischargers were tested. The first was of the Bertsch type as used on the French lines, and composed of two metal plates 118 mm. long by 70 mm. wide, and 19 mm. apart; each plate is fitted with 292 metal points 9 mm. long, so that the points are 1 mm. apart. The next three dischargers consisted of brass plates of the same size as the above, but with perfectly smooth surfaces; in the first of the three the plates were 1 mm. apart, in the second 0·5 mm., and in the third 2 mm. Two other dischargers similar to the first of the group of three were.tested in which the opposed surfaces were one-half the size; also a discharger with striated surfaces instead of smooth ones, the striae on one being at right angles to those on the other. The last form experimented upon consisted of six metal points opposite a metal plate and 1 mm. distant therefrom.

The conclusions which the author draws from his experiments are that the Bertsch double-point discharger is the worst of all, and that the two metal plates with smooth surfaces 1 mm. apart give the most perfect protection.

LIST OF OTHER ARTICLES

ELECTRICITY AND MAGNETISM,

Appearing in some of the principal Technical Journals during the month of
JANUARY.

(*Philosophical Magazine, Vol. 25, No. 152, January,* 1888.)

T. H. BLAKESLEY—Geometrical Determination of the Conditions of Maximum Efficiency in the Case of the Transmission of Power by means of Alternating Currents. **H. TOMLINSON**—Effect produced on the Thermo-electrical Properties of Iron when under Stress or Strain by raising the Temperature to Bright Red. **Professor H. LAMB**—Theory of Electric Endosmose and other allied Phenomena.

(*Annales Télégraphiques, Vol. 14, July—August,* 1887.) '

SÉLIGMANN-LUI—Maxwell's Theory of Electricity. **VALLANCE**—Telegraph Lines in Tonkin. **E. MERCADIER** — Theory of the Telephone. **VASCHY**—Action of an Electrostatic Field on a Variable Current. **VASCHY**—Electro-capillary Phenomena. **E. BOUCHARD**—Optical Telegraph. **J. CARPENTIER**—New Form of Electrometer.

(*September—October,* 1887.)

J. RAYNAUD—Biography of E. E. Blavier (*continued*). **GIDEL**—Electric Light Installation at St. Etienne.

(*Journal Télégraphique, Vol. 12, No. 1, 25th January,* 1888.)

Dr. ROTHEN—Telephony (*continued*). **M. P. SANTANA**—New System of Duplex Telegraphy with Ordinary Morse Instruments and Keys. **E. LACOINE**—Testing a Conductor traversed by a Current, by the False Zero Method. *Anon.* — Japanese Telegraphs from 1st July, 1885, to 31st March, 1886.

(*Bulletin de la Société Internationale des Electriciens, Vol. 5, No. 44, January,* 1888.)
LEMONNIER—Passage of the Suez Canal at Night.

(*Journal de Physique, Vol. 7, January,* 1888.)

A. LEDUC—Variable Period of a Current in the Circuit of a Faraday Electro-Magnet.

(*Comptes Rendus, Vol.* 106, *No.* 1, *2nd January,* 1888.)

A. CORNU—An Objection to the Use of Electro-magnetic Dampers in the Synchronisation of Clocks. **C. WOLF**—Reply to above. **AIMÉ WITZ** —The Energy necessary to Create a Magnetic Field, and to Magnetise Iron.

(*No.* 2, *9th January,* 1888.)

C. WOLF—Synchronisation of Pendulums: Reply to Mr. Cornu. **A. CORNU**—Regulation of an Electric Current giving a Fixed Amplitude to Synchronised Oscillations. **P. LEDEBOER**—Effect of Temperature on the Magnetisation of Iron.

(*No.* 3, *16th January,* 1888.)

A. CORNU—Note on Mr. Wolf's Paper (see *supra*). **PAUL JANET**— Application of Transversal Magnetisation to the Study of the Coefficient of Magnetisation of Iron.

(*No.* 5, *30th January,* 1888.)

R. BLONDLOT—Double Dielectric Refraction. **G. MANŒUVRIER and P. LEDEBOER**—Use of Electro-Dynamometers for Measuring the Mean Value of Alternating Currents.

(*La Lumière Electrique, Vol.* 27, *No.* 1, *7th January,* 1888.)

G. RICHARD—Construction of Dynamo Machines. **E. ZETSCHE**—Line Tools for Silicium-Bronze Wires. **E. DIEUDONNÉ**—Copper Accumulator. **B. ABDANK ABAKANOWICZ**—Integrating Machines.

(*No.* 2, *14th January,* 1888.)

C. DECHARME—Constant Deflection Galvanometer. **F. DROUIN**— Arrangement to avoid Parallax in Dial Instruments. **J. BORGMANN** —Transmission of an Electric Current through Air. **A. PALAZ**— Transmission of Power for Working Pumps in Mines. **J. WETZLER**— New Measuring Instruments of the Edison Co., U.S.A.

(*No.* 3, *21st January,* 1888.)

E. DIEUDONNÉ—Portable Electric Light Plant. **J. BORGMANN**— Transmission of an Electric Current through Air (*continued*).

(*No.* 4, *28th January,* 1888.)

A. PALAZ—Photometric Standards. **E. WÜNSCHENDORFF**—Submarine Telegraphy. Part V. **E. DIEUDONNÉ**—Magnetisation of Steel Bars. **G. RICHARD**—Electric Furnaces. **J. BORGMANN**—Transmission of an Electric Current through Air (*continued*).

(*Annalen der Physik und Chemie, Vol.* 33, 1888, *No.* 1.)

F. HIMSTEDT—A New Determination of "*v.*" **E. COHN and L. ARONS** —Specific Inductive Capacity of Conducting Liquids. **E. COHN and L. ARONS**—Addendum to Article on " Conductivities and Specific

"Inductive Capacities." **F TOMASZEWSKI** — Specific Inductive Capacity of Liquids. **W. KOHLRAUSCH**—Relation between Magnetic Permeability and Electric Conductivity in Different Sorts of Iron and Nickel. **K. HARTWIG**—Conductivity of Solutions of Fatty Acids in Water and in Alcohol. **C. FROMME**—Maximum Polarisation of Platinum Electrodes in Sulphuric Acid. **A. v. ETTINGSHAUSEN**— Note on the Article "A New Polar Working of Magnetism on Galvanic Heat in some Bodies." **A. v. ETTINGSHAUSEN**—Effect of Magnetic Forces on the Nature of Heat Conductivity in Bismuth. **C. FROMME** —Maximum of Temporary Magnetism. **C. FROMME** — Abnormal Magnetisation. **W. v. ULJANIN**—Note on Exner's Paper on the Contact Theory.

(No. 2)

E. WIEDEMANN and H. EBERT—Effect of Light on Electric Discharge. **G. MEYER**—Thermal Changes of Daniell Cells and of Accumulators. **F. NARR**—Conduction of Electricity by Gases. **W. HALLWACHS**— Effect of Light on Electrostatically-charged Bodies. **F. STENGER**— Absolute Measure of Magnetic Fields. **F. BRAUN**—Explanation of Diamagnetism.

(Beiblätter, Vol. 11, No. 12, 1887.)

L. DONATI—New Form of Quadrant Electrometer. **G. GRASSI**—Method of Calibrating a Galvanometer. **P. MOENNICH**—Differential Inductor for Measuring Resistances. **A. BATTELLI**—Thermo-Electricity of Mercury. **A. BATTELLI**—Thermo-Electricity of Amalgams. **W. OSTWALD**—Compensation Electrometer. **L. HERMANN**—Polarisation between Electrolytes. **L. DONATI**—A New Accumulator. **L. BOLTZMANN** — Thermo-chemical Law of Pebal relating to Non-reversible Electrolytic Processes. **A. v. ETTINGSHAUSEN**— Alteration of Resistance of Bismuth, Antimony, and Tellurium in a Magnetic Field.

(Vol. 12, No. 1, 1888.)

J. LEHMANN—A Glass Condenser. **J. KLEMENCIC**—Mica as a Dielectric. **A. HANDL**—Grouping of Cells. **G. P. GRIMALDI**— Electric Resistance of Sodium and Potassium Amalgams. **A. FLEMING** —Galvanometer for Alternate Currents. **D'ARSONVAL**—Very Sensitive Galvanometer. **G. P. GRIMALDI**—Effect of Magnetism on the Thermoelectric Properties of Bismuth. **G. P. GRIMALDI**—The Thermomagnetic Experiments of v. Ettingshausen and Nernst. **F. MAGRINI**— A Paradoxical Case of Electro-dynamic Induction. **R. F. PITONI**— Electric Condition induced in a Rotating Plate or Ball by a Magnet. **E. SEMMOLA**—Heating of Metal Points during an Electric Discharge. **F. UPPENBORN**—E.M.F. of the Electric Arc.

(*Elektrotechnische Zeitschrift, Vol. 9, Pt. 1, January,* 1888.)

W. SIEMENS — Glow Lamps in Series. *Anon.*—Line Tools and Line Erection.

(*Pt. 2, January,* 1888.)

GÖRZ—Electric Light in Theatres. **Dr. WEINSTEIN**—Calculation of Resistance of Mercury Columns. **J. KAREIS** — Siegfried Marcus's Petroleum Engine. **Dr. E. HOPPE**—Historical Note on Volta's Law of Potential Difference. *Anon.* — Siemens & Halske's Electric Winding Engine for Mines. **Dr. PIRANI**—Westinghouse Transformer System. **Dr. F. VOGEL** — Calculation of Lightning Rods. *Anon.*—Siemens & Halske's Safety Appliances for Steam Engines in Factories. **Dr. WIETLISBACH**—Theory of Telephonic Conductors. **A. ALTHELLER** —New Conclusion Signal for Telephone Exchanges. **Dr. PIRANI**— Submarine Telephony. *Anon.*—Behaviour of Powders in relation to Microphones. *Anon.*—Phonograph and Gramophone. **T. SCHWARTZE** —E.M.F. of Magnetisation. **T. SCHWARTZE**—Transverse Magnetisation. *Anon.*—An Apparent Exception in Electro-magnetic Induction. *Anon.*—Rung's Pneumatic Speed Indicator. **Professor K. FUCHS**— Method of Releasing a Pendulum.

CURVES REFERRED TO BY MR. W. B. ESSON IN HIS REMARKS ON
MR. KAPP'S PAPER ON "ALTERNATE CURRENT TRANSFORMERS."

See Journal, Part 71, p. 206.

(Owing to a misunderstanding, this Diagram was omitted in Part 71.)
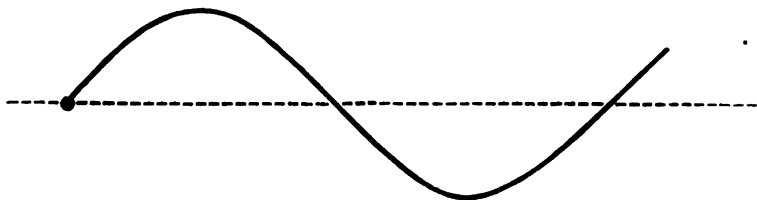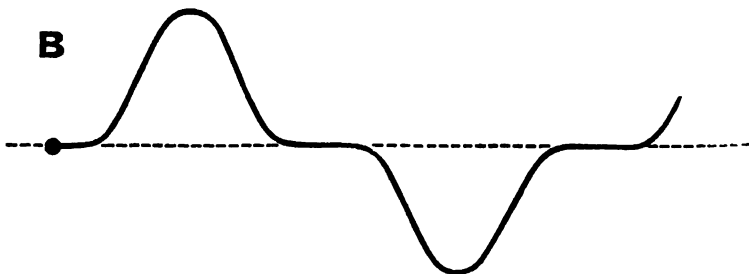
The One Hundred and Seventy-fifth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, March 8th, 1888—Mr. Edward Graves, President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council, viz. :—

From the class of Associates to that of Members—

Frederick Wyles.

The President then called on Mr. R. von Fischer Treuenfeld to read his paper.

Mr. R. von Fischer Treuenfeld : With trembling voice and deeply moved heart I pray you all to allow me to make a few remarks, before beginning my paper, with reference to the news which has just reached us of the death of my beloved Emperor. I am quite sure that you all will feel with me the great loss which the whole world has sustained by losing a monarch who

not only was the incorporation of all virtues, but whose existence was devoted for the benefit of humanity, the welfare of nations, and the peace of the whole world.   I hope you will feel with me and mourn for his memory.*

THE PRESENT STATE OF FIRE-TELEGRAPHY.

By R. Von Fischer Treuenfeld, F.R.G.S., Member.

It is now eleven years since I had the pleasure of bringing for the first time before this Society of Telegraph-Engineers, and before the British Public in general, the subject of " Fire-Tele- " graphs," and of proving by statistics the considerable reduction in the percentage of " serious fires " which can be attained by the adoption of a system of fire-telegraphs, especially such as comprise a large number of street call-point apparatus.

Such a system of electric fire-alarms, with all its details of construction and working, was proposed, as early as the 17th October, 1861, by the firm of Siemens Bros. & Co., to the town of Manchester.   That proposal was even then based upon an experience of more than twelve years, gained in many towns on the Continent, the first fire-telegraph being erected in Berlin in the year 1849, where it has given continuous proofs of its value, practicability, and efficiency.

Great objection, unfortunately, was taken by the Manchester Town Council to the " plan of having a pane of glass to be broken " for every alarm, as mischievous persons would have great " facilities for giving false alarms."   This imaginary difficulty not only postponed the introduction of fire-telegraphs into Manchester, but was also the principal cause of fire-telegraph call-points not being introduced into London until the year 1880, when the Metropolitan Fire Brigade erected the first 40 call-points, twenty-seven years after their introduction in Berlin and other Continental and American towns.

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* The announcement of the death of the German Emperor had just been published in the evening newspapers, although, as it subsequently transpired, the statement anticipated the actual event by twelve hours.—Editor.

Fire-telegraphs and call-points having proved to be reliable and valuable appliances, and seeing that nothing of the kind had been adopted in British towns up to 1877, I made it the object of my first paper on fire-telegraphs to break through those superstitious obstructive arguments by laying before you statistical figures obtained from the chief officers of a large number of fire brigades, which all proved the practicability and utility of such fire-telegraph systems. The contents of that paper may be summarised into the now generally recognised law, that: "in towns with perfect fire-telegraph systems the "serious fires are reduced to an average percentage of 4; "with less perfect systems the serious fires are more frequent, "reaching an average percentage of 17; while the percentage "of serious fires in towns possessing no fire-telegraphs reaches "as high as 29."

In venturing again to bring the subject of "Fire-Telegraphs" before this Society and before the public, I can scarcely hope to produce any new arguments in favour of fire-telegraphs, nor to advance anything which could modify the doctrine previously arrived at. The object of this paper consists rather in repeating and confirming what has been already established in the former one, in the hope that such repetition, based upon results of later date, will contribute to a more speedy adoption of these means of communication, which have long ago proved to be a great protection against the ravages of fires, and which yet only find their way into this country with an inexplicable and deplorable tardiness.

It will be convenient, in the first place, to examine the state of the Metropolitan Fire Brigade's telegraph system during the decade from 1876 to 1886, and to draw from it such conclusions as have reference to the protection of life and property. We will then review the fire-telegraphs of some of the principal provincial towns, and endeavour to show how unwarrantably backward they still remain, in spite of the achievements in Continental and American cities, and in spite of the good example recently set by the Metropolitan Fire Brigade. Finally, we shall compare the results obtained from British fire-telegraphs with similar ones

from Continental and American towns, and thus endeavour to show how much more could, and ought to be done in perfecting and extending the telegraph systems of British fire brigades.

Table No. 1 shows the number of fires in London from 1866 till 1886. The figures give the actual numbers and the percentages of serious and slight fires. False alarms and chimney alarms are not included in any of the tables, nor do the figures include trifling damages by fires which were not sufficiently important to require the attendance of firemen.

Table I.

LONDON FIRES.

YEAR.	NUMBER OF FIRES.			PERCENTAGES.		
	Serious.	Slight.	Total.	Serious.	Slight.	Total.
1866	326	1,012	1,338	25	75	100
1867	245	1,152	1,397	18	82	100
1868	235	1,433	1,668	14	86	100
1869	199	1,373	1,572	13	87	100
1870	276	1,670	1,946	14	86	100
1871	207	1,635	1,842	11	89	100
1872	120	1,374	1,494	8	92	100
1873	166	1,382	1,548	11	89	100
1874	154	1,419	1,573	10	90	100
1875	163	1,366	1,529	11	89	100
1876	166	1,466	1,632	11	89	100
1877	159	1,374	1,533	10	90	100
1878	170	1,489	1,659	10	90	100
1879	159	1,559	1,718	9	91	100
1880	162	1,709	1,871	9	91	100
1881	167	1,824	1,991	8	92	100
1882	164	1,762	1,926	9	91	100
1883	184	1,960	2,144	9	91	100
1884	194	2,095	2,289	9	91	100
1885	160	2,110	2,270	7	93	100
1886	151	1,998	2,149	7	93	100

Table No. 2 shows how considerably the telegraphic circuits of the Metropolitan Fire Brigade telegraph system have given

Table II.
LONDON FIRE-TELEGRAPH SYSTEM.

Year	Telegraphs between Fire Stations	Points at which Fire Alarms can be raised.												Total.	Population of London.	Proportion between Alarm-Points and Inhabitants.
		Telephones between Fire Stations.	Alarm Circuits round Fire Stations.	Telegraphs to Police Stations.	Telephones to Police Stations.	Telegraphs to Public or other Buildings.	Telephones to Public or other Buildings.	Direct Fire-Alarms.	Land Fire-Engine Stations.	Movable Land Stations.	Fire-Escape Stations.	Floating Stations.	Street Call-Points in Alarm Circuits.			
1876	56 (101 miles)	...	...	...	...	...	...	...	49	...	107	4	...	160	2,805,000	1 to 17,531
1877	57 (104 ,,)	...	...	...	...	...	...	...	50	...	108	4	...	162	2,939,400	1 ,, 18,144
1878	57 (104 ,,)	...	...	...	...	...	...	...	50	...	109	4	...	163	3,073,800	1 ,, 18,857
1879	57 (106 ,,)	...	...	...	...	...	...	...	52	1	113	4	...	170	3,208,200	1 ,, 18,871
1880	58 (107 ,,)	...	6	...	...	...	...	...	52	5	117	4	40	218	3,342,600	1 ,, 15,333
1881	53 ...	7	7	...	...	...	...	...	58	11	121	4	44	233	3,477,000	1 ,, 14,923
1882	49 ...	17	11	...	...	...	...	...	54	12	124	4	77	271	3,611,400	1 ,, 13,322
1883	45 ...	18	28	4	18	16	13	12	55	12	127	4	151	407	3,745,800	1 ,, 9,203
1884	40 ...	24	34	4	16	15	16	18	55	...	127	4	220	470	3,880,200	1 ,, 8,256
1885	28 ...	38	42	3	18	13	20	18	55	...	127	4	263	510	4,014,600	1 ,, 7,871
1886	28 ...	39	54	8	18	14	20	15	55	...	127	4	347	603	4,149,000	1 ,, 6,880

way, since 1881, to telephonic and alarm circuits, and how the points at which fire alarms can be raised have thus been augmented. This, again, is equivalent to the establishment of a more favourable proportion between the number of alarm-points and of inhabitants; which proportion, as shown later on in Table No. 4, keeps step with a diminution of "serious fires," and consequently with a saving of property.

It was in 1880—three years after the utility of fire-telegraphs, and the beneficial influence which such must have on the reduction of the number of "serious fires," were brought to your notice—that the London Fire Brigade began to introduce the fire-annunciators or call-points. Captain Shaw, C.B., the gallant Chief Officer of the Metropolitan Fire Brigade, had in that year, in his twentieth report, the satisfaction of calling attention in his annual statistics to some specially interesting points, which became the beginning of a new era in the development of British fire-telegraphs. We can do no better than repeat verbatim that officer's report of 1880, in which he says:—

" A very important improvement has been effected this year
" by the establishment of 6 circuits of fire-alarms with an
" aggregate of 40 call-points, which very considerably reduce the
" distance to be run by persons giving alarms of fire, and conse-
" quently the time of our getting information. Although this
" improved mode of communication has not been established more
" than a few months, we have already received by means of it no
" less than 44 good calls. Unfortunately, however, we have also
" received 33 false alarms, many of which were without doubt
" wilfully raised. Thus it will be seen that the great advantage
" of the system has not been obtained without harassing the men
" and casting a doubt on the value of all messages received by
" these instruments. This has been the experience of all places
" which have adopted the system of street fire-alarms; but I had
" great hope that in such a city as London we should have been
" free from an annoyance which, if persevered in, must eventually
" force us to discontinue what ought to be a very great improve-
" ment. I am still most unwilling to abandon this hope; and I
" think I may say, though not with any great confidence, that I

" begin to observe symptoms of a better state of things with
" regard to these useful machines for the future. . . . The future
" of these street fire-alarms must eventually depend to a very
" great extent on the inhabitants of London, who can render
" invaluable assistance in the way of protecting the brigade from
" being imposed upon by the tricks of foolish and evil-disposed
" persons, who have not enough intelligence to appreciate the
" gravity of the offence, with its possible effect of necessitating
" the abolition of a scheme, as yet only imperfectly applied,
" which promises to secure a more efficient and more extended
" means of protecting the metropolis from fire."

This report of Captain Shaw must have been exceedingly
welcome to all who were firm believers in what was then for the
first time officially recognised as " a scheme which promises to
" secure a more efficient and more extended means of protecting
" the metropolis from fire."

In that great hope the gallant Chief Officer has not been dis-
appointed.   Tables Nos. 1 and 2 show how, since the intro-
duction in 1880 of the first 40 call-points, and during their
gradual increase to 347, the percentage of " serious fires " has
been reduced from 9 to 7—a result undoubtedly most beneficial to
the protection of property, and which stands in close connection
with, if it is not entirely caused by, the considerable reduction of
the distance to be run by persons giving alarms of fire.

At the time of writing the first paper on " Fire-Telegraphs,"
the proportion of alarm-points to inhabitants in London was at
an alarmingly low figure, and Captain Shaw's official report gives
the percentage of " serious fires " for the year 1876 as 11; whereas
with the gradual increase of alarm-points, which in the year 1886
shows the more favourable figure of 1 : 6,880 inhabitants, the
serious fires have already been reduced from 11 to 7 per cent.
This is exactly what was predicted; and I do not hesitate for a
moment to repeat what was said eleven years ago—that a perfect
fire-telegraph system, in which the proportion of alarm-points to
inhabitants will become as high as about 1 : 2,000, instead of
1 : 6,880, as it was still in London at the end of 1886, would
reduce the average percentage of serious fires to 4, which figure

may be considered as an average minimum, but which a town like London, or any other British town of importance, ought to, and we have no doubt will, attain.

The serious complaint contained in the 1880 report of the Metropolitan Fire Brigade — viz., that 33 false alarms were received during the first year of the installation of the street call-points, and that these were without doubt wilfully caused by " evil-disposed persons "—is certainly much to be regretted. But in this respect the hope which the Chief Officer expressed at the time—that London would be free from such annoyance—has also been realised. Indeed, judging from similar, but previous, experiences on record in the history of a vast number of Continental and American fire-telegraphs, such maliciously raised alarms had been experienced at all places during the first few years after the erection of the fire-alarm apparatus; but owing to the vigilance and activity of the police, and the public in general, they soon became everywhere reduced to a minimum. Thus we find that in only one more report of the Metropolitan Fire Brigade—that for the year 1881, the second after the installation of the fire-annunciators—is attention drawn to the " occasional " harassing false alarms caused by the fire-alarm circuits." This second report not only acknowledges the great value of the new means of electrical communication, but it foreshadows already the speedy disappearance of wilful tampering with the fire-annunciator. It says:—

" The fire-alarm circuits have been of great service during " the past year, and have undoubtedly enabled us in some " instances to save both lives and property; but we still continue " to be harassed by occasional false alarms through them. The " magistrates seem determined to punish most severely anyone " detected in doing damage to them, and the vigilance of the " police is beyond all praise; but it is not on either the magis- " trates or the police that we must depend ultimately for the " protection of these posts. Nothing but the assistance of the " public can make them really safe, and I am bound to say that " in this particular there is a very marked improvement during " the past year. Indeed, I am aware that anyone discovered by

" a passing crowd in tampering with an alarm-post would be
" in danger of a summary punishment far more severe than
" any which a magistrate would inflict. On the whole, we
" have reason to be satisfied with what the authorities and the
" public have done in this matter, and I have every hope that
" each succeeding year will be characterised by even a greater
" advance in the value of these most useful aids to our efficiency."

So far the report of 1881, the second after the installation
of the fire-annunciators in London.   The later reports, up to
1886, no longer contain any complaint as to wilful tampering
with the alarm circuits ; and we trust that long since the
men of the Metropolitan Fire Brigade, so unusually overtaxed
with labour, have had no serious complaint to make of being
needlessly harassed by maliciously raised alarms.   These reports
only mention, in reference to the fire-telegraph system, that in
1885 "the abolition of payments for calls has been attended
" with very satisfactory results in reducing the number of
" unnecessary alarms to an appreciable extent."   The report
for 1886 further contains the statement that "the substitution
" of telephones for telegraphs, which was commenced some years
" ago, still continues, and, though for financial and other reasons
" carried out only gradually, has proved a great advantage.   We
" are now proceeding more rapidly, and I anticipate that before
" the end of 1887 the change will be completed throughout the
" whole of our system."

The next point to be considered in this paper is, whether the
increase of fire-engine stations, escape stations, of telegraph,
telephone, and alarm circuits, and the introduction of call-points,
have produced an apparent decrease in the loss of life.   The
figures contained in Table No. 3 bear upon this question, and are
all extracted from the report of the Chief Officer of the Metro-
politan Fire Brigade.   This table shows the number of persons
seriously endangered by fires in London during the eleven years
from 1876 till 1886, viz., 1,844 persons endangered, of whom
1,437 were saved and 407 lost their lives.   Of these 407 persons
lost, 202 were taken out alive, but died afterwards in hospitals
or elsewhere, and 205 were suffocated or burned to death.

Table III.
Lives Endangered by London Fires.

Year	Lives Saved			Lives Lost							Total Endangered			Proportion of Lives Lost	
				Taken out Alive.			Not taken out Alive.							Total Number of Fires.	Lives Lost per 100 Fires.
	Male.	Female.	Total.	Male.	Female.	Total.	Male.	Female.	Total.	Total.	Male.	Female.	Total.		
1876	50	38	88	6	17	23	1	11	12	35	57	66	123	1,682	2·14
1877	90	46	136	4	10	14	5	10	15	29	99	66	165	1,588	1·89
1878	63	63	126	6	6	12	5	8	13	25	74	77	151	1,659	1·51
1879	75	57	132	8	12	15	12	5	17	32	90	74	164	1,718	1·86
1880	68	59	127	4	10	14	9	10	19	38	81	79	160	1,871	1·76
1881	70	44	114	6	10	16	9	15	24	40	85	69	154	1,991	2·01
1882	76	63	139	11	11	22	10	4	14	36	97	78	175	1,926	1·87
1883	88	49	137	5	8	13	13	13	26	39	106	70	176	2,144	1·82
1884	96	62	158	8	12	20	9	13	22	42	113	87	200	2,289	1·83
1885	96	58	154	12	12	24	9	14	23	47	117	84	201	2,270	2·07
1886	71	55	126	18	16	29	11	9	20	49	95	80	175	2,149	2·28
	843	594	1,437	78	124	202	93	112	205	407	1,014	830	1,844	21,182	1·91

The last column of the table shows the percentage of lives lost during the eleven years, which very nearly amounts to 2 lives lost at every 100 fires; and it also demonstrates that this percentage has not been diminished by the introduction of fire-telegraphs, at least not in London, it remaining practically the same before and after the introduction of the fire-alarm circuits. This fact negatives the general belief that the fire-telegraph offers a greater protection to life, as in reality only a protection to property is apparent; but in this latter respect it

Table IV.

LONDON FIRE STATISTICS.

YEAR.	Number of Points at which Fire Alarms can be raised.	Proportion of Alarm-Points to Inhabitants.	Percentage of Serious Fires.	Lives Lost per 100 Fires.
1876...	160	1 to 17,581	11	2·14
1877...	162	1 ,, 18,144	10	1·89
1878...	163	1 ,, 18,857	10	1·51
1879...	170	1 ,, 18,871	9	1·86
1880...	218	1 ,, 15,333	9	1·76
1881...	233	1 ,, 14,923	8	2·01
1882...	271	1 ,, 13,322	9	1·87
1883...	407	1 ,, 9,203	9	1·82
1884...	470	1 ,, 8,256	9	1·83
1885...	510	1 ,, 7,871	7	2·07
1886	603	1 ,, 6,880	7	2·28
				Average, 1·91

is of an unlimited value, and cannot be too highly appreciated. With regard to the saving of lives, the official figures of the " Reports of the Chief Officer of the Metropolitan Fire Brigade " indicate sufficiently that the death-rate is influenced chiefly by circumstances intimately connected with the very outbreak of fires. Over these even the speedy call of the brigade seems to have but little control, and the rate is principally kept at its low figure by the renowned bravery and deeply rooted sense of duty of the corps of the London Fire Brigade.

We are here confronted with the rather unexpected, but

nevertheless correct result that, although the increase of the fire brigade and the further development of the fire-telegraph had a decided influence upon the decrease of "serious fires," and consequently upon a greater protection of property, those increased advantages had nevertheless no apparent effect in increasing the protection of life. It appears, therefore, that the Metropolitan Fire Brigade has long since arrived at the utmost limit which human efforts could attain for the protection of life in cases of fires, viz., at the low average of about 2 lives lost per 100 fires—a minimum which even the introduction of increased means of rapid communication could not diminish in London. Based upon these unquestionable official figures, it would be interesting to know the opinion of so high an authority as Captain Shaw, who in his report of 1881 stated that "the fire-alarm circuits "have enabled the brigade in some instances to save both lives "and property." Also, Mr. E. B. Bright, in his paper on "A System of Electric Fire-Alarm," read before the Society of Telegraph-Engineers and Electricians in January, 1884, repeatedly draws our attention to the supposed "protection of lives "caused through the application of electric fire-alarms."

With regard to this question it must, however, be kept in mind that London is not yet sufficiently provided with electrical call-points for giving fire alarms, having only one alarm-point for about every 6,880 inhabitants; whereas other towns better provided with fire-annunciators, such as Cologne, Nuremberg, Munich, Amsterdam, Stockholm, Chicago, New York, &c., &c., possess one alarm-point to about every 2,000 or less inhabitants. It is thus not impossible that a further decrease in the percentage of lives lost during London fires might result from a considerable increase of fire-annunciators.

We shall, in the course of this paper, have occasion to return to this question, when the results obtained at other towns will be compared with those of the metropolis.

After this review of the London fire-telegraphs, let us now see how the principal towns of the British Empire are provided with facilities of communication for rapidly calling aid for the extinction of fires.

Table V.

									Remarks
...Salford... "Certain Fires." "Proportion of Population" and of (At least Loss) by Private Things									No street call-points. Stations are connected by telephone. "Slight fires" are here such as result under £2,000 damage. This explains the comparatively small number of serious fires.
alford ... ... { 1876–81	10·4	6·0	2·6		1 : 24,9821	: 24,562	7·70	1·05	0·00
{ 1881–86	6·7	5·0	2·7				0·00	0·00	0·00
1876–91	59·5								

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As the space of this paper does not allow of such information being given in detail, it is necessary to condense the reports obtained from twenty of the principal towns into one single table (No. 5); the order of classification being in accordance with the more or less favourable proportion of inhabitants to alarm-points. By " alarm-points " are here understood all fire stations with permanent equipments of men, besides all police stations in telegraphic or telephonic communication with the fire brigade, and also all electrical street call-points. But connections with telephone exchanges, with private houses, stores, &c., and with automatic fire-annunciators based upon electrical contact-making by expansion through heat, are not considered in this present account as alarm-points accessible to the public in general, and therefore do not count as such in these tabulated statistics.

The results shown in Table No. 5 are mostly derived from data extending over the last eleven years, and they are split into two periods, representing either the results obtained during the time before and after the adoption of street call-points, or, where street call-points have not yet been adopted, the results corresponding to two equal periods or numbers of years. By adopting this plan of splitting each report into two periods, it can be seen at a glance what progress a town has made in reducing the proportion of serious fires and of lives lost; and this is done quite independently of the various standards which different towns have adopted for the classification of their serious fires.

This table thus represents a true picture of the present fires and fire-telegraphs of the principal British towns, showing in its foreground the encouraging result that a great general progress has taken place during the last ten years in the reduction of serious fires, and thus in protecting property more efficiently. With the exception of Cork, Nottingham, and Manchester, the percentage of serious fires has everywhere been reduced during the second of the two periods into which the observations have been divided.   And again, with the exception of Sunderland, Brighton, Cork, Oldham, Blackburn, and Bradford, the proportion between public alarm-points and inhabitants has likewise been everywhere considerably increased; so that a general increase of

alarm-points and a general decrease of serious fires has taken place in provincial towns as well as in the metropolis.

But these results, however gratifying in themselves, are nevertheless not yet quite satisfactory. That table shows only too clearly that fire-telegraphs have not yet attained that popularity and usefulness in British towns which they have enjoyed for a long time in Holland, Germany, America, and elsewhere, and which they really merit. The proportion of inhabitants to alarm-points in the former towns ranges, as seen from Table No. 6, as high as 20,000 to 30,000, and even—as is the case in Cork, Preston, and Blackburn—to 80,000, 100,000, and 115,000 inhabitants respectively to one single alarm-point; whereas most of the Dutch, German, and American towns possess an alarm-point to every 2,000 or 3,000 inhabitants, as will be seen from Table No. 10.

Table VI.

BRITISH TOWNS HAVING NO PUBLIC STREET CALL-POINTS UP TO
THE END OF 1886.

Town.	Inhabitants.	Number of Alarm-Points.	Proportion between Alarm-Points and Inhabitants.
Brighton	130,000	8	1 : 16,250
Cardiff	120,000	6	1 : 20,000
Manchester	373,583	18	1 : 20,754
Liverpool	586,990	23	1 : 21,173
Edinburgh	258,629	12	1 : 21,552
Salford	196,894	8	1 : 24,562
Bradford	219,411	8	1 : 27,427
Bristol	220,915	8	1 : 27,614
Sunderland	125,000	4	1 : 31,250
Oldham...	130,216	4	1 : 32,554
Hull	200,000	6	1 : 33,300
Birmingham ...	450,000	13	1 : 34,616
Sheffield	316,000	9	1 : 35,111
Cork	80,000	1	1 : 80,000
Preston...	100,406	1	1 : 100,406
Blackburn	115,000	1	1 : 115,000
TOTAL... ...	3,628,044	130	Average, 1 : 27,869

The consequences of such vast differences in public facilities for raising fire alarms naturally show themselves in a considerably higher percentage of serious fires in British towns, of which it is only necessary to mention Liverpool with 19 per cent., Bradford with 28 per cent., Sunderland with 41 per cent., Cork with 50 per cent., and Hull with 64 per cent. of serious fires.

In this unsatisfactory state of affairs lies the motive which induced me to collect and tabulate the statistics now laid before you. As similar tables which I brought forward ten years ago were precursory to the introduction of street call-points into this country, I trust now that the present discussion will contribute to pave the way to a greater popularity of fire-telegraphs amongst those who have thus far neglected this auxiliary, so important to every fire brigade.

The towns which have already adopted electrical street call-points are still very few in this country, and we can only mention here London, Leeds, Nottingham, Glasgow, Newcastle, Bolton, and Dublin.

Table No. 7 shows the number of street call-points adopted in those towns up to 1886.

Table VII.

BRITISH TOWNS HAVING ADOPTED STREET CALL-POINTS.

Towns.	Inhabitants.	Number of		Proportion between Alarm-Points and Inhabitants.
		Alarm-Points.	Street Call-Points.	
Glasgow	538,915	96	82	1 : 5,614
London	4,149,000	603	347	1 : 6,880
Leeds	339,057	36	12	1 : 9,418
Newcastle	145,359	13	6	1 : 11,181
Bolton...	108,000	8	7	1 : 13,500
Nottingham ...	234,000	15	9	1 : 15,600
Dublin	353,082	21	12	1 : 16,813
TOTAL	5,867,413	792	475	Average, 1 : 7,408

After this brief review of the fire-telegraphs in some of the British towns, let us now turn our attention to fire-telegraphy in

foreign towns, and see what has been the influence of increased means of rapid communication on the protection of life and property.   For this purpose I have chosen quite at random a few of the larger European and American towns; but there can be no doubt that similar average results would only repeat themselves should other towns be selected instead.

FIRE AND POLICE TELEGRAPH AT AMSTERDAM.

Table No. 8 shows the percentage of serious fires and that of lives lost from fires during the years 1875 till 1886, and Table No. 9 shows the extent of the fire-telegraph and the proportion of inhabitants to alarm-points.

The first Amsterdam street call-points were erected at the end of 1874, and the average percentage of serious fires before that time was as high as 10, whereas there is now an average of only 3·5 per cent. of serious fires.

Table VIII.

FIRE AND POLICE TELEGRAPHS IN AMSTERDAM.

YEAR.	NUMBER OF FIRES.			PERCENTAGE.			LIVES LOST.		
	Serious.	Slight.	Total.	Serious.	Slight.	Total.	Total.	Per 100 Fires.	Percentage during 6 Years.
1875	12	207	219	5·5	94·5	100	2	0·91	⎫
1876	11	311	322	3·4	96·6	100	...	...	⎪
1877	12	304	316	3·8	96·2	100	1	0·31	⎪
1878	15	391	406	3·7	96·3	100	1	0·25	⎬ 0·40
1879	12	470	482	2·5	97·5	100	1	0·21	⎪
1880	16	522	538	3·0	97·0	100	4	0·74	⎭
1881	16	538	554	2·9	97·1	100	2	0·36	⎫
1882	13	533	546	2·4	97·6	100	...	...	⎪
1883	18	625	643	2·8	97·2	100	...	...	⎪
1884	23	600	623	3·7	96·3	100	2	0·32	⎬ 0·42
1885	35	688	723	4·8	95·2	100	11	1·52	⎪
1886	33	857	890	3·7	96·3	100	3	0·34	⎭
TOTAL	216	6,046	6,262	Average. 3·5	Average. 96·5	100	27	0·43	

Table IX.

FIRE AND POLICE TELEGRAPHS IN AMSTERDAM.

Year	Length of Telegraph Lines						Alarm-Points				Number of Inhabitants.	Proportion between Alarm-Points and Inhabitants.
	Underground Cable.		Overhead Lines.		Total.		Telegraph Stations with Morse Apparatus.		Street Call-Points.	Total of Alarm-Points.		
	Connecting Telegraph Stations.	Connecting Police Stations.	Connecting Telegraph Stations.	Connecting Police Stations.	Kilo-mètres.	English Miles.	Fire Telegraph.	Police Telegraph.				
	Mètres.	Mètres.	Mètres.	Mètres.								
1875	64,770	8,270	17,160	...	90·2	56·0	23	7	131	161	282,000	1 to 1,751
1876	67,610	8,270	19,260	1,800	96·4	59·8	27	9	136	172	287,000	1 ,, 1,668
1877	68,340	8,270	22,140	2,900	101·6	63·2	29	11	138	178	290,000	1 ,, 1,629
1878	70,110	8,570	22,840	2,900	104·4	64·9	29	12	138	179	296,000	1 ,, 1,653
1879	70,680	8,570	27,660	2,900	109·8	68·2	30	13	141	184	302,000	1 ,, 1,641
1880	71,755	8,570	30,835	2,900	115·0	71·5	38	18	142	198	309,000	1 ,, 1,561
1881	73,800	9,530	37,335	7,900	130·6	81·1	43	22	146	211	316,000	1 ,, 1,498
1882	77,000	11,530	37,000	7,900	133·6	83·0	43	22	155	220	326,200	1 ,, 1,483
1883	79,000	11,700	35,000	8,000	134·2	83·4	43	23	159	225	328,000	1 ,, 1,458
1884	80,000	12,160	36,000	8,500	136·7	84·9	45	24	166	235	350,200	1 ,, 1,490
1885	81,800	12,300	41,000	10,500	145·6	90·5	46	25	178	249	361,300	1 ,, 1,491
1886	84,200	12,600	43,800	11,450	152·0	94·5	46	25	185	256	366,700	1 ,, 1,432

These tables, as well as all the others, are compiled on the same plan as those of the London fires, omitting entirely all alarms of chimney and other insignificant fires; the figures being taken from the official reports of the chief officer of the brigade, Mr. Van Moock. There were also some fires on board vessels in the port of Amsterdam, and others outside the town limits, which the brigade had to attend, but which are not included in these tables.

The number of lives lost at Amsterdam shows in a certain respect a remarkable coincidence with those lost at London fires, and, as we shall see later on, at many other towns—viz., the percentage of lives lost from fires does not seem to be visibly influenced by an increase of alarm-points. On the contrary, there is sufficient evidence to show that, in spite of a general increase of fire stations, of rapid alarm signalling, and of extended telegraphic communication between fire stations, there is a decided tendency in modern fires to become more and more fatal. The causes of this destructive tendency, which even a combined and increased efficiency of the fire brigade and the fire-telegraph has not been able to conquer to any visible degree, seem to be the same in this as in other countries.

It is evident that the Amsterdam fire and police telegraph is making constant progress, yearly augmenting the length of its lines and the number of stations and street call-points. At the end of 1886 the proportion between alarm-points and inhabitants had already been increased to the high figure of 1 : 1,432, and there can be no doubt that the low average percentage of serious fires—viz., 3·5—is greatly due to this proportionally great number of alarm-points. The street call-points are of Siemens system, and the station apparatus are principally self-starting Morse inkers, which, on account of their leaving a written record, are preferred to other systems of telegraphic apparatus. But since 1881 the chief office of the brigade has also been connected with the Telephone Exchange of the town, so that telephone subscribers can communicate with the central station of the fire brigade.

It would have led us too far to produce more detailed descrip-

City	Period				Ratio				Remarks
Breslau	1876-81	7.7	5.5	2.7	1 : 4,354	1.92	0.32	0.00	Street call-points were introduced in 1863; they are of Gurlt's system. Besides 55 street call-points, there are 85 similar apparatus in private establishments. The proportion between alarm-points and inhabitants has lately been lowered in consequence of an increased military garrison population.
	1881-86	5.4	4.1	2.1	1 : 4,975	3.60	0.64	0.00	
	1881-86	10.0	4.4	0.0	4.4	0.00	0.00	0.00	Webb's system.
Lisbon	1876-81	7.7	5.8	3.6	1 : 7,148	2.05	1.08	0.46	No street call-points, but the fire-telegraph, besides having 48 alarm-points at fire stations, is also connected with the police stations; these latter, however, are not included in the list. The fire-telegraph was established according to Hermann's system in 1878.
	1881-86	8.1	6.2	3.0	1 : 5,417	2.40	0.91	0.00	
Copenhagen ...	1876-81	15.7	11.5	5.4	1 : 17,750	1.18	0.14	0.00	Besides the street call-points, which were introduced in 1874, there are also 28 such call-points in private houses, not accessible to the public, and these are therefore not taken into account in this list.
	1881-86	14.8	8.6	3.7	1 : 8,788	1.74	0.38	0.00	
Paris	1876-83	30.9	27.4	23.6	1 : 21,157	1.93	0.68	0.11	Street call-points were introduced in 1884. Besides 103 such call-points in 1887, there are 82 theatres and 87 buildings telegraphically connected with the fire-telegraph system.
	1884-86	27.5	26.9	26.5	1 : 11,329	0.63	0.41	0.28	
Antwerp	1876-81	28.6	17.3	11.8	1 : 16,480	0.00	0.00	0.00	No street call-points. All fire stations are in telephonic communication.
	1881-86	19.5	15.0	12.1	1 : 11,579	0.00	0.00	0.00	
Brussels	1876-81	13.5	10.1	7.9	1 : 17,000	6.76	3.05	0.00	No street call-points; but there are 36 call-apparatus at theatres and Government buildings, established since 1882, according to Wiesenthal's system.
	1881-86	23.4	8.6	5.4	1 : 14,167	5.26	1.90	0.00	

tions of the development of fire-telegraphs in other foreign towns, and it therefore became necessary to condense the various reports obtained into one single table (No. 10). The towns are here divided into three groups, viz., German, American, and various; the classification is otherwise, in accordance with the rule adopted in forming Table No. 5.

The same satisfactory development as observed with English fire-telegraphs can clearly be traced among the results obtained by these three groups of towns. With the exception of Magdeburg, the serious fires in all other German towns have decreased; and the proportion between public alarm-points and inhabitants, being already at the high ratio of 1,000 to 7,000 inhabitants to each alarm-point, has mostly been further increased, and only in a few instances has it slightly altered for the worse.

The same favourable observations hold good for American towns, with this important difference, however—that fire-telegraphs in America are far more popular than in Germany and England, showing a proportion between public alarm-points and inhabitants of from 500 to 2,000 inhabitants to each alarm-point.

Then, again, the third group in Table No. 10, embracing Austrian, Hungarian, French, Russian, Danish, Swedish, Portuguese, Dutch, and Belgian cities, shows only two towns—viz., Prague and Lisbon—where a slight increase of serious fires has taken place; whereas the proportion between alarm-points and inhabitants has everywhere been favourably increased.

After having thus reviewed the fire-telegraphs of a number of towns, British and foreign, the conclusion to be drawn is, that the primary object of all fire-telegraphs ought to be the reduction to a minimum of the time which elapses between the discovery of a fire and the appearance of the brigade on the spot, so as to prevent as much as possible the chances of a fire becoming " serious."

The results before us prove that this cannot be attained by a fire brigade, however well equipped and organised, unless assisted by a telegraph system which possesses the means of reducing delay to a minimum.

These means principally consist in the adoption of a large number of electrical street call-points.

How much the time elapsing between the raising of the alarm and the appearance of the brigade can practically be reduced by the aid of such a fire-telegraph system, could easily be shown by extracts from numerous official reports, of which I will only draw attention to the following :—

Table No. 11 shows the respective lapse of time between the raising of the alarm and the appearance of the first section of the brigade at the place of conflagration, as taken from the official report of the fire brigade at Aix-la-Chapelle. There were only 89 calls to fires during 1886, 5 of which came from places so far out of the town that it necessarily took more than 11 minutes for the brigade to reach them. Deducting these 5 cases, the fire brigade only required, on the average, 4 minutes to reach the respective places of conflagration.

Table XI.

AIX-LA-CHAPELLE FIRE-TELEGRAPH.

Lapse of Time between Receipt of Fire Alarms and the Appearance of the Brigade at the Spot of Conflagration.

Lapse of Time.	FIRES DURING THE YEAR 1886.					Alarms not requiring the Aid of the Brigade.	TOTAL.
	Large.	Medium.	Small.	Chimney.	Forest.		
2 minutes	...	1	5	4	...	3	13
3 ,,	...	1	8	4	...	2	15
4 ,,	1	2	10	5	...	5	23
5 ,,	...	...	16	1	...	...	17
6 ,,	...	1	5	2	...	2	10
7 ,,	...	...	1	...	...	1	2
8 ,,	...	...	1	...	...	1	2
9 ,,	...	...	...	...	...	...	...
10 ,,	...	...	1	...	...	...	1
11 ,,	...	...	...	...	...	1	1
More than 11 ,,	...	2	...	...	3	...	5
TOTAL ...	1	7	47	16	3	15	89

The chief officer of the Edinburgh fire-engine establishment, Mr. Samuel Wilkins—who has good reason to be proud of his low rate of only 0·5 per cent. of " serious fires," and 0·35 of lives lost per each 100 fires—fully acknowledges in his last annual report the advantages derived from rapid communication, by saying:— " Extinguishing fires depends wholly upon the rapidity with " which the requisite appliances are brought to bear upon them; " it follows, then, that as the number of places where notice of " fires can be given, and from which appliances can be brought " for the purpose of extinguishing them, are increased, the saving " of life and property is increased in the same proportionate " degree.   The promptness in turning out the brigade, and its " speedy arrival at the scene of action, during the last few years, " have reduced cases of serious conflagration within the city of " Edinburgh almost to a minimum."

One of the most valuable opinions has been communicated to me by the chief officer of the Berlin Fire Department, representing the experience of a brigade which was not only the first to adopt fire-telegraphs, but the results of which rank with the very best in existence.   That officer says :—" We have likewise arrived at " the conclusion that, supposing structural and industrial cir- " cumstances to remain the same, and the general precautions of " fire-efficiency to be also equally observed, a decrease of serious " fires undoubtedly results from an increase of fire alarm-points."

It must, however, be pointed out here, as was already done in my former paper, that we must not conclude that security against fires depends *only* upon the amount of facilities for raising an alarm.   The structure of buildings, the density of population, the industrial character of the towns, the water supply, the efficiency of the brigade and their equipment—all these are factors of equally high importance.   In order, then, to obtain the highest degree of efficiency and of security, the entire system of fire extinction must be equally well perfected.   Where this has been neglected, or has been impossible, an irregularity in the ratio of serious fires naturally results; and this considera- tion alone explains many of the deviations from strict regularity in the tables laid before you.

Another opinion in favour of electrical fire-alarms is given by the Superintendent of the Bolton Fire Brigade. After having in 1877 electrically connected about 70 cotton mills with the fire station, this officer had already, in his annual report for 1880, the satisfaction of stating that " the loss by fire has been remark-" ably small as compared with previous years, and this I attribute " chiefly to the fact of our getting early intelligence of the out-" break of any fire, whereby we are enabled to get quickly on to " the spot and check it in its infancy."

In the United States of America, where fire-telegraphs are more popular than in England, the same favourable opinions prevail. Thus the Commissioner of the Brooklyn Fire Depart-ment, Mr. John Ennis, after having run telephone wires from his headquarters to all officers' quarters, fire stations, and to the Telephone Exchange, reports as follows:—" This is a great " improvement, and an important advantage to the department, " since we now can send one or two companies to a fire, when the " nature of the same is fully understood, instead of being obliged " to answer every call. Thus a large amount of wear and tear is " saved, and much greater protection incurred." After having erected in Brooklyn 400 electrical call-boxes, that officer continues to say:—" The street fire alarm-boxes should be increased as " rapidly as possible, as experience has proved that they are " indispensable to the successful working of the Fire Depart-" ment." These expressions are fully confirmed by the reports of the Brooklyn Fire Brigade, the results of which are given in Table No. 12. Although the number of fires has doubled since 1876—say 385 in 1876, and 800 in 1886—the loss of property nevertheless amounts now to only about half of what it was in 1876, or, in round figures, $500,000 against $1,000,000. Whereas the average loss of property per each fire amounted to about $2,000 in 1876, it only amounts now to about $800. This reduction in the yearly loss of property through fires is a natural consequence of the decrease of serious fires—viz., from 16 per cent. in 1876, to 9 per cent. in 1886—and it is mainly due to the gradual increase of public fire alarm-points, which in 1876 amounted to only 22, whereas in 1886 there were 321 public alarm-points in Brooklyn.

Table XII.

BROOKLYN.—COMPARATIVE STATEMENT OF FIRES AND LOSSES.

Year.	Population.	Total Fires and False Alarms.	Percentage of Serious Fires (not counting False Alarms).	Lives Lost.		Number of Public Alarm-Points.	Loss of Property through Fires.	Average Loss of Property per each Fire (including False Alarms).
				During the Year.	At every 100 Real Fires.			
1876	500,154	385	15·6	295	76·62	22	$ 712,490	$ 1,850
1877	516,129	371	16·2	6	1·62	22	1,125,656	3,084
1878	532,553	382	12·4	8	2·17	28	304,814	798
1879	549,440	404	13·6	5	1·26	28	608,243	1,505
1880	566,689	407	16·9	9	2·31	24	1,682,540	4,134
1881	586,520	510	20·8	6	1·25	83	825,947	1,594
1882	607,050	595	11·4	7	1·21	110	1,338,272	2,249
1883	628,300	689	10·7	6	0·89	111	793,666	1,152
1884	650,290	708	9·4	34	4·91	164	1,042,719	1,488
1885	673,050	814	7·6	20	2·49	202	663,666	827
1886	718,000	*719	9·0	6	0·86	321	420,469	601

* Eleven months only.

A similar result has been obtained in New York. Table No. 13 shows the number of fires and losses during the last twenty years, given in periods of five years. It will be seen from these figures that the average loss of property per fire was nearly 25 per cent. less for the last five years than for the lowest preceding five years since the organisation of the Fire Department. Although the number of fires in New York has nearly trebled since 1867, the average yearly total amount of loss remains practically the same, and the average loss of property per each fire is to-day less than half the amount it was twenty years ago. This, again, is the consequence of the proportionately smaller percentage of serious fires during later years, due to an increased facility of telegraphic communication.

Table XIII.

NEW YORK.—NUMBER OF FIRES AND LOSSES OF PROPERTY, 1867–1886, BY FIVE YEARS PERIODS.

Years.	Number of Fires.	Amount of Loss.	Average Loss per Fire.
1867 – 1871	4,685	$16,996,961	$3,628
1872 – 1876	7,274	14,668,513	2,017
1877 – 1881	8,223	19,770,469	2,404
1882 – 1886	11,470	18,292,695	1,595

In order to show how even comparatively small towns became well acquainted with the use of the fire-telegraph, the town of Aix-la-Chapelle, with only about 90,000 inhabitants, may here be mentioned. The official report for the year 1886 shows that the total number of 89 fires were brought to the notice of the fire brigade by the public in the following way :—

    In 78 cases by street call-points.

    „ 6   „   verbal reports at stations.

    „ 3   „   telegraphic messages.

    „ 1 case by telephonic message.

    „ 1   „   simultaneous telegraphic and telephonic

    —   messages.

Total,   89 fire alarms, of which 83 were received by electrical appliances, and only 6 by verbal report.

In the case of the town of Munich, a medium-sized city of 261,981 inhabitants, there were 76 fire alarms during the year 1886, which were received by the following means of communication:—

In 54 cases by street call-points.
  „ 6 „   gong alarm towers.
  „ 13 „   the Telephone Exchange.
  „ 3 „   verbal report at stations.

Total, 76 fire alarms, of which 73 were received from points included in the electrical circuits, and only 3 by verbal report at stations.

The chief officer of the fire brigade in Leeds, a still larger town, of 339,057 inhabitants, says in his last report:—" From " the street fire-alarms (only 12 in number) 38 alarms have " been received, and 29 were found to be genuine cases of fire. " In these cases the arrival of the brigade has no doubt been " expedited by the alarms."

· In the much larger city of New York the number of alarms during the year 1886 amounted to 2,643, of which 1,655 were received by electrical fire-telegraph appliances, and only 938 by verbal communication. About two-thirds of the alarms were, therefore, transmitted by fire-telegraphs.

Considerable advantage must naturally arise to any fire-telegraph system, and consequently to the protection of property, from the connection of such system with that of the Telephone Exchange. Such connection has already taken place in many of the larger towns; and although Exchange alarm-points cannot be considered in the same advantageous light as public street call-points, they have nevertheless proved in many instances to be valuable fire-alarms. Thus, for instance, the annual report (May, 1887) of the Edinburgh Fire Brigade shows that during the three years that the fire stations have been connected with the Telephone Exchange 18 fire alarms were received from the Exchange, viz., 7, 8, and 3, respectively.

The Superintendent of the Brighton Fire Brigade, Mr. Thos. Gibbs, in his annual report for 1886 tenders his thanks to the

Manager of the Exchange Telephone Company for his courtesy and assistance in forwarding immediate information of fires to the Town Hall, as several calls have been transmitted by the company's wires during the past year.

There is only one point more to which I should like to direct your attention, and that is the question how far the increase of alarm-points—that is to say, the combined increase of fire stations, telegraph stations, and electrical street call-points — offers a greater protection to the loss of life from fires.

Tables No. 3 and 4, which have already been referred to when speaking of the London fires, and Table No. 8, referring to the Amsterdam fires, show beyond doubt that, in spite of a . steady increase of alarm-points and a corresponding decrease of serious fires, the percentage of " lives lost " has not diminished, but slightly increased.

It is only natural that such apparently contradictory phenomena should not occur in London and Amsterdam alone, and in fact my researches prove that this increase in the percentage of lives lost is not the exception, but the rule, with most of the towns. In order to show this more clearly I have grouped in Table No. 14 all such towns which have already adopted street call-points, including cities of various countries, and have classed them according to the higher or lower ratio of street call-points to inhabitants.

With the exception of Nottingham, Baltimore, Magdeburg, and Prague, all other towns have diminished their ratio of serious fires conjointly with an increase of alarm-points. There can thus be no doubt about the reciprocity of these two elements of fire statistics.

But quite a different result will be observed when comparing the increase of alarm-points with the percentage of lives lost through fires. By far the greater number of towns show an increased rate of lives lost, in spite of a considerable increase of efficiency in telegraphic communication and in fire-extinguishing appliances.

This want of reciprocity between " lives lost " and " increased " communication " shows itself very strikingly in many of the

Table XIV.

EFFECT OF STREET CALL-POINTS UPON THE PROTECTION OF PROPERTY AND LIVES.

Towns.	Street Call-Points were adopted in the year	Since the adoption of the Street Call-Points "Serious Fires" have		Since the adoption of the Street Call-Points "Lives Lost" have		Number of Public Street Call-Points adopted, and Number of In-habitants at the End of 1886.		Proportion between Public Street Call-Points and Inhabitants.
		Dimin-ished.	In-creased.	Dimin-ished.	In-creased.	Street Call-Points.	Inhabi-tants.	
Chicago...	1874	dim.	...	...	incr.	1,349	725,000	1 : 537
Nuremberg	1878	dim.	...	none	none	99	116,000	1 : 1,172
New York	1867	dim.	...	dim.	...	1,103	1,429,697	1 : 1,296
Gotha	1881	dim.	...	none	none	17	28,000	1 : 1,647
Frankfurt-on-Main	1874	dim.	...	none	none	89	156,032	1 : 1,753
Aix-la-Chapelle ...	1872	dim.	...	(no record obtained)	(no record obtained)	51	90,000	1 : 1,765
Stockholm	1876	dim.	...	...	incr.	113	220,000	1 : 1,947
Amsterdam	1874	dim.	...	...	incr.	185	366,700	1 : 1,982
Baltimore	1877	...	incr.	(no record)	(no record)	207	417,220	1 : 2,011
Munich...	1879	dim.	...	...	incr.	111	261,981	1 : 2,360
Cologne...	1878	dim.	...	...	incr.	68	164,000	1 : 2,382
Brooklyn	1881	dim.	...	dim.	...	289	713,000	1 : 2,467
Prague	1878	...	incr.	none	none	98	253,500	1 : 2,587
Königsberg	1858	dim.	...	station-ary	station-ary	59	155,177	1 : 2,630
Dresden...	1877	dim.	...	...	incr.	65	249,220	1 : 3,834
Budapest	1872	dim.	...	(no record)	(no record)	98	422,557	1 : 4,312
Vienna	1876	dim.	...	(no record)	(no record)	180	786,000	1 : 4,367
Magdeburg	1880	...	incr.	none	none	35	175,212	1 : 5,006
Breslau	1863	dim.	...	...	incr.	55	303,480	1 : 5,518
Berlin	1853	dim.	...	...	incr.	246	1,362,384	1 : 5,538
Gand	1876	dim.	...	dim.	...	25	143,242	1 : 5,730
Glasgow	1879	dim.	...	(no record)	(no record)	82	538,915	1 : 6,572
Hanover	1859	dim.	...	dim.	...	20	141,500	1 : 7,075
Hamburg	1872	dim.	...	dim.	...	56	483,687	1 : 8,637
Copenhagen... ...	1874	dim.	...	...	incr.	22	290,000	1 : 13,183
Bolton	1885	dim.	...	...	incr.	7	108,000	1 : 15,429
Newcastle	1886	(not classed)	(not classed)	none	none	6	145,859	1 : 24,226
Nottingham	1882	...	incr.	...	incr.	9	234,000	1 : 26,000
Leeds	1884	dim.	...	dim.	...	12	339,057	1 : 28,255
Dublin	1883	dim.	...	dim.	...	12	353,082	1 : 29,423
Paris	1884	dim.	...	dim.	...	46	2,254,545	1 : 49,012
London	1880	dim.	...	...	incr.	347	4,149,000	1 : 119,594

reports before us.   For instance, Sunderland, Hull, Cork, Sheffield, and Blackburn, being all towns with a very insufficient distribution of alarm-points, nevertheless show no loss of lives during the last ten years; whereas other towns—London, Leeds, Hamburg, Cologne, Stockholm, Munich, Brooklyn, and others—show a high death-rate in spite of a favourable proportion between alarm-points and inhabitants.   With regard to these latter towns, Brooklyn may be considered as the worst case; because, in spite of possessing excellent and extended telegraphic facilities, consisting of 32 speaking stations, 289 street call-points, 111 private call-points, and a connection between the fire-telegraph and the Telephone Exchange, besides showing a proportion of one alarm-point to every 2,221 inhabitants, Brooklyn nevertheless presents, since the erection of street call-points, a rate of a little more than 2 lives lost per every 100 fires.

One of the most striking indications that the number of lives lost and persons injured through fires is not dependent on the efficiency of the fire brigade, is contained in the official reports of the Commissioner of the New York Fire Department, an extract of which is given in Table No. 15.   There we have a Fire Brigade which can boast of possessing all the most modern appliances of a perfect fire equipment, and of surpassing every other in rapidity of appearance at the place of conflagration; and still, out of 606 persons killed, or fatally, seriously, or slightly injured, through fires during the years 1879 till 1886, only 9 of these cases happened after the arrival of the fire brigade, but 597 persons are registered as being killed or injured " before the " arrival of the brigade."

These facts must lead us to the conclusion that the protection of life is not so much influenced by a greater efficiency of the fire equipment and by the extension of fire-telegraphs, but that it chiefly depends upon factors which lie very much beyond the reach of even the most efficient brigade.   On the other hand, it would be blindness to overlook the repeated cases, officially reported by many fire departments, which testify that frequently the names of firemen have been placed on the roll of honour for heroic bravery displayed in saving life, rescuing with great per-

Table XV.

CASUALTIES AT NEW YORK FIRES.

Year	Fires. Total Number of Fires.	Fires. Percentage of Serious Fires.	Number of Public Alarm-Points.	Casualties before the arrival of the Brigade, but not including Members of Department. Killed.	Injured. Fatally.	Injured. Seriously.	Injured. Slightly.	Total.	Casualties after the arrival of the Brigade, but not including Members of Department. Killed.	Injured. Fatally.	Injured. Seriously.	Injured. Slightly.	Total.	Per every 100 Fires were Killed and Fatally Injured.
1879	1,551	(Different classification)	613	...	12	44	10	66	...	...	...	...	0	
1880	1,783	(Different classification)	652	...	12	25	14	51	...	...	...	...	0	
1881	1,785	4·3	666	...	36	29	15	80	...	...	...	...	0	
1882	2,001	5·6	720	...	19	38	15	72	...	...	...	...	0	1·11
1883	2,169	4·4	792	...	26	16	15	57	...	...	...	...	0	
1884	2,406	4·1	845	7	8	28	35	78	...	2	1	1	4	
1885	2,479	4·3	1,043	8	24	56	21	109	1	...	3	1	5	1·06
1886	2,415	3·4	1,103	...	24	27	33	84	...	...	...	...	0	
1879 – 86	16,589	4·31	1,103	15	161	263	158	597	1	2	4	2	9	1·08

sonal risk to themselves persons from a third, fourth, or fifth story. But even these cases have not been sufficient to produce an apparent decrease in the rate of fatalities, and the best that can be said is, that probably the list of fatal cases would have increased still more had not the number of alarm-points generally been increased during later years.

The tendency of modern fires to become more fatal than in former years will have to be looked for in structural and industrial circumstances, in the concentration of population, and in the greater accumulation of inflammable household and other materials. I therefore agree with the chief officer of the Berlin Fire Department, who opines that the percentage of lives lost has been increased, in spite of increased fire-alarms and increased efficiency of the brigades, in consequence of a proportionately much greater number of factories and other establishments lately erected of a more inflammable nature.

Before recapitulating the conclusions drawn from the various tables before us, and referring to the effect which fire-alarm installations have had upon the decrease of serious fires, and consequently upon the protection of life and property, I feel compelled to repeat—what was said already eleven years ago—that I am well aware of the difficulty, or, rather, impossibility, of correct classification of " serious fires." There is, unfortunately, no recognised " unit " by which to determine the seriousness of a fire, and hence statistical tables will not be able to claim anything like perfection or mathematical exactness until a standard unit is recognised and used by all the fire departments.

Generally speaking, a " serious fire " is to be understood as a fire for the extinction of which more than two engines or reels are required. But this unit is not everywhere adhered to. Some fire brigades have adopted a much lower standard, and consider every fire as serious where more than one reel or engine, instead of two reels or engines, are required. This lower standard naturally gives rise to a much higher percentage of serious fires, as, for instance, is the case in the reports of the Munich Fire Brigade, where an average of 14 per cent. of serious fires in conjunction with a proportion of one alarm-point to every

2,000 inhabitants appears entirely out of proportion; and a much more favourable result would be obtained had Munich adopted the standard of the two reels or engines.

Other towns adopt the amount of property damaged or destroyed as the standard of comparison. Thus, for instance, at Manchester " serious fires " mean such where one-sixth or more of the property is damaged or destroyed.   This is rather a high standard, and hence the comparatively favourable result of only 6½ per cent. of serious fires in conjunction with the low proportion of one alarm-point to every 20,754 inhabitants.

Again, at the town of Salford "slight fires " are considered to be all those which result in damage under £2,000, whereas " serious " fires " show from £3,000 to £12,000 damage.   This is a very high standard, and fully explains the small amount of only 5 per cent. of serious fires in a town where only one alarm-point counts for every 24,562 inhabitants.

Some towns—such, for instance, as Aix-la-Chapelle—classify their fires in three instead of two groups, viz., serious, medium, and small fires; and other towns have adopted even a still greater divisibility.   To these latter belongs the city of New York, possessing four classes of fires, viz.—

      1. Such as are extinguished with more than three delivery hose ;

      2. Such as are extinguished with two or three delivery hose ;

      3. Such as are extinguished with one delivery hose; and

      4. Such as are extinguished with buckets of water only.

Notwithstanding all these and various other units of classification, I firmly believe that the adoption of a universal standard, and a consequently new classification of fires, would not practically alter any of the results to be drawn from the tables laid before you.   I therefore hope my critics will not reproach me that the tables are not absolutely comparable.   I must console myself with one of the highest authorities on fire statistics— Mr. Cornelius Walford, barrister-at-law, and author of the " Insurance Cyclopædia "—who earnestly complains of the difficulty of obtaining from fire departments, materials based on uniform standards, and who thinks that, " where the materials

" are incomplete and disconnected, we can do no more than make
" the best of facts as they stand."

Fortified by this exculpation, I will now recapitulate the facts
which result from the numerous official reports laid before you, viz.—

1. Since the subject of fire-telegraphy was first publicly
   introduced before this Society in 1877, a great general
   progress has been made in British towns and else-
   where in the increase of facilities for raising speedy
   alarms at the outbreak of fires. The time between
   the discovery of a fire and the appearance of the
   brigade has thus been considerably reduced.

2. Fire-telegraph street call-points were first introduced in
   London in the year 1880. They not only proved to
   be of great value in reducing serious fires, and thus
   saving property, but it also soon became evident that
   the fear of wilful damage to the fire-telegraphs and
   of false alarms being raised is not so well founded
   as to detract from the high value of a complete fire-
   telegraph installation.

3. Not only has the number of alarm-points been generally
   increased, but a limited number of British towns has
   already successfully adopted a system of public call-
   points.

4. Although a general increase of population, and a con-
   sequent increase in the total number of fires, has
   everywhere taken place, the percentage of serious
   fires, and with it the amount of property destroyed,
   has nevertheless been generally decreased. This has
   been the consequence not only of a greater efficiency
   of the brigades, but especially of the higher propor-
   tion of alarm-points to inhabitants, which, again, is
   identical with ampler provision of telegraphic appli-
   ances and with more rapid telegraphic communication.

5. Although the number of alarm-points has generally
   increased in British towns, and the percentage
   of serious fires has accordingly diminished, these
   undoubted improvements have nevertheless not yet

reached that point of perfection which they ought to attain. Both the proportion between alarm-points and inhabitants and the percentage of serious fires in British towns compare badly with Continental and American towns. Whereas in the former an average number of 27,000 inhabitants counts for each fire-alarm point, only about 3,000 inhabitants corresponds to every alarm-point in Continental towns, and even less in America.

6. As serious fires have become proportionately less, the total loss of property has not increased during the last ten years, although the total number of fires has nearly doubled. The present average loss of property for each fire is thus only about half, and often even less than half, the amount it was ten years ago.

7. The percentage of lives lost from fires has not been appreciably influenced by an increase of alarm-points. In spite of the increased efficiency of brigades and of more extended telegraphs, modern fires show a more destructive tendency, so that an increase in the percentage of lives lost is apparent not only in England, but generally. As the protection of life chiefly depends upon factors which lie very much beyond the reach of even the most efficient brigade and the most perfect telegraph equipment, the latter can only contribute in preventing a more rapid increase in the death-roll from fires.

8. The inhabitants of all towns which have adopted fire-telegraphs soon became used to the electrical appliances. Thus by far the greater number of fire alarms have been transmitted by street call-points and other electrical appliances, and only a very small proportion have been received verbally.

9. Considerable advantages accrue to any fire-telegraph system, and consequently to the protection of property, from the connection of such system with that of the Telephone Exchange.

Mr. President and gentlemen, before sitting down I beg to make one comparison, which, I hope, may smooth any roughness which these lines and figures might have created in the feelings of those who are responsible for a better state of telegraphic appliances in connection with the fire departments.

When, a few months ago, Colonel Lonsdale Hale and my much-esteemed friend Major C. F. Beresford, R.E., read a joint paper at the Royal United Service Institution on "Field Telegraphy," His Excellency the Right Hon. Viscount Wolseley, K.P., G.C.B., G.C.M.G., &c., &c., did not hesitate to lay before a distinguished gathering of gallant officers, and before the public in general, a most severe criticism, with the patriotic view of contributing to a much-needed national amelioration. His Lordship expressed himself as follows :—" I do not hesitate to say our forts at home " and abroad are in a condition which is discreditable to the " nation, and discreditable to every one who is responsible for " them."

I imagine that the figures and tables which I have had the privilege of laying before you just now, allow of a parallel criticism with reference to British fire-telegraphs in general ; and, being influenced by similar motives to those of the author of the above criticism, I can only add the expression of my sincere hope that a public appreciation of facts will stimulate a more rapid development of fire-telegraphy in this country.

The PRESIDENT : Captain Shaw, will you open the discussion ?

Captain EYRE M. SHAW, C.B. : I venture to say, Sir, that it would, I think, be better if some other gentlemen proceeded to make their remarks before me ; but, at the same time, I am at your orders. If I begin I may anticipate the remarks of others.

The PRESIDENT : Perhaps you are right, and that it will be better that you should favour us with your remarks later on.

Mr. SIDNEY SHARP : The paper we have just had the pleasure of listening to has, I am sure you will all agree, been very interesting and most instructive. We have had placed before us a record which will for years to come be a standard of reference, and will at a future time form a basis for comparison.

The annual loss of life and vast destruction of property by Mr. Sharp. fire raises the subject of fire prevention and extinction to the rank of one of the most pressing problems of the day.   This country is, as has been shown us by the author of the paper, very far behind the Continental countries and the United States of America in the manner of giving notice of fires.   Our fire brigades are thereby placed at a great disadvantage.   When they arrive at the scene of a fire, very often it has attained such huge dimensions that all their efforts are directed to saving neighbouring buildings rather than the one already in flames.   The consequence is that more engines and more firemen are called to the spot, and the town in other quarters is denuded.   Why do we not attempt to give earlier information ?   Even with all the street alarms, the incipiency of a fire may not be known until it has attained a strong foothold.   Street alarms are dependent upon human agency for their application.   A passer-by may notice a glimmer, but his curiosity is not sufficient to cause him to pause.   Later on someone else sees the glare ; he then calls the attention of a policeman, and they hurry off to the nearest fire-alarm and despatch a signal, and the brigade arrives, probably too late.   The huge dimensions of our fires, especially in London, making such heavy demands on the Fire Brigade, have caused an outcry for more men, which will entail heavier rates, and a demand on insurance companies to increase their contributions towards the expense.

I would go a step further than Mr. von Treuenfeld, and say that warehouses, large buildings, and factories should have their own alarms—alarms not dependent upon human agency, but dependent upon the fire itself : let the fire be its own telegraph messenger ; let it send its own alarm.   We have heard from the author of the paper that in Berlin 70 cotton mills are already connected up with the fire stations.   The same thing ought to be adopted in this country.   The reason that this has not been done with us is because people are unaware that there are means to carry it out.   Hitherto there have been three great drawbacks to what are commonly called "automatic" fire-alarms.   First, the slowness of action of the alarms themselves, which are

Mr. Sharp. generally dependent upon the effect of heat upon mercury or other metals. Well, we know that it takes a considerable amount of heat to expand mercury or metals; and whilst that is going on, of course the fire is increasing in strength.

The second objection to the systems hitherto in use consists in their having closed circuits, so that consumption in the cells is always going on and their strength running down.

The third drawback is the want of reliability—that when these alarms are called upon for action they are possibly not ready, or out of order.

Those are the chief reasons why I think automatic fire-alarms have not made greater progress in this country.

Having examined into several fire-alarm systems, I have found one which overcomes all the three drawbacks which I pointed out. I have placed on the wall a diagram showing an arrangement of thermostats in connection with a relay and bells which effects this object. The thermostats are small circular cases fitted with a Bourdon spring, containing a small portion of some liquid hydro-carbon which evaporises at 118° F. These can be set to go off at any predetermined temperature, say 10° F. above the highest normal temperature in the building in which they are placed. They are fixed on, or close to, the ceilings, whither the heat naturally rises; and should a fire break out the thermostat closes its circuit and raises an alarm, the indicator pointing out the spot. The specific heat of hydro-carbon is some twelve hundred times less than that of mercury, and therefore it can be readily imagined with what rapidity such thermostats act. Secondly, instead of a closed circuit, this system depends on an open circuit, and therefore no waste goes on in the battery. And, thirdly, in connection with the thermostats a watchman's clock and indicator are fixed. At the end of every fire circuit a plug is placed outside the building. It is an important point that watchmen should not be in buildings, as they are not needed there, and are much more safe outside, where their sleeping and smoking is less dangerous. The watchman pushes a key into this plug, sends current both ways round the core of the relay, and records his movements without ringing the bell. This

ensures the system being always in perfect readiness.   This Mr. Sharp.
combination has been in use for some four years now in America,
and covers some of the largest factories, mills, and warehouses in
that country: the Boston Storage Warehouse, the Washburn
Flour Mills, the Arlington, and others are covered with it.   There
has not been a single claim made on fire insurance companies
in the places where the system has been adopted.   It is not that
there have not been fires—indeed, there have been—but they have
been discovered in their early stages and put out.   By such a
system the brigade has a better chance of fighting the fire on
equal terms in its early stage.   The arrangement was described
in the *Electrical Review* last week, March 2nd.

I am sure we have listened with very great pleasure to Mr.
Von Treuenfeld's paper, and what I have said has been to
supplement it by referring to fire-alarms in buildings which
do not depend on human agency, but shall make a fire its own
messenger.

Mr. C. W. S. CRAWLEY: I have placed on the table, with the Mr. Crawley.
permission of the inventors, Messrs. Saunders & Brown, a fire-
alarm box which was sent to my firm the other day, and which I
thought might be of interest to-night.   Several boxes are in series
on a single-line wire, and when the handle is pulled a pendulum
breaks and makes the line circuit (which is normally closed), sets
in motion at the fire station a pendulum of the same rate of
swing, and rings a bell there.   It is re-set by sending a strong
current to line.   As many as three boxes can call simultaneously.
A circuit of five of these instruments has been at work for five
years at the Portland Road Fire Station.

With regard to Mr. v. Treuenfeld's paper, I should like to ask
him if, in taking the proportion of deaths to serious fires, he has
included those deaths due to non-serious ones.   If so, the increased
proportion is easily understood.   Electric alarms have materially
reduced the serious fires, but have not had so much effect on the
smaller ones, which cause by far the larger number of deaths;
hence the *proportion* is raised, although the actual number of
fires and deaths may be diminished.

The PRESIDENT: Before calling upon another speaker I may The President.

The
President. say that it appears to me that the last two speakers have got rather beyond the scope which the discussion should take. In his paper Mr. Treuenfeld said nothing about the special merits of any particular system, and from his tables it will be seen that some twenty different systems are referred to; and we can hardly discuss the details of new systems while the old ones are unexplained, as there are no advocates for them. Therefore I think the discussion should not take the form of advocating the merits of any particular kind of instrument.

Mr.
Bright. Mr. E. B. BRIGHT: I have certainly made the subject of fire-alarms a study for a number of years. On the present occasion I quite agree with you, Sir, that we are dealing on broad principles and statistics, and not with the details or value of this or that alteration or fresh suggestions as to apparatus. I should have referred to the very point that was brought forward by Mr. Sharp as to the fact that the street fire-alarm posts do not really deal thoroughly with the question—they are only auxiliaries—but that point has been already brought into the discussion.

The only way to really grapple with fires is to arrange that the buildings themselves give the alarm in an automatic manner, by means of a large bell or gong acting outside the doors of large warehouses or buildings, or to some occupied part of the building, directly the temperature has unduly increased in any room. Then the alarm call can be quickly given either from the building itself or from the nearest call-post to the fire brigade station, with the indication of the spot. This is the only way to nip fires in the bud.

I lectured on this subject from 1877 for several years, in Liverpool, Manchester, Leeds, Bradford, and many other places, bringing experimental apparatus before the Town Councils and in the presence of the fire brigade officers, showing what aid such a system of house-to-house fire-alarms would be. The result is, after all my trouble and expense, that they have scarcely ever been adopted. I was led to communicate with the fire insurance companies, as it appeared to be a manifest benefit to them if the loss by fires could be greatly reduced. I was in communication for years with some of the leading insurance companies, specially

introduced by their directors to the managers, and the answer that I got was a very curious one. I was told, "Yes; if this " apparatus is introduced into the different warehouses and mills, " and if fire-alarms become very general, we shall have no large " fires." I was told distinctly by the manager of one of the very largest fire insurance companies in this kingdom, "We do not " want that: we find that if there is an extensive fire it brings " in a shoal of insurances, and we would much rather not have your automatic appliances."

Mr. F. HIGGINS: Having been engaged in introducing fire- alarms to this country before Mr. Bright, and having succeeded with the Metropolitan Board of Works in placing the first electric street alarms which were ever adopted in England, I should like to say a few words on the subject. We have had precisely the same experience as Mr. Bright. In endeavouring to induce almost everybody in the kingdom to adopt fire-alarms, we failed in precisely the same manner. The fire insurance companies prefer to take the risks exactly as they find them. If they reduced risks they would have to reduce their premiums and their funds all round, and they therefore do not require to do anything of the kind. We have some very efficient automatic fire-alarms, which can be set to go off at a change of temperature of only one degree, and signal the name of the house, in print if necessary, to the fire station: any one house, or any one room, out of 500 may send a signal which will transmit the name of that room without any difficulty whatever; and the whole cost for maintenance of such a system would not amount to one shilling an instrument per annum. But, notwithstanding that, we have always failed to induce anyone to take a sufficient number to enable us to set up a district. In the United States the American district system supplies messengers on a similar call arrangement, and provides automatic fire-alarms; but in this country the people will not take it. We set up a system in the City of London with 800 signals upon it, and at the end of three years' work we only had six subscribers, all the others being gratuitous; and I think the prospects of fire-alarms in this country are somewhat discouraging.

Mr.
Higgins.

I really do not know how our fire-alarms work in London; but on a previous occasion Mr. Bright, in explaining his system, had a very large map with his posts placed about it, each covering about an acre of land, and the lines of communication about 100 yards wide. He explained, in reply to a question of Mr. von Treuenfeld, that the large space apparently unfilled in the centre was principally occupied by Hyde Park. That space was from Pimlico to Whitechapel and from Islington to Clapham, and it was occupied by the fire-alarms of the Exchange Telegraph Company; but as our Chairman this evening has decided that we must not discuss the merits of any particular system, I have nothing more to say.

The PRESIDENT : Perhaps Mr. H. E. Hall will give us the views of the insurance companies ?

Mr. Hall.

Mr. H. ERNST HALL: I am very glad indeed to have an opportunity of saying a few words on behalf of the Insurance Companies, because a statement made by Mr. E. B. Bright certainly took me very much by surprise. It is altogether new to me that insurance companies like large fires. I can only say that if they do they have had their taste gratified in the last few years.

Insurance companies have nothing in the world to do with fire extinction : their function is to provide an indemnity for the public against loss by fire. It is for the public to put out their fires; it is for the public to provide appliances for doing so; and it is very unreasonable to ask the insurance companies to contribute towards these expenses. If, owing to increased appliances in any particular district, the number of fires decreases, or the amount of loss that is done by the fires diminishes, the result is that the premiums, which are settled by the aggregate amount of losses which the insurance companies suffer, must be reduced, and the public themselves get the benefit immediately. The insurance companies do not like large fires; but what they do like, and what they do not get, is to have small fires at Hampstead and elsewhere in the suburbs, to keep up people's taste for insuring, and then to escape large fires in the City.

With regard to the general subject of the paper there cannot

possibly be two opinions. Any appliances which tend to enable
the brigade to come quickly to the spot and to deal with a fire in
its incipient stage must be of the greatest possible advantage,
because it stands to reason that a very small amount of water
when a fire first commences may do ten thousand times more
good than tons of water a quarter or half an hour afterwards :
that is a self-evident proposition.

A great deal has been said to-night about automatic appliances.
With regard to these appliances, in the first place, as has already
been said, they are not reliable.   One cannot always be perfectly
sure that they will be in working order, and if they are ever out
of order it is sure to be at the time when they are wanted.   More-
over, automatic appliances tend to this : people place so much
reliance upon them that when a fire breaks out they are not
disposed to send at once for the brigade.   If they have " automatic
"sprinklers" and things of that kind they trust to them to put the
fire out, and it is only when they have been found to fail and the
fire has got somewhat ahead, they think they must send for the
fire brigade; consequently the brigade is not then in such a good
position to cope with the fire.

We have had a great many valuable statistics placed before
us to-night, and Mr. Treuenfeld has very fairly dealt with· the
difficulty which arises in comparing them, viz., that the same
thing is not meant everywhere by the term "serious fire."   Of
course, in comparing the yearly statistics in any particular place,
they are fairly reliable ; but you cannot trust to them when you
come to compare, for instance, the serious fires in London with
those in Liverpool, Manchester, or abroad.   I will explain what
I mean by taking an instance.   Under the Scotch Police Act
the local authorities are empowered to charge half the expense of
extinguishing a fire, but not exceeding £15, to the owner of the
house where a fire occurs.   The authorities read that Act in this
way: Supposing a fire begins in one house, and spreads to
another, and then to a third, they treat that as three fires,
because they say, " Oh ! it is perfectly true that it was one fire
" that broke out in the first house, but it was a fresh fire when it
" extended to the next house, and so you must pay us another

Mr. Hall. "£15." I suppose if they got three very large houses they would treat them as three "serious fires," whereas probably Captain Shaw would call it one serious fire. I will give an example to show how fallacious it may be to compare the statistics of different places. Suppose you take the case of the great fire of London—I mention that because it is one which most of us have heard of—the great fire of London would, if Captain Shaw had been alive in those days, probably have been put down by him as one "serious fire"—and undoubtedly it was a serious fire—but if that fire had occurred in Scotland the local authorities would have put it down as some thousands of fires.

The PRESIDENT: 13,000 houses were burnt.

Mr. HALL: .The charge for extinguishing that fire would have been that many times £15; and that would have been the charge for extinguishing a fire which, I believe, in fact never was extinguished, but burnt itself out.

With regard to the statistics of fires in London, it is perfectly clear from Mr. Treuenfeld's tables that the number of " serious fires " has in the last twenty years very materially diminished in London, the reduction being from 9 to 7 per cent. during the last eight years. But I think he takes rather too much credit to the fire-alarms for the diminution in the number of serious fires. There are a great many other causes that have been at work. It must be borne in mind that the area within which serious fires occur in London is limited. It extends over the City and the district in which there are large shops, but this area has not been materially extended within the last few years; whereas London has been growing very considerably in the outskirts, where there is not the chance of " serious fires," for they do not occur in dwelling-houses and such like. Therefore, while fires have taken place in greater number, there has been no increase in the number of serious fires.

Mr. Treuenfeld was rather unhappy because he did not find a corresponding reduction in the number of lives lost. I think that arises from the converse of what I said just now, viz., that the area within which lives are endangered, owing to the building of dwelling-houses in the outskirts of London, is greater than

it was, and therefore more lives are endangered now than ^{Mr. Hall.} formerly.

Some reference was made in the paper as to the experience of the fire insurance offices in America and elsewhere. All I can say is that I think those who are better acquainted with the returns of fire insurance companies' business than I am will hardly agree that the American business has been by any means profitable of late years.

Captain EYRE M. SHAW, C.B.: Allow me to say, in com- ^{Capt. Shaw.} mencing, that I approach this subject from a totally different point of view to that of all the gentlemen professionally engaged in electric telegraphs: I am the user, while they are the makers. As to the paper itself, I have very little to say except in the way of the most friendly criticism. I had an opportunity of reading it before coming here, and really there are very few points which I would wish to raise.

At the commencement I find that Mr. Treuenfeld says that the progress of fire-telegraphs has been tardy to a deplorable extent. I think if he had studied a little the nature of public life in England he would not have found fault with that at all. Those who are engaged in the use of these telegraphs understood their value quite as much as those who were professionally interested in their introduction, and it has only been due to the exigencies of public life that they are used in such small numbers at the present day.

Since fire-alarm call-points have been introduced in London, we who have to use them have received very great benefit from them. The statistics for the year just passed show that we have had calls amounting to 2,363. Of those calls no less than 1,195 were received at fire stations through the electric call apparatus; and I will explain to you the trouble which we have had to encounter with that apparatus, in order that you gentlemen who make the instruments may have an opportunity of judging of what difficulties have to be overcome in making them useful to us. Of those fire-alarm calls during the past year, 170 were distinctly traced to malice on the part of the public; 74 were unknown, and we could not discover whether they were malicious

or not; 91 were due to the lines being out of order; 13 were caused by men working on the line ringing the bells by accident; 13 were sent by persons who supposed from seeing smoke coming from a chimney that there was a fire; and 14 were from what seems to me a most extraordinary cause—that is, collisions of carts or other vehicles against the posts; making altogether 375 false alarms. Those false alarms are of course a very great trouble to us.

On the other side, I have to say—and I do so with very great pleasure—that we received no less than 820 good and valuable calls, which enabled us to save a very large amount of property, and probably of life. Certainly we have had cases, even within the last few days, where lives were saved, not by the fire escapes which first reached the fire, but by a strong body of men coming on with the first engine, which was called by an electric alarm. We had a case in Houndsditch within the last very few weeks in which such a thing as that occurred. Lives were saved by the fire engines which came on, and which could not have possibly reached the scene in time but for the fire-alarm call-point.

With regard to the trouble which we receive from these false alarms, I have studied what goes on in other places, and I am in the same difficulty with regard to these calls as Mr. Hall said the reader of the paper was about his general statistics—that is, the difficulty of getting at the truth. I do not know whether there are any gentlemen from America here, and I should be the last to offend anybody from that country, but I must say that their statistics are absolutely impossible of comprehension. For the purposes of this meeting to-night I have studied some of my books, which are at my fingers' ends, because I wanted to get up a few figures for you, and I found that some of the statistics are quite unintelligible, and, I fear, not altogether trustworthy; but they are the best which can be obtained from the books in my possession, which books are authentic documents given to me by persons with whom I correspond in the States and elsewhere. In Baltimore, which is full of these fire-alarm call-points, I find that last year there were 438 calls, out of which there were 170 false alarms, while 268 were for fires. That is an enormous per-

centage of false calls out of the total.   In Boston I am still more
perplexed, because I have to tell you that there were 927 total
calls: there were said to be 730 fires, but the false alarms are
either 197 or 519; and I tell you that I have studied the return
for hours to-day—and I am an old statistician—and yet I cannot
say whether the false alarms were really 197 or 519.   I mention
this for the purpose of pointing out that if any gentleman present
wishes to pass an undue criticism on the distinguished reader of
the paper to-night, at all events the latter has this to go on—that
he has taken perhaps the best statistics at his disposal, and has
done with them as he could according to the lights which he has
for reading the book, and that every allowance may be made for
errors on his part; and when I, who have studied these things
for years, cannot make out from the papers what the numbers
really are, I think he may be very well excused for a few
mistakes, if he has made any.   In Chicago the total calls were
1,796, of which no less than 349 were false, and 1,447 for fires.
349 is an enormous number of false alarms to have in one town.
In Cincinnati the total calls were 587, and, from what I can make
out, the fires were 129, while the false alarms were 358, or nearly
treble.   As to Detroit, there, I think, the figures are a little more
authentic than in some other cases — 338 total calls, 90 false
alarms, and 248 fires.   Coming to New York, which is a great
difficulty always, the total calls were 2,643, of which 1,380 were
false and 1,263 for fires.   In San Francisco there were 525 calls,
299 being false and 226 for fires.   Lastly, we come to London,
which I have kept till the end.   Here we had 3,059 calls, of which .
696 were false and 2,363 were for fires: this does not include
chimneys, and I am afraid that some of the other returns probably
do.   When you compare that with what I have said about our good
calls and our bad calls—that out of 1,195 fire-alarm calls 820 were
good—you will understand that we in the Fire Brigade have great
reason to be indebted to the gentlemen who have perfected the
electric fire-alarm system so far as it has gone.

   We have heard from Mr. Treuenfeld that there has not been
a corresponding diminution of loss of life; and that, I think, may
be quite understood.   He asks in the early part of his paper what

Capt. Shaw. I have to say on the subject of a statement made by me many
years ago that I hoped that one day the introduction of these
alarms would lead to a diminution of loss of life.   Well, gentle-
men, I think I may say that it certainly has done so to a small
degree; and, without being too confident about the matter, I do
say this—that I am perfectly prepared to go on hoping; and that,
I think, is as much as one can say in the present state of the
system we have at work.   We have commenced on a small scale,
and with some difficulty; we have proceeded gradually; and now,
with the great assistance which I have received from your worthy
President who is in the chair to-night, and from his very able
assistant, Mr. H. Eaton, whom I see present, we have advanced to
such a point that, at all events, the work of the Fire Brigade is
very materially reduced by the introduction of these electric fire-
alarm call-points, and the loss of property has been reduced in a
still greater ratio.   Whatever the loss may be now, it would be
very much greater but for these fire-alarms.

As to automatic fire-alarms, of course it is very interesting to
hear the statements which have been made to-night, and one can,
to a certain extent, corroborate them; but the speakers have only
referred to the advantages.   But there is a serious point in con-
nection with them as regards their uncertainty of action.   I
should like to know what system of automatic fire-alarms could be
placed, say, in this room, which is not a very large one—not
anything like the size of some of the places in London which
contain vast quantities of valuable property;—in what part of this
room would any of the gentlemen who make these automatic
fire-alarms place the alarm so that it should be certain to act if a
single door or window were opened, or even a small portion of a
curtain?   They would have to be put, as far as I know, almost all
over these vast rooms; and you will remember it is only in the
vast warehouses that it would be worth while to introduce them
at all.   They may perhaps be got into rooms of moderate size in
hotels; but it is probable that the opening of a door or window,
the extinguishing or lighting of a fire, may set them entirely
wrong.   That has been the difficulty throughout.   I know that in
America these automatic alarms have been introduced; but then

I know also that in America they have been worked by the same ^{Capt. Shaw.} people who are themselves the insurance agents, and that, in fact, the gentleman who had an interest in working as an insurance agent also had an interest in advertising and working the machine ; and therefore this double arrangement has resulted in a want of confidence in the machines all over America, as far as I know.   I regret very much to have to say it, because I am always inclined to support everything in the way of automatic action which will assist us in the discharge of our duties, and, of course, contribute to the reduction of loss by fire.   But in America the difficulty has been that men have been engaged in the two operations, and a gentleman who is engaged as agent to a fire insurance company will in that capacity have the power of reducing premiums on property in which the other thing, in which he is also interested, has been introduced.   The result is that the public in America has not got as much confidence in these things as it would have had if they had been in entirely separate hands.

As to the tardiness of introducing electric fire-alarms.   As many as twenty or twenty-five years ago these alarms were brought here from other countries and were put up in my place with a great flourish of trumpets, but they were unable to work from room to room.   They got out of the hands of such gentlemen as I see about me to-night—the scientific workers—and got into the commercial hands, too soon, and the result was that improvement stopped at that point.   For years I was pestered with these things being brought to me, all of which, without any exception, turned out after a few days to be perfect trash, and useless for my work.   It was most disappointing to me ; and all those engaged with me in business, and who have to do with such matters as we are discussing to-night, know how disappointed I was ; but I was obliged to put my foot down and say, " You must " go on improving your instruments, and when you have got them " into a state in which I can use them I shall be perfectly ready " to recommend them to the authorities under which I serve."

I have brought here a map of the whole of the communications which I have in London now.   It is rather large, and there is not

Capt.Shaw. sufficient time to go into its details, but it is available for the inspection of anyone who is interested in the subject.

With regard to fire-alarms generally, there are two or three suggestions which may be of great use if the system is to be proceeded with at any rapid pace. First, in public alarms, the fire brigade must be in some way protected from the constant interference of persons, whether wilfully or for amusement. There are always a certain number of persons doing punishment for such freaks, but they are only a small proportion to the total number. The cure, I presume, will be—and I speak with due deference to all here—that anyone who rings a bell must at the same time make such a noise—in fact, such a row—in the street that he will wake everyone in every house for a hundred yards, and so call attention to his action ; and the fact of doing so will terrify persons who would otherwise be inclined to raise alarms wilfully for their own amusement. Another point is with regard to fire-alarms from warehouses. Our experience—which has now been growing for a few years, as I have many of them always at work—has reached this point, that I believe we ought not to have any more of mere bell-ringing. The worry and anxiety of a bell is very great. We ought to have speaking apparatus of some kind, and I presume it will not be very difficult to make it cheap enough —at least, if a great number of persons adopt it. The mere ringing of a fire-alarm bell is a very troublesome thing to a fire brigade. After having been out, say, 18 or 20 hours, to come in and have one of those rings to which we reply in force, and then to find that it is nothing at all, is very troublesome. If, instead of a fire-alarm only, it were a speaking arrangement, such as a telephone, information could be sent exactly as to what was wanted, and in many cases we should be stopped from turning out at all. Many of the false alarms during the past year have come from useless ringing up, and it is, I think, undesirable to increase the number of bell-pulls in connection with our stations unless there is some protection to us from turning out un-necessarily.

Only within the last few days a very important building, which belongs to what we call a customer of ours—Mr. Whiteley—in

POND SQUARE

HOLLOWAY
26

HAMPSTEAD
14

KENTISH TOWN
25

ISLINGTON
17

St JOHN'S WOOD
13

CAMDEN TOWN
24

B

CITY Rd

SHORED
37

N.KENSINGTON
10

CLERKENWELL 16

St LUKES
18

BISHOPSG
36

PORTLAND Rd
23

PADDINGTON
11

TOTTENHAM COURT Rd

HOLBORN
21

FURNIVALS INN

OLD JEWRY

BAKER St
12

22

Gt MARLBORO' St

LUDGATE CIRCUS

WATLING

NOTTING HILL
9

A

SCOTLAND YARD
20

WATERLOO

KENSINGTON
8

SCOTLAND Yd FLOATING
27

58

HEAD QUARTERS
SOUTHWARK

TOOLEY St

KNIGHTSBRIDGE 7

2

KENNINGTON
40

45

HAMMERSMITH

CHELSEA 5

WESTMINSTER

BROMPTON
4

FLOATING
15

Oval

FULHAM
3

BRIDGE Rd

BATTERSEA

SMEDLEY St

CAMBERWELL GREEN

50

57
BATTERSEA

CLAPHAM
55

BRIXTON
53

EAST DULWICH

PUTNEY

56
WANDSWORTH

TOOTING
54

LR NORWOOD
52

LOWER TOOTING

STREATHAM

LOWER NORWOOD

SYDENHAM
51

UPPER NOR

RIVER THAMES

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MAP OF THE
METROPOLITAN FIRE BRIGADE
ON THE 1ST JANUARY, 1888.

● FIRE BRIGADE STATIONS
○ POLICE STATIONS

The straight lines represent communication by telephone.
In addition to these there are 54 Alarm Circuits round
Fire Stations with 351 call points which are not shown.

EYRE M. SHAW
Chief Officer, M.F.B.

SCALE OF MILES

0 ¼ ½ ¾ 1   2   3   4

Jurisdiction of the Metropolitan Board of Works

C

RIVER THAMES

VICTORIA PARK

BOW
32

BETHNAL GREEN
MILE END
31

33 POPLAR

NORTH WOOLWICH

WOOLWICH
43 ○ WILLIAM ST.

ISLE OF DOGS
○ MANCHESTER RD

44
DEPTFORD
○ EAST GREENWICH

47
GREENWICH

SHOOTERS HILL
46

BLACKHEATH
45

BROCKLEY

○ HIGH STREET

ELTHAM ○

LEWISHAM
46

SYDENHAM

Limit of the Jurisdiction of the Metropolitan Board of Works

connection with which there is a bell-pull, rang us up, and we Capt. Shaw.
turned out in very great force in reply; but the ring was entirely
due to an accident, caused by one of the *employés* in the house,
who thought he was doing one thing and did another, but the
result was that he rang the bell. They were in the greatest
trouble about it, and probably in deadly terror of the insurance
companies; but I treated it as a case of ordinary occurrence, and
said no more about it. That shows the difficulty we are under—
that, with all these improvements, we are still liable to be called
out a greater number of times than we would be if these things
were not in existence. That is a point which you gentlemen who
are engaged in this business should turn your attention to. We
want no more bells to be brought to the stations : everything
should be done by telephones, and those should be worked by
responsible persons; and when such a system is introduced, I shall
encourage it in every possible way. I may tell you, and Mr.
Eaton will confirm me, that at the present moment we have not
got in the Fire Brigade one single telegraph. The whole of the
telegraphs have been abolished, and we have speaking telephones.
The result is an enormous advantage to the working of the
institution, not only for information about fires, but also for
communicating with each other throughout the whole 59 stations.
If that can only be introduced in some way, or encouraged in the
way of providing fire-alarms as against mere bell-ringing, it will
be a very great advantage to all.

In conclusion, I have only to say, Sir, that I feel very much
interested in the paper which has been read to-night, and that, as
far as I am concerned, I shall endeavour to profit by the
information that we have all received from it.

Mr. R. von Fischer Treuenfeld, in reply, said : I should say Mr.
Treuenfeld.
that since my paper was written the report of the Chief Officer
of the Metropolitan Fire Brigade, whom we all thank for the
honour he has given us of being present this evening, has ap-
peared for the year 1887, and it contains the satisfactory result
that the percentage of serious fires remains at the previous
average obtained for London of 7 per cent. But, on the other
hand, this official report also states that the percentage of lives

lost has again been increased—viz., 55 lives lost, and 2,363 real fires, or 2·33 per cent.—during the past year, which is the highest figure obtained during the last twelve years, if not more, but I have not gone further back. The percentage of lives lost in 1886 only amounted to 2·28. This increase has taken place in spite of all the improvements, of the increased attention of the brigade, and of the further development of the means of rapid telegraphic and telephonic communication.

With regard to the question Mr. Crawley raised, I must say that the death-rate in my paper includes all such persons that have lost their lives in consequence of fires, quite irrespective of whether the fires were small or serious ones; but it does not include persons who belong to the fire brigade who lost their lives in the operations of extinguishing fires, nor such persons who have gone home drunk and have fallen into the fire and been burnt to death, or those whose lives have been lost through the upsetting of paraffin lamps and similar accidents.

As to Mr. Hall's remark, I must say that I had a certain hesitation myself in bringing the paper before the Society. I thought it would probably be, as Mr. Hall expressed it, a " self-" evident proposition." I quite agree with him that it is a self-evident proposition to give the fire brigade the means of appearing as quickly as possible at the place of conflagration. But if people fail to learn the lesson placed before them it is well to preach it to them again and again; and seeing that they had profited little by the lesson I pointed out eleven years ago, I thought it well to again bring the subject before the Society.

The difficulty as regards the standard for " serious fires " was repeated by Mr. Hall, and I quite agree that it is very awkward to deal with existing standards, which are so widely different; but I can only say that, as I have always said, whatever standard be adopted, the great bulk of the results brought out by the paper will remain the same.

I must certainly acknowledge my thanks to Captain Shaw for having expressed the opinion that he agrees with the principal points brought forward by me, and I am also thankful that he acknowledges the tardiness of the introduction of the system.

I purposely abstained from giving the explanation of that tardiness, Mr. Treuenfeld. and we are certainly obliged to Captain Shaw for having laid the explanation before us.

As to my American statistics, I must say that I received them all from the published official reports of the chief officers of the fire brigades; so that if American fire statistics should not be reliable when received from the chief officers of those brigades, I certainly must feel sorry for their errors; but I cannot quite accept this proposition.

Mr. H. ERNST HALL: May I be allowed to say just one word Mr. Hall. of explanation? I hope that Mr. von Treuenfeld did not misunderstand what I meant in saying that his proposition was self-evident. By that remark I did not mean for one moment to suggest that his paper, or any part of it, was superfluous. I am sure the paper was most valuable, and, for my own part, I have listened to it with extreme interest.

The PRESIDENT: Late as the hour is, I think we can hardly The President refrain from expressing our obligation to Mr. von Treuenfeld for the paper he has brought before us. Although not technical, it is none the less interesting. He has compiled, evidently with very great labour, a batch of statistics which will be of great value at some future period when they can be compared with the facts then existing. That they are of great present interest is certain —an interest not only to insurance companies, but to individuals; for there can be very few people who like to have their property accidentally consumed by fires, or who would like to be involuntary subjects for cremation. I beg, therefore, to propose that a hearty vote of thanks be accorded to Mr. von Treuenfeld for his paper; and, in doing so, I am sure I am only anticipating the feeling of the meeting in assuring him that we all deeply sympathise with him and his countrymen in the great loss which they, and, indeed, the whole of the civilised world, have sustained through the death of the German Emperor.

The motion was carried unanimously.

A ballot took place, at which the following candidates were elected :—

Member :
Mark H. Robinson.

Associates :

H. B. Bourne.

L. H. Chase.

George Edward Hartmans.

Arthur P. Haslam.

Frank Walter Henton.

Edward H. Miller.

William Charles Mountain.

A. Harris Wickenden.

Students :

Peter V. McMahon.

Eustace Oxley.

Gustav H. C. Rish, Jr.

The meeting then adjourned until March 22nd, 1888.

The One Hundred and Seventy-sixth Ordinary General Meeting
of the Society was held at the Institution of Civil Engineers,
25, Great George Street, Westminster, on Thursday, March
22nd, 1888—Mr. EDWARD GRAVES, President, in the Chair.

The PRESIDENT: I have to state that, on account of illness, the
Secretary is unfortunately unable to be present to-night, and his
assistant, Mr. F. E. H. Webb, will therefore act for him.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to
suspended.

The following transfers were announced as having been
approved by the Council:—

From the class of Associates to that of Members —

> Rudolph Howard Krause.
> Michael Holroyd Smith.

From the class of Students to that of Associates—.

> Albert Lewis Davis.

Donations to the Library were announced as having been
received since the previous meeting from the Institution of Civil
Engineers; Herr F. Uppenborn; Messrs. Whittaker & Co.;
Jacques Manne, Foreign Member, to whom a hearty vote of
thanks was accorded for their presentations.

The PRESIDENT: I have to present, and call your attention to
the Annual Balance Sheet of the Society for the year 1887, a
copy of which has been sent to each Member, and which is now
on the table, and to ask if any one wishes to comment thereupon.

As no remarks appear to be forthcoming, I beg to move that
the Balance Sheet for the year ending 31st December, 1887, as
now presented, be received and adopted.

Mr. T. BUCKNEY having seconded the motion, it was carried
nem. con.

The following paper was then read :—

ON THE OPTICAL DEMONSTRATION OF ELECTRICAL STRESS.

By A. W. RÜCKER, M.A., F.R.S., Member, and C. V. BOYS, A.R.S.M.

A very beautiful optical method of demonstrating the stress which exists in a dielectric in the neighbourhood of charged conductors was some years ago described by Dr. Kerr (*Phil. Mag.*, l., pp. 337, 446, 1875, and viii. [5], pp. 85, 229, 1879, and xiii. [5], pp. 153 and 248, 1882). His experiments have been repeated and extended by Gordon (*Phil. Mag.*, ii., p. 203, 1876), Quincke (*Wied. Ann.*, x., p. 538, 1880), Röntgen (*Wied. Ann.*, x., p. 77, 1880), and others. Mr. Gordon, using glass as the dielectric, exhibited the phenomenon in a lecture delivered at the Royal Institution (*Proc. Roy. Inst.*, 1879). Professor Silvanus Thompson tells us that he showed it to his class some years ago with carbon bisulphide; and it is probable that others may have done the same; but even in its most simple forms the experiment is not yet as often exhibited as its interest and importance demand.

We have recently had occasion to show it, and have devised some modifications which are, we believe, novel. Some of these were exhibited to the Physical Society a few weeks ago; but, as we have since added considerably to the list of the experiments, it may not be uninteresting if we give a fuller account of them. In order to exhibit more completely the logical order of the experiments and the simple nature of the apparatus employed, we shall describe several which differ from Dr. Kerr's only in the fact that the apparatus has been modified so as to render the effects visible to an audience.

We do not consider it necessary to give at any great length the theory of phenomena which are, for the most part, well understood. Our object in this paper is not only to describe some modifications and extensions of Dr. Kerr's experiments, but also to draw attention to the use which can be made of them for teaching purposes.

The fact that the dielectric plays an important part in electrical phenomena may be illustrated by a lecture experiment based on the method originally used for measuring specific inductive capacities.

Three insulated metal plates are arranged side by side, and parallel to each other (Fig. 1). The two outer ones (B and C) are connected with the opposite pairs of quadrants of a quadrant electrometer by wires which dip into insulated mercury cups. The plates and quadrants are first put to earth by connecting the cups by wires with a third uninsulated cup. The central plate (A) can then be charged without disturbing the electrometer needle. The earth connections being broken, a plate of paraffin attached to a long arm of ebonite carried by a wooden platform which travels
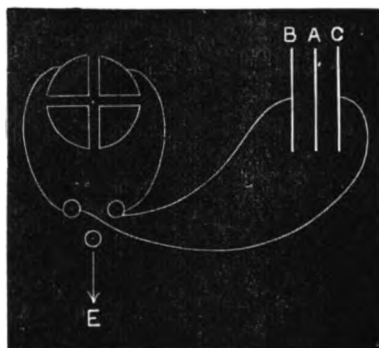
Fig. 1.

in a groove in a baseboard, is pushed between two of the plates (A and C, say). A deflection of the electrometer is immediately observed in the same direction as that which would have been caused by bringing C nearer to A. This proves that induction takes place through paraffin more readily than through air.

To meet a possible objection—viz., that the effect is due to the surface of the paraffin having become a conductor either because it is damp or dusty, or from some other cause—the plate of paraffin may be put to earth when in position between the plates A and C. No effect is produced on the electrometer. If, however, the experiment be repeated after the paraffin has been enclosed in a tin-plate sheath, although the deflection at first will be similar to

that which was previously obtained, its direction will be immediately reversed when the metal is earth-connected.

Although this experiment is well fitted to impress upon students the fact that the dielectric plays a part of primary importance in electrical phenomena, yet the lesson is still more effectively taught by the optical demonstration of the stresses which are produced in it. Some of the experiments we are about to describe are indeed too complicated to be usefully shown to students who have not a considerable knowledge of the properties of polarised light. In other cases a comparison of the behaviour of the non-conducting medium with that of stressed glass when placed between crossed Nicols would be legitimate and useful even when it would not be desirable to enter into a detailed explanation of the causes of the luminous effects observed.

The dielectric we have employed is carbon bisulphide, which must be carefully dried and filtered. This substance exerts no solvent action upon shellac, and it may therefore be used in wooden cells with glass ends. The glass plates fit into grooves in the sides and base, and leakage is prevented by running shellac varnish along the junctions. The condensers to be used are attached to glass plates, which serve as covers to the cells. Holes are drilled in these, through which pass the wires which support the conductors and connect them with the electrical machine. The wires are held in their places by shellac.

It is important that the cells should thus be covered, and that they should not leak; otherwise the inflammable vapour of the CS_2 is apt to ignite if a spark accidentally passes in the neighbourhood of the cell. To make it absolutely certain that no danger can arise from this cause, or from the fracture of the glass ends of the cell by a discharge within it, the apparatus may be placed in a larger cell without a cover, which, in turn, stands on a wooden tray the edges of which are about an inch high. A rectangular tin-plate extinguisher is also provided, which, when placed in the tray, encloses the two cells. Should the vapour ignite, the apparatus can thus be enclosed and the flame smothered. If the cells are carefully constructed, and the glass

covers fit well, these precautions are perhaps unnecessary; but
they involve little additional trouble, and it is safer not to omit
them.

In order to exhibit Dr. Kerr's phenomenon to an audience,
the light must pass through a considerable thickness of the
liquid. The cells we employ are about 114 mm. long, 46 mm.
wide, and 100 mm. high. The ends are made of glass, so that
the light travels through about 100 mm. of stressed liquid.

The condensers are made of brass plates or tubes, held apart
by thin glass rods, which are bent so as not to cross that part of
the field of view in which the phenomenon is to be exhibited.
The ends of these are, when necessary, inserted in narrow brass
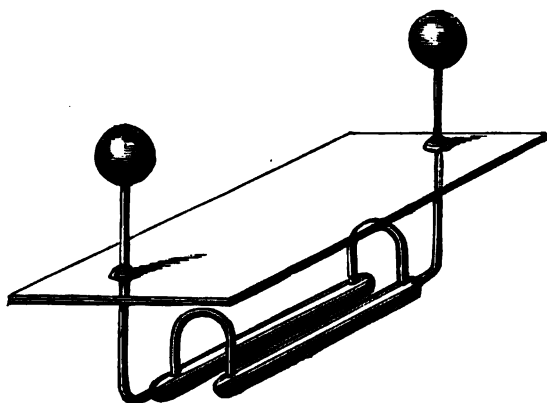
FIG. 1, a.

tubes which are soldered to the principal plates or tubes.
The wires which support the conductors should be attached to
them, and should penetrate the glass plate at points as distant as
possible from each other. The risk of a spark passing between
the electrodes is thus diminished.

Experiment I.—This is merely a repetition of Dr. Kerr's
original experiment, except that two cylinders are substituted
for spheres. The cylinders, which are about 100 mm. long, are
arranged with their axes parallel, so that the nearest points are
about 4 mm. apart. The principal sections of the polarising and
analysing Nicols are inclined at 45° to the vertical, and crossed so
as to give a dark field. When the cylinders are properly levelled

and the ends nearest to the screen are focussed, the image is similar to that of two balls. On connecting the two cylinders with the opposite terminals of an electrical machine, a bright patch of light appears upon the screen, which proves that the stress is most intense in those parts of the medium which are immediately between the two oppositely electrified bodies. Fig. 1 (*a*) is a perspective view of the cylinders, bent glass rods, conducting wires, and glass cover ready to be placed in the cell.

The image seen upon the screen when the Nicols are crossed and the cylinders are oppositely electrified is given in Fig. 1 (*b*). We shall not supplement descriptions of other experiments by perspective views of the apparatus, as the general arrangements will be readily understood from the description and from the single figure we have already given.
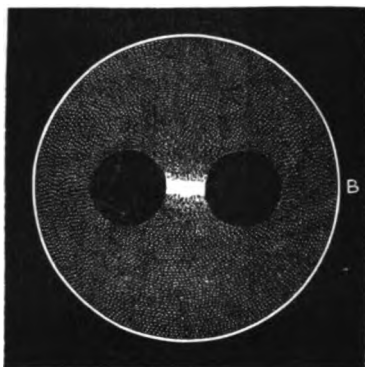
Fig. 1, *b*.

Experiment II.—In this arrangement the cylinders are connected outside the cell by a wire, and lie between two vertical brass plates which are also in conducting communication. The cylinders and plates being attached to opposite terminals of the machine respectively, a glow appears between each cylinder and the nearest plate, but the space between the cylinders themselves is dark. Taken in connection with the first experiment, this illustrates the fact that the tendency to separate exhibited by similarly electrified bodies may be regarded as due rather to an attraction in play between each of them and surrounding objects than to a mutual repulsive force.

Experiment III.—Two vertical brass plates, about 100 × 70 mm., are placed in the liquid, about 5 mm. apart, and oppositely electrified, thus constituting an arrangement electrically equivalent to Franklin's pane. The space between the plates is brightly illuminated (Fig. 2, *b*).

This experiment, simple as it is, is, perhaps, at present of more theoretical interest than any of the others. The double refraction in an electrically-stressed liquid may conceivably be produced in either of two different ways. The structure of the liquid may become crystalline in the sense that it is different in directions parallel and perpendicular to the lines of force. In this case double refraction would take place in a field subjected to a
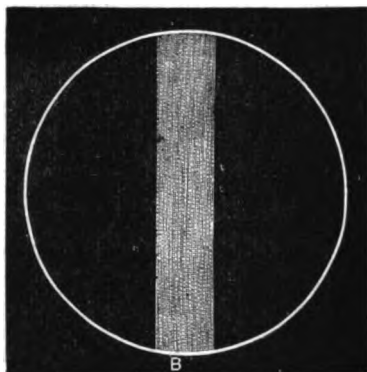
Fig. 2, *b.*

uniform stress. If, however, the effect of the electrical forces is merely to produce a change in the volume of the liquid, the double refraction would only be produced at points where the stress varied. Carbon bisulphide in a uniform field, like glass heated uniformly throughout, would be singly refracting. Several observers have tried and failed to obtain double refraction in glass subjected to a uniform electrical stress, and Röntgen and others have concluded that it is only produced when the stress varies from point to point.

We have therefore passed plane-polarised light between brass plates the lower parts of which are continuous, while in the upper part of each are two openings exactly opposite to those in

the other. If, then, the double refraction is due to the unequal stresses near the edges of the plates, the upper part of the field ought to be the more brilliantly illuminated. The light which traverses it passes near six edges, while that in the lower part of the field is affected by the neighbourhood of two only. As the lower part is far the brighter, the experiment strongly supports the view taken by Dr. Kerr—that the constitution of the stressed liquid is analogous to that of a crystal (*Phil. Mag.*, xiii., p. 262, 1882). Whatever, then, may be the cause of the difference in the behaviour of glass and liquids, we shall assume, with Dr. Kerr, that the optical properties along, and at right angles to, the lines of force are different in CS_2.
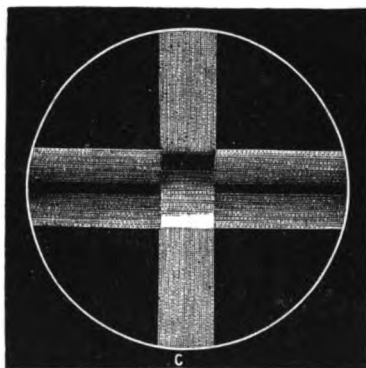
Fig. 2, c.

If a small bar of glass be held between crossed Nicols and bent in a vertical plane by pressure applied directly by the hands or by a screw, it becomes doubly refracting. A black line is seen near the middle of the bar, which corresponds to the layer of no distortion. Both above and below this the light increases gradually but rapidly. On one side the bar is compressed, and vibrations parallel to the median line travel faster than those perpendicular to it. On the other side it is stretched, and vibrations perpendicular to the line of no distortion travel fastest.

If the bar be held in front of the cell, with its length parallel to the lines of force, the black band appears broken, that part of it which is in front of the electrically-stressed liquid being shifted towards the side on which the bar is compressed. This proves

that in the liquid vibrations parallel to the lines of force are transmitted with a less velocity; so that the liquid behaves like a positive crystal the direction of the axis of which coincides with that of the lines of force. Fig. 2 shows—*b*, the restoration of the light when the plates are electrified; and *c*, the appearance presented when the bent glass is held in front of the cell. The band is shifted towards the compressed side of the glass.

Experiment IV.—The two plates of the condenser are bent twice at right angles, and the wire attached to the inner plate carries a knob (see Fig. 3, *a*). The whole thus represents a
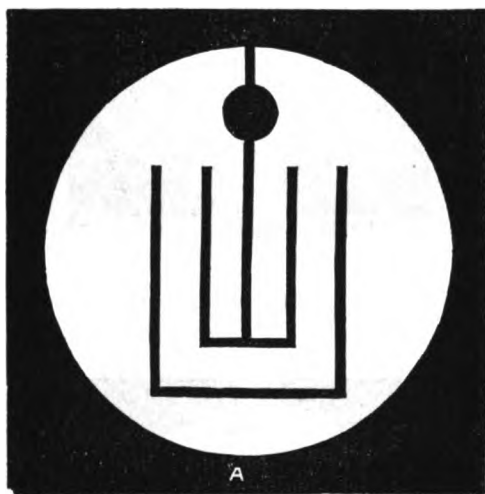
Fig. 3, *a*.

section through a Leyden jar. On electrification the space between the conductors becomes illuminated, except at the corners, where two black brushes are seen, the axes of which are parallel to the principal planes of the Nicol's prisms (Fig. 3, *b*). If the compensating glass be placed so that the line of no distortion is coincident with the image of the horizontal base of the inner conductor, the upper part of the bar being compressed, on electrification the black bands divide into three parts. One of these moves downwards through the horizontal illuminated space below the inner conductors, while the others rise between the vertical sides of the conductors (Fig. 3, *c*).

This serves to illustrate the fact that the velocities of propagation of vibrations parallel and perpendicular to the lines of force are different.
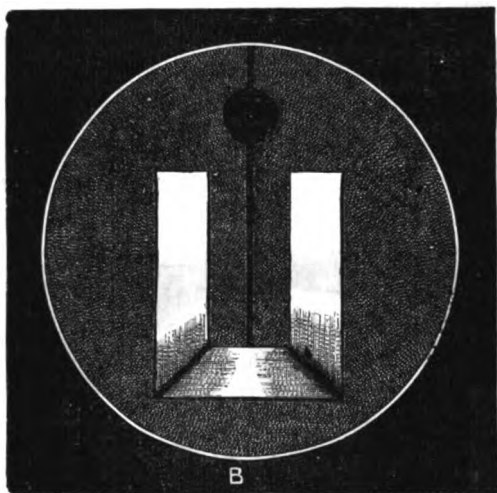
Fig. 3, b.

Experiment V.—The conductors are two concentric cylinders. The light passes through the intervening space parallel to their
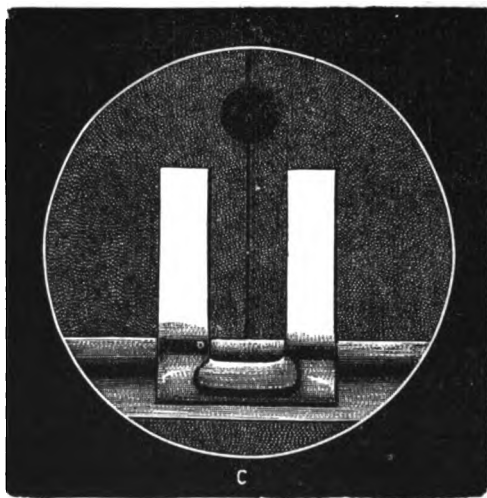
Fig. 3, c.

axes. A well-developed black cross appears, the arms of which are parallel to the principal planes of the Nicols (Fig. 4).

Experiment VI.—If the plane condensers in Experiment III. are replaced by two thicker brass plates the edges of which are rounded, and which are separated only by about 2 mm., the stress may be increased so that the liquid displays colours which may rise to the green of the second order. We have even succeeded in increasing the stress until the red of the second order appeared. A very beautiful experiment of Dr. Kerr's is thus repeated with very simple apparatus.

If the electric light is used, by simply interposing a prism between the analysing Nicol and the screen, a bright spectrum of the light which passes between the plates can be obtained. As the potential rises a black band is seen to enter at the violet end and traverse the whole length of the spectrum.*
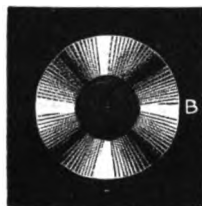
Fig. 4.

Experiment VII. is similar to the last, except that one of the plates is curved. The colours thus vary from the centre outwards, and present the appearance of a section through Newton's rings.

Experiment VIII.—Very beautiful effects may be obtained by repeating some of the above experiments with a plate of mica or selenite of suitable thickness interposed between the Nicols. This method has also the advantage that the field is never dark, and that the outlines of the apparatus can be seen before electrification.

In the case of Franklin's pane the colour displayed by the crystal between the conductors changes as the potential rises.

With the Leyden jar apparatus the difference of path of the

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* Note.—In a later experiment we have reached the red of the third order, by which time the second black band was vanishing and the third was entering upon the green.

interfering rays is altered in opposite ways in the vertical and horizontal sections. It is increased in one case, diminished in the other; hence a considerable difference of tint is produced. In like manner, when the experiment is tried with the concentric cylinders, the two pairs of opposite quadrants become differently coloured.

The PRESIDENT: The subject of the paper is hardly one for discussion in a general sense, but perhaps some members may wish to make observations.

Professor G. CAREY FOSTER, F.R.S.: I am very glad to have the opportunity of expressing what I am sure must be the feeling of all present—extreme admiration of the beautiful way in which these experiments have been shown to us, and of the unusual ingenuity in the arrangement of them, and skill in their performance. I do not feel that I have anything to say bearing generally on this subject beyond what Professor Rücker has said already. The experiments are not only, as he says, beautiful in themselves, but they are most instructive and important in their bearing on the constitution of dielectrics and on the general theory of electricity.

Professor W. GRYLLS ADAMS, F.R.S.: I am very glad, Sir, to add a word or two to what Professor Foster has stated. Some of us know by experience what very great difficulties there are in the way of presenting to an audience such a subject as the one brought before us this evening, and perhaps that point cannot be too strongly dwelt upon. The experiments we have seen to-night are in themselves exceedingly beautiful, but it is also exceedingly beautiful to be able to repeat them; and Professor Rücker and Mr. Boys have overcome that difficulty so well, that a stepping-stone, I may say, is given over a great deal of very difficult mathematics that perhaps would hardly be appreciated before this Society. We have here, in fact, the experimental stepping-stone whereby the phenomena are presented to an audience completely. If there were not the facility of representing these experiments on the screen, I am afraid that the audience would have to take it on the faith of the lecturer, or else

have to go through a great deal of mathematics to be convinced Professor
that the phenomena spoken of were true.   In this case there can Adams.
be no question at all ; and I think that for students the great
advantage of the experiments is, that not only are they pretty
in themselves, but they are most important from a scientific
point of view.

Mr. J. E. H. GORDON: As these experiments illustrate the Mr. Gordon.
theories of Clerk Maxwell, it will not be out of place if I remind
the Society of a passage in a very little-known book by Clerk
Maxwell.   It is a little elementary text book on electricity,
published by the *Christian Knowledge Society*, and it contains a
speculation of Clerk Maxwell's, in which he points out that we see
objects by our sense of sight, which gives us differences in per-
ception of the amount of illumination of different bodies.   He
says that all bodies are in a state of different potential and
different electrification, and he speculates on what we should see
if we had a sense that would enable us to detect differences of
electrical potential in the same way as our eyes detect differences
of optical potential, as we might call optical brightness.   I think
that Professor Rücker's experiments have gone very far towards
putting that sense at our disposal.   They enable us to actually
see the difference of potential in different bodies,—the possibility
of seeing which was predicted by Clerk Maxwell some 15 years
ago,—and I think that that makes these experiments of even
greater interest than they would be by themselves.   But beautiful
as they are, these experiments must yield in interest to the
conclusions which may be obtained from them, and to the
possibility of research given us into possibly the ultimate nature
of electricity.   Here are we, a great and active Society, dealing
with electricity—some of us selling it by measure, some generating
it, but none having more than the very faintest idea of what it is ;
but it is only by experiments of this nature that we have any
hope of ever coming to any such conclusion ; and it is very curious
to see how every experiment that is shown, every new research,
seems to confirm the ideas given to us by the physical insight of
Faraday, supplemented afterwards by the mathematical insight
and theory of Clerk Maxwell, with his beautiful conception that

Mr. Gordon. light itself is an electro-magnetic disturbance. He, as we may remember, pointed out that all the phenomena of electric induction could be explained by the supposition of an ether which may have certain properties which would transmit the waves of electro-magnetic induction. He then points out how, by experiments on this very specific inductive capacity, we could determine the velocity of electro-magnetic induction, not only in air and vacuo, but in a great many bodies ; and he pointed out that if it should be shown hereafter that those velocities were the same, not only in air and vacuum, but in other bodies, it would be a great confirmation of the theory that light itself is an electro-magnetic disturbance. I remember him saying verbally (and I do not think the statement is in his book) to me, when I had the honour of being his pupil, that it would be absurd to consider the whole of every part of space to be filled with two ethers which were exactly alike in their most important properties—exactly alike in every property that we could measure or discuss, and yet should be different and not the same. His theory was that the same ether transmitted the waves of electricity and the waves of light. Since then a great many experiments have been made. I made a certain number myself on various substances, and I think Dr. Hopkinson has made a great many, and I think we all found certain very curious resemblances, and certain discrepancies, of course, as we all know. The velocities in vacuo were taken from different considerations altogether, namely, from the ratio of electro-magnetic and electrostatic units, and they were found to be practically identically equal. But whenever we came to use substances like paraffin and other solid substances we had difficulties, and sometimes we would find a coincidence, sometimes a great discrepancy. I remember that Professors Ayrton and Perry published the first experiments on gases, and they found that where we might expect to be free from disturbing causes those velocities were almost identically equal throughout the whole long series of gases. I forget the number, but I think it was 10 or 12 gases.

Professor PERRY : Yes.

Mr. J. E. H. GORDON : And it is more likely that the evidence

of gases, where they are not likely to be influenced by disturbing causes, would be trustworthy than any evidence on the other side from solid bodies, in which a number of extraneous conditions interfere.

A kindly allusion was made by Professor Rücker to some experiments of mine on the residual discharge of a Leyden jar, which I showed at the Royal Institution. I should mention that, although shown by me, they were communicated to me by Dr. Hopkinson, and were really brought out by him a good many years ago.

Professor J. PERRY, F.R.S.: I wish to ask Professor Rücker a question, for the purpose of destroying a wrong impression which may have come into the minds of a few of the younger members, or perhaps he will correct me if I am wrong. In the first experiment this evening, a plate of paraffin was inserted in one of the air condensers, giving to it a greater capacity on account of the specific inductive capacity of paraffin being greater than that of air. This is clear enough; but in order to show that there was no moisture on the surface of the paraffin, a plate of tin was placed round it, and I think that Professor Rücker may have led us astray in saying—I do not know that he meant it—that in using the plate of tin the deflection was still about the same as with the uncovered paraffin; whereas, of course, as a matter of fact, the tin being put in makes the whole plate have an infinite specific inductive capacity. As the tin is larger than the paraffin, there ought to be a greater deflection than if the paraffin were completely wet all over, and a very much greater deflection than uncovered dry paraffin.

Professor W. E. AYRTON: I believe that Faraday tried for a very long while to show optically the stress in glass when it was electrified, but failed to do so.

I would ask Professor Rücker if he has made any calculations as to the sort of effect that would be obtained by the mere electrostatic pressure of the two electrified coatings of a glass Leyden jar. Why is it that so little effect is obtained with glass which is supposed to be under electrical strain? I understand from Dr. Lodge that, as a matter of fact, the sort of effect pro-

Professor
Ayrton.
duced on polarised light can be explained by the mere electrical
attraction of the two electrified coatings squeezing the glass; a
very little pressure on a piece of glass will produce quite as much
effect with polarised light as is produced with a very large P. D.
between the two coatings. If the experiments on glass only
existed, we should be inclined to think that the whole thing was
due to mere electrostatic pressure, and not due to an electrical
stress in the glass at all. I do not of course conclude that there
is no true electrical strain in the glass, but experiment with
polarised light has not proved its existence. Looking, however, to
the very beautiful effects we have had this evening performed with
liquids, we cannot apply the same reasoning and say that they are
produced by the electrostatic pressure of the charged coatings, as
of course a fluid would yield under any such pressure, so that at
any rate in liquids there is genuine electrical stress. I would
like to echo Dr. Hopkinson's question, and ask, Why is it, when
Faraday failed with glass—why is it, when polarised light is sent
through glass under electrical strain, you get scarcely any effect
at all ?

I believe the experiment about the metal jacket on the paraffin
wax was directed against myself personally, and therefore I am
led to remark that I still hold my judgment in suspense, for I am
inclined to think, from what I have seen this evening, that the
specific inductive capacity of the paraffin wax cannot with
certainty be accurately measured in the manner shown. I should
rather be inclined to say that it proved that there were layers of
moisture on the paraffin wax, because, as Professor Rücker
remarked, you do get about the same effect when the metallic
jacket is put on both sides of the paraffin wax as when there is no
metallic coating. The conclusion therefore would be, that there
is a slightly conducting coating of moisture on the surface of the
wax. The metallic coating is instantaneously discharged on
touching it, as it is a good conductor, but not so the moisture on
the wax, because it is only a poor conductor. It seems to me that
the effect may be produced, not because the wax has acted as a
dielectric, but because the wax is itself moist, and has acted as if
its surface were really quite dry and had been very lightly
sprinkled over with fine metallic powder.

We have all immensely enjoyed the lecture we have had this evening. It is one thing to know and to teach that the action of a Leyden jar is in the dielectric and not on the coatings—it is one thing to state this fact and to work out mathematically the experimental expressions for the residual charge, and it is quite another thing to have the phenomena presented to our eyes in that unmistakable and convincing manner in which they have been presented this evening, which reflects the greatest credit on the skill and ingenuity of Professor Rücker and Mr. Boys.

Professor SILVANUS P. THOMPSON: Having enjoyed greatly the experiments which have been exhibited upon the screen, and Professor Rücker's very lucid description of them, I cannot help thinking, how very fortunate we are in being Britons. For all those who have any acquaintance with Continental text-books on electricity know how the important part of the subject is carefully left out of them—that is to say, when they deal with electrostatic effects, nearly all the Continental text-books leave out everything about the medium, and tell only about the conductors that get the charges on their surfaces. When we are brought in this way face to face with the fact that it is the medium which is the one essential fact, we ought to congratulate ourselves on being Englishmen. Many, if not most, of our Continental friends are still under the shadow of the great mathematicians Coulomb, Laplace, and Poisson. They take the law of inverse squares for the basis of electrostatic action and calculate out everything on a supposition that the medium has nothing whatever to do with the action. Well, happily, Faraday delivered us from that, and now we have before the eye in these results the truth that the medium is the essential thing to think about, and not the mere mathematics of one thing acting on another at a distance.

While Mr. Gordon spoke about Clerk Maxwell's speculations as to what we should see if we had an electrical sight, my recollection has been travelling back to a book which I have no doubt is well known to some few, but I wish it were known to all students of electricity—a book from which I myself learned a good deal—a little book on electricity, by Mr. F. C. Webb—a

Professor Thompson. book that I believe has gone out of print, but which had some excellent diagrams in it, showing us what ought to happen in electric fields of various kinds. Now, that is the kind of thing from which I say I learned a great deal, and I believe it would be a very good book to have still.

Mr. J. E. H. GORDON : The book is " Webb's Treatise on the Principles of Electric Accumulation and Conduction."

Professor S. P. THOMPSON: If we could only have some of those diagrams in Webb's book made up in little models, and put into arrangements of this kind, we should be able to realise actually on the screen, in the way in which we have done the Leyden jar, Franklin's plane and other things. I may go a little further. Faraday made speculations, not only on what we should see if we had an electric sight, but on what we should see if we had a magnetic sight; in fact, he himself, in those splendid experiments of his on the effect of light when passed through his heavy glass, gave us the first information on what we should see if we had a magnetic sight. There is another old experiment very little known, but one that I am very fond of, which bears a little in this direction, I mean that beautiful experiment of Mr. Justice Grove, where he fills a tube with dilute mud made with precipitated magnetic oxide of iron. On magnetising the tube containing the mud, light came through that tube along the lines of magnetic force more freely than it did when the mud remained unmagnetised. That optical magnetic experiment certainly deserves to be better known than it is. Some seven or eight years ago, when I had occasion to think about this matter, I had just been seeing the late Professor Guthrie show some of these effects with glass and with Canada balsam under electric stress, and I tried to modify Grove's experiment to see if I could not get actual magnetic fields thrown upon the screen. I got a quantity of very fine iron filings, and I mixed them up along with a jelly made of gelatine, just softened, so that the iron filings would sink into it and remain fixed when the jelly was cold. I found that the jelly field of magnetic particles, when subjected to magnetic stress, also showed optically on the screen, and one could get light coming round in certain directions

through it, and I was in hopes of being able to elucidate some Professor Thompson. magnetic phenomenon in that way. I am sorry to say, however, that I did not carry the experiment very much further than making one batch of jelly, and trying it with the few magnets at my disposal; but I think a great deal more might be done in that direction.

Professor A. W. RÜCKER, in reply, said: I must thank the Professor Rücker. members for their very kind appreciation of the experiments. As regards the paraffin experiment, I may say, in answer to Professor Perry, that I referred not to the exact relative values of the two deflections, but only to the fact that they were both sufficiently large to throw the spot of light off the scale. The point on which I laid stress was, that whereas the deflection was unaltered when the paraffin was touched, it was reversed in direction when the metal cover was put to earth. The method does not admit of precise measurement.

To Professor Ayrton I reply that his argument involves the assumption of a change in the properties of the paraffin during the experiment. If it be true that the deflection when the uncovered paraffin is used is nearly the same as when it is enclosed in a metal case, and if this is because the surface of the paraffin is a conductor, then it must be a very good conductor. In both cases the effect is instantaneous, and the experiment can only be explained by supposing that electricity spreads over the surface of the paraffin almost as readily as over the metal case. It is therefore, on this hypothesis, impossible to account for the fact that the electrometer was not affected when I touched the paraffin, by assuming that it is a bad conductor. Its conductivity cannot be both high and low. If the effects of induction are immediate, it ought to be readily discharged when put to earth.

Mr. Boys and I have not repeated the experiments on which the statements as to the behaviour of glass under electrical stress are founded. The evidence is, however, sufficiently good to have given rise to the theory that the double refraction is due to the variation of the stress from point to point. In passing, I may remark that I am sure Professor Thompson would except the author of this theory—Professor Quincke—from the rather sweeping criticism which he passed upon foreign electricians.

Professor S. P. Thompson : Excuse me, I said foreign text-books.

Professor
Rücker.
Professor A. W. Rücker: Few have experimented on di-electrics with more success than Professor Quincke. Wiedemann, in his "Galvanismus," sums up on the whole in favour of the view that the double refraction is due to unequal expansion, and not to a crystalline constitution which can be produced by a uniform electrical stress. He admits that the experiment of Dr. Kerr's, which I have shown you, in which carbon bisulphide takes the place of glass in Franklin's plane, is opposed to this view unless the whole effect can be ascribed to the action of the edges, the possibility of which I have this evening disproved. It is conceivable that solids and liquids may behave differently, but if this is so, further proof is to be desired. The matter can hardly remain where it is, and I hope that before long further light will be thrown upon it.

Mr. Boys.
Mr. C. V. Boys: Might I offer one remark in answer to an objection raised by Professor Ayrton, who has, in fact, whether he knew it or not, offered a possible explanation why it is that glass does not show the effect of electrical stress when that stress is uniform, while liquids do show it? Since the suggestion, if it was one, may not have been clearly understood, I should like to take this opportunity of pointing it out. Professor Ayrton said that in the case of a solid it is a question whether the stress which is or may be observed is due to the electrostatic attraction of the plates on either side squeezing the solid, and thus setting up a mechanical stress the effect of which is observed by means of the polarised light. In the case of the liquid bisulphide of carbon, we had electrical stress set up, and observed it optically without any difficulty; but in the case of the solid glass, somehow or other it is not generally observed. It is a fact that in the case of some materials the stress along the lines of force is equivalent to a mechanical compression; in other materials the stress along the lines of force is equivalent to a mechanical extension. I cannot off-hand say which it is with glass. There is the possibility that, if the effect referred to by Professor Ayrton is really appreciable, and if also, as is possible,—I cannot say whether it is so or not, —the electric stress in glass is in such a direction as to be

equivalent to a compression and not to an extension, the result Mr. Boys.
of the combined action may be equivalent to nothing; and if
that is so, possibly the explanation of this discrepancy is, that
the electric and mechanical stresses which are set up at the
same time, and which necessarily increase and diminish together,
produce equal and opposite effects, and so nothing is seen upon
the screen.

The PRESIDENT: I am sure that this meeting will only be too The
President.
anxious to express its thanks to Professor Rücker and Mr. C. V.
Boys for the very valuable and interesting address to which we
have listened, and also for the exceedingly interesting experiments
with which it was illustrated.   It opens out a new field, and will
give us much more extended ideas than we possessed in regard to
the condition of dielectrics.

A hearty vote of thanks was accorded to Professor Rücker and
Mr. C. V. Boys for their paper.

The PRESIDENT: Mr. Johnston Stephen will now explain
Robertson's writing telegraph, which has been lately introduced
in America.

Mr. JOHNSTON STEPHEN: It was quite a surprise to me to-night
to be asked to say anything about this writing telegraph, because
it was only yesterday that I heard from Mr. Preece that he would
like to show the writing telegraph here and explain it to the
Society.   I came down here, brought the instrument with me,
and found to my consternation that Mr. Preece was not to be here
after all.   I am extremely sorry for it, for I am quite sure that
Mr. Preece could have explained it; and I expected that he would
have brought some diagrams with him, but we have none here,
and it is difficult to explain satisfactorily without them.   I fancy,
however, that there must be some of the members present who
have already heard a little of this writing telegraph, or of another
writing telegraph that is somewhat analogous.

I fancy that so long ago as the time when the telephone was
first introduced it must have occurred to a great many that a
writing telegraph then became possible.   We knew when the

telephone came—we might have known it before, but it was not so much brought before us—that the movement of a tympanum at one end of a line could give exactly similar motion to a tympanum at the other end. When the carbon microphone of Professor Hughes was introduced, we knew that pressure of the carbons would diminish the resistance to a great extent, and when the diminished resistance was brought about, of course a greater current passed. Mr. E. A. Cowper, in London, about 1879, introduced a writing telegraph, in the arrangement of which he had a double stylus which analysed, in fact, at the transmitting end, the complicated curves of hand-writing into simple right-and-left and up-and-down constituents: these constituents had values sent in variations of currents which more or less energised a pair of electro-magnets at the other end, these more or less attracting an armature, against the pull of an antagonistic spring by which the armature was held in its ordinary position. The instrument which I have to show you to-night resembles Mr. Cowper's instrument very much, I think, as the practical telephone of to-day resembles the telephone of Reiss. We use what Professor Bell called, and what has been so much talked about, an undulatory current. Mr. Robertson, the inventor of this instrument, an Edinburgh man, who for the last few years has resided in America, used a very pretty illustration in describing the difference that exists between his instrument and that of Mr. Cowper. He said that there was all the difference in the world; there was just the difference that existed between bumping down stairs from step to step in a sitting attitude and sliding comfortably down on the railing: in his own instrument it was pretty much like sliding up the railing as well—up and down with no break at all. This is proved by the fact that, if a telephone be placed in circuit, not the slightest sound is heard in it, and indeed this instrument, although it was not intended for the purpose, solves the problem of telephonic and telegraphic working simultaneously. We can work both instruments at the same time along the same wire; the currents employed for the writing telegraph, being so gradually varied, do not at all affect the receiver of the telephone, nor of course do the high potential currents in the slightest degree affect our slow-

moving armature. I think that my function will be better served
by being here to answer any questions that I possibly can, than
in actually trying to explain the instrument to you, quite
unprepared as I am, and without any diagrams. It is always
extremely unsatisfactory to speak about circuits and so on
without diagrams. I feel that I cannot do it; but Professor
Hughes and several gentlemen whom I see here to-night, know
quite as much as I do—perhaps a great deal more—about the
philosophy of this instrument, and I hope that the points will be
better illustrated by discussion and by my answering questions,
than by attempting to describe the instrument.

Professor D. E. HUGHES, F.R.S. : I have no questions to ask
relative to Mr. Robertson's instrument, having already put them
to him when I examined the instrument at the American
Exhibition last year. I could only then, as now, express my
greatest admiration of the most perfect manner in which the
instrument worked. My only regret is that this most beautiful
instrument did not come out before the telephone, for it would
then have had a great field for its use. There still, however,
remains a field for its usefulness, and there is not the slightest
doubt that the instrument will work very well, and I can only
express my great admiration for it.

The PRESIDENT : We will now close the business of the meeting,
and members will be enabled to examine the working of the
Robertson writing telegraph instrument.

The next Ordinary General Meeting will be held on April 12th,
and the next Students' meeting will be held on April 20th.

A ballot took place at which the following candidates were
elected :—

Associates.

| F. J. Beaumont. | L. J. Josephs. |
| T. H. Harrison. | C. T. Williams. |

Students.

| J. H. Fossett. | F. Taylor. |

The meeting then adjourned.

THE LIBRARY.

Atkinson [E.]   [*Vide* Mascart and Joubert.]

Bottone [S. R.]   Electrical Instrument-making for Amateurs.   12mo.   175 pp.
London, **1888**
[Presented by Messrs. Whittaker & Co., Publishers.]

Cloeren [M. H.]   Conditions d'Équilbre d'un Fil de Bronze phosphoreux tendu entre deux appuis.   8vo.   20 pp.   *Bruxelles*, **1888**

Fein [W. E.]   Elektrische Apparate, Maschinen und Einrichtungen.   La. 8vo.   392 pp.   *Stuttgart*, **1888**
[Presented by R. Howard Krause, Member.]

Institution of Civil Engineers.   Minutes of Proceedings.   Vol. XCI.   604 pp.   Plates.   *London*, **1888**

Mascart [E.] **and Joubert** [J.]   A Treatise on Electricity and Magnetism.   Vol. II.   Methods of Measurement and Applications.   Translated by E. Atkinson, Ph.D., F.C.S.   8vo.   792 pp.   *London*, **1888**
[Presented by Messrs. Thos. De La Rue & Co., Publishers.]

Planté [Gaston].   Phénomènes Électriques de l'Atmosphère.   16mo.   323 pp.
Paris, **1888**
[Presented by Messrs. J. B. Bailliere et Fils, Publishers.]

Spon [E. & F. N.]   Spons' Engineers' Price Book.   Giving Tables for Estimating Cost of Materials and Labour on Current Prices; and many useful formulæ for the use of Engineers and Shipbuilders, together with a list of Members of the various Engineering Societies throughout the United Kingdom.   8vo.   444 pp.   *London*, **1888**

Uppenborn [F.]   Lichtbogen und Bogenlicht.   8vo.   10 pp.   [Separatabdruck aus dem Centralblatt für Elektrotechnik].   *Munchen*, **1888**

Wyde [James].   Editor.   The Industries of World.   A Complete Course of Technical Education for the Manufacturer, Operative, and all Persons engaged or interested in Trade and Commence.   Vols. I. and II.   Portraits and Plates.   *London and New York.*
[Presented by C. H. W. Biggs, Esq., Member.]

Zeitschrift der Oesterreichischen Ingenieur-und-Architekten-Vereins.   Part of 1885 and whole of 1886-87.   4to.   *Wien*, **1885-87**
[Presented by R. Howard Krause, Member.]

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ABSTRACTS.

In the path of a ray of light from a slit are placed, in the order named, a polarising Nicol; a condenser, consisting of two horizontal brass plates, enclosed in a glass tube filled with bisulphide of carbon; an analysing Nicol; and lastly, a mirror revolving about a vertical axis. The two plates of the condenser are connected respectively to the two coatings of a Leyden jar. The Nicol having first been turned so as to cut off the polarised light, the plane of polarisation being at an angle of 45° with the horizontal, on charging the condenser the light reappears. The difference in phase which arises between the horizontal and vertical component of the ray is, as Kerr and Quincke have shown, proportional to the square of the potential difference of the two plates of the condenser. What will happen if the Leyden jar is discharged through a solenoid, so as to make the discharge vibratory? It is evident that, if there is no retardation of the double refraction over the electrical phenomenon there will be successive appearances of the light, separated by eclipses.

Experiment showed that the image of the slit when observed at the moment of discharge is composed of a series of luminous bands separated by dark bands. Hence the optical phenomenon occurs simultaneously with the electrical vibrations, and consequently the change in the dielectric which renders it doubly refractive occurs with immense rapidity. In the actual apparatus used the duration of a vibration was somewhere about one twenty-thousandth of a second.

In a second experiment the apparatus was so arranged that the polarised ray which had passed through the condenser, and another ray which had passed through the solenoid used for discharging the condenser, were intercepted by a rotating mirror in such a way that the image of one slit was a prolongation of the image of the other. If there is no retardation of the optical effect, the black bands of the image produced by the ray traversing the condenser ought to correspond with the most brilliant part of the luminous bands of the image produced by the ray traversing the discharging solenoid. This is in reality the case. Hence no retardation occurs; or if, owing to imperfections in the observations, it does exist, it cannot exceed one forty-thousandth of a second.

G. MANŒUVRIER and P. H. LEDEBOER.— USE OF ELECTRO-DYNAMOMETERS FOR THE MEASUREMENT OF THE MEAN INTENSITY OF AN ALTERNATING CURRENT.

Objections having been raised to the use of electro-dynamometers for measuring alternating currents owing to their self-induction, the authors have investigated both theoretically and experimentally the extent of the error introduced. Two types of electro-dynamometers were selected, the Siemens, in which the two coils are in series, and the Carpentier, in which they are in parallel. In the former the readings are proportional to the square of the current traversing both coils; in the latter, to the product of the two currents traversing the two coils independently. In both the mutual induction is eliminated and the self-induction becomes a constant quantity, since the two coils are always at right angles.

The simplest case of an alternating current is the Siemens machine, since the revolving coils have no iron cores, and its coefficient of self-induction may be considered constant. According to Mr. Joubert, the value of the variable current, i, at any moment, t, must satisfy the differential equation.

$$R i + L \frac{d i}{d t} = E_o \sin. 2 \pi \frac{t}{T},$$

where R is the resistance, L the coefficient of self-induction, E_o the maximum value of the E.M.F. of the whole circuit, and T the time of an entire period. From this equation it follows that

$$I_m = \frac{E_o}{R \sqrt{2}} \frac{1}{\sqrt{1 + \frac{4 \pi^2 L^2}{T^2 R^2}}}.$$

If now a Siemens electro-dynamometer of resistance r and self-induction l is introduced into the main circuit, the above equation becomes

$$I'_m = \frac{E_o}{(R + r) \sqrt{2}} \frac{1}{\sqrt{1 + \frac{4 \pi^2 (L + l)^2}{T^2 (R + r)^2}}}.$$

Comparing these two values of the mean current, we obtain the fraction

$$\frac{I'_m}{I_m} = \frac{R}{R + r} \sqrt{\frac{1 + \frac{4 \pi^2}{T^2} \cdot \frac{L^2}{R^2}}{1 + \frac{4 \pi^2}{T^2} \cdot \frac{(L + l)^2}{(R \times r)^2}}}$$

the second term of which expression shows the disturbing effect of the instrument.

In the case of the Carpentier electro-dynamometer, the corresponding equation is

$$\frac{I'_m}{I_m} = \frac{\sqrt{r r'}}{r + r'} \sqrt{\frac{1 + \frac{4 \pi^2}{T^2} \cdot \frac{l'}{r'} \cdot \frac{l''}{r''}}{1 + \frac{4 \pi^2}{T^2} \frac{(l' + l'')^2}{(r' + r'')^2}}}$$

where r', r'', l', l'' are the resistances and coefficients of self-induction of the two coils of the instrument.

The results of calculation and of experiment show that an electro-dynamo-meter, placed in the main circuit of an alternate current machine, has an effect on the mean intensity which depends on the periodic time, T, and increases when the latter diminishes. This disturbing effect is greater when the two coils of the instrument are parallel than when they are in series. But in both cases it does not exceed one per cent. of the mean current, at any rate for speeds up to 1,500 revolutions per minute. In the case of alternating currents of shorter periodicity, the mean value could always be measured with the same degree of approximation, provided that the coefficient of self-induction of the electro-dynamometer were diminished in proportion as the speed was increased.

P. H. LEDEBOER—THE CRITICAL TEMPERATURES OF IRON.

(*La Lumière Electrique*, Vol. 27, No. 1, January 7, 1888, *pp.* 3-8.)

In a brief review the author touches on the results previously obtained as a sort of preliminary to his own experiments, an account of which are given in the following abstract.

According to Mr. Becquerel, the magnetic properties of iron cease to exist at a dull red heat. Nickel and cobalt are subject to a similar phenomenon; and in the case of nickel, Mr. Berson has fixed the critical temperature at 336° C.

Mr. Pionchon has made a thorough investigation of the specific heat of iron at various temperatures, and gives the following values:—

From 0° to 660° S = $0.11012 + 0.00005066\ t + 0.000000164\ t^2$
,, 660° ,, 720° S = $0.57803 - 0.002972\ t + 0.000003585\ t^2$
,, 720° ,, 1000° S = 0.218
,, 1050° ,, 1200° S = 0.19887

Moreover, there is a change of state between 660° and 720°, with an absorption of 5.3 calories as latent heat; and a second change of state between 1,000° and 1,050°, with an absorption of 6 calories.

The thermo-electric properties of iron have been the subject of special investigation by Mr. Tait. The irregularities noticed in the behaviour of iron may be traced to the above-mentioned changes of state. The line representing the thermo-electric power of iron, as compared with lead, for example, is not a regular curve, but shows two sharp bends corresponding to the two critical temperatures. Nickel behaves in a similar way, but the critical temperatures are much lower, between 200° and 400°.

A curious phenomenon has been noticed in the expansion of iron, by Mr. Gore and Mr. Barrett. When an iron wire which has been heated to a bright red heat is allowed to cool gradually, it shortens; but at one point in the dull red it suddenly lengthens again momentarily; the reverse phenomenon, viz., a shortening on heating, may also be noticed by the aid of sufficiently delicate apparatus. If the experiment be performed in the dark, a momentary increase

of brightness is noted at the same instant; this is the so-called recalescence of iron.

The only observation on the resistance of iron wires at various temperatures is one by Mr. Macfarlane, who noticed a change at a dull red heat.

The effects of torsion have been minutely studied by Mr. Tomlinson, who concludes that there are two critical temperatures, one at a dull red heat, and the other at a higher temperature near the bright red. The iron loses its magnetic properties at the former.

The same two temperatures, i.e., about 700° and 1,000°, seem also, according to Mr. Moissan, to have very curious effects on the allotropic modification of the oxide of iron.

P. H. LEDEBOER—EFFECT OF TEMPERATURE ON THE MAGNETISATION OF IRON.

The fact that an iron bar heated to redness loses its magnetism has been long known. Rowland, in 1873, working up to a temperature of 230°, found no change; Poloni, in 1882, and MacRae, in 1885, arrived at the same negative result for temperatures up to 300°; Berson, in 1886, found that nickel loses its magnetic properties at 300°, but iron shows no change up to 340°. In all these cases the temperatures were too low.

The method adopted by the author was to measure the magnetic permeability of an iron bar, by determining the coefficient of self-induction of a solenoid surrounding it. In order to eliminate the effect due to the solenoid itself, an exactly similar solenoid was placed in the opposite branch of the Wheatstone bridge. The absolute value of the magnetic permeability could not be determined on account of the effect of the ends of the bar; for the same reason the remanent magnetism was not determined.

The bar was heated by means of a doubly-wound spiral of platinum wire surrounding it, through which a current of about 15 to 18 amperes could be sent. The solenoid was protected from the heated platinum spiral by means of a water jacket. A thermo-electric couple, consisting of a pure platinum wire, and one of platinum alloyed with 10 per cent. of rhodium, was placed next the iron bar, but separated from it by a sheet of mica, and served to measure the temperature approximately within about 10°. The thermo-electric couple was calibrated previously by means of observations on the boiling point of ammonium chloride (340°), the boiling point of selenium (665°), and the point of fusion of potassium sulphate (1015°).

The experiments were made on a bar of soft iron, with fields of the intensity of 35, 100, and 200 C.G.S. units. From the plotted results it is apparent that iron preserves its magnetic properties for all temperatures below 680°, but that after this point the loss is very rapid. At about 750° there is scarcely any trace left, and at 770° the magnetism has entirely disappeared. This rapid change, therefore, occurs through a range of temperature of from 80° to 100°. On cooling, the magnetic properties reappear

Professor H. F. WEBER.—TRANSMISSION OF POWER FROM KONIGSTETTEN TO SOLOTHURN.

Owing to the criticisms made on the previous experimental determination[*] of the efficiency of the machines used in this transmission of power, viz., that the experiments were carried out when both generators and motors were in the manufacturer's workshop and directly connected by an artificial line of iron wire of 10 ohms resistance, it was determined to renew the experiments on the machines in their actual working condition, and after they had been in daily use for several months.

The two generators and two motors are similar in construction, and each pair is coupled in series. The line is 7½ kilomètres long, and consists of a bare copper wire 6 mm. in diameter, carried on fluid insulators.

The observations were carried out by a committee of scientific men, and the instruments used were all those belonging to the electrical laboratory of the Polytechnic School at Zurich, and were not commercial instruments.

The two generators at Konigstetten were driven by a turbine, and the power required to drive them was calculated from the head of water and the opening of the guide blades; a preliminary series of experiments, in which the armature of one of the generators was removed and replaced by a pulley fitted with a brake, having given the exact horse-power given out by the turbine for various heads of water and various degrees of opening of the guide blades.

The current was measured both at Konigstetten and at Solothurn by means of two exactly similar tangent galvanometers. The potential difference was also measured on tangent galvanometers, previously calibrated by means of a standard Daniell cell. The power given out by the motors was measured by a friction brake.

The values obtained, detailed particulars of which are given in the paper, must be summarised in the following tables:—

(a) GENERATING STATION AT KONIGSTETTEN.

Date.	Power absorbed. Horse-power (735·4 watts)	Current Ampères.	Potential Difference.	Resistance of Machines.	E.M.F. $(V_1 + i_1 R_1)$
	A_1	i_1	V_1	R_1	E_1
11 Oct. ...	26·17	14·204	1177·7	3·797	1231·6
,,	24·56	13·245	1186 8	3·797	1237·1
12 Oct. ...	30·85	11·474	1753·3	7·251	1836·5
,,	30·85	9·785	2057·9	7·240	2128·7

[*] See Abstract, Vol. 17, p. 80.

(b) RECEIVING STATION AT SOLOTHURN.

Date.	Power given out. Horse-power (735·4 watts)	Current Ampères.	Potential Difference.	Resistance of Machines.	E.M.F. $(V_2 - i_2 R_2)$
	A_2	i_2	V_2	R_2	E_2
11 Oct. ...	17·85	14·177	1042·0	3·770	988·6
,,	16·74	13·286	1066·9	3·770	1016·8
12 Oct. ...	23·21	11·420	1655·9	7·060	1575·8
,,	23·05	9·785	1965·2	7·042	1896·3

On the first day only one machine was used at each station; on the second day, two machines at each end, connected in series. The resistance of the line was also carefully measured, and gave opportunities of controlling the calculated values of the loss in the conductors.

From the above figures the following efficiencies can be calculated :—

Date.	Generators.		Motors.		Efficiency of Transmission.
	Electrical Efficiency.	Commercial Efficiency.	Electrical Efficiency.	Commercial Efficiency.	
	$\dfrac{E_1 \times i_1}{A_1}$	$\dfrac{V_1 \times i_1}{A_1}$	$\dfrac{A_2}{E_2 \times i_2}$	$\dfrac{A_2}{V_2 \times i_2}$	$\dfrac{A_2}{A_1}$
11 Oct. ...	0·908	0·869	0·936	0·888	0·682
,, ...	0·907	0·871	0·911	0·868	0·682
12 Oct. ...	0·929	0·887	0 949	0·903	0·752
,, ...	0·918	0·888	0·913	0·881	0·747

In this particular instance, therefore, about 30 horse-power have been transmitted with two generators and two motors a distance of 7½ kilomètres, with an efficiency of 75 per cent. This very good result depends on the high efficiency of the dynamos themselves, the short distance and the low resistance of the line, the high E.M.F. employed, and the excellent insulation of the line.

E. COHN and L. ARONS—MEASUREMENT OF THE SPECIFIC INDUCTIVE CAPACITIES OF CONDUCTING LIQUIDS.

(Annalen der Physik und Chemie, Vol. 33, No. 1, 1888, pp. 13-31.)

There is one method of determining specific inductive capacities which depends on measurements of force, and is therefore distinct from those methods which deal with the charge of condensers. It is based on the principle first used by Silow for the purpose. The principle is the following :—
If a system of conductors, placed in any homogeneous medium, and each of

which is maintained at a given constant potential, undergoes an alteration of form, then the work given out by the electric forces is proportional to the specific inductive capacity of the dielectric medium.

In adapting this principle to measurements, two electrometers were used— one (M) of Mascart's construction, in which the dielectric was air; the other (F) of a special form suitable for containing the various fluid dielectrics experimented upon. The needle, one pair of quadrants, and the case of each electrometer were connected to earth and to one terminal of a Helmholtz induction coil, worked by an electro-magnetically-controlled tuning fork; the second pair of quadrants were connected to the other terminal of the induction coil. If, when F is filled with air, the deflections on the two electrometers were respectively F_a and M_a, and when F was filled with the fluid, the deflections were F_f and M_f respectively, then the specific inductive capacity,

$$\mu = \frac{F_f}{M_f} \Big/ \frac{F_a}{M_a}$$

Distilled water has a specific inductive capacity of $\mu = 76$, with a possible error of at most 5 per cent. Within the limits of accuracy of the measurements, this specific inductive capacity is independent of the amount of slight impurities contained in it, even when these impurities increase the conductivity five times.

The specific inductive capacity of ethyl alcohol (98 $°/_o$) is $\mu = 26.5$, with an error of at most 5 per cent. No effect is produced on the specific inductive capacity by the addition to the alcohol of salts, which increase its conductivity five times. It was very difficult to determine the value of μ for absolute alcohol; but it probably does not differ by more than 5 per cent. from the above value of 26.5.

Amyl alcohol gave a value $\mu = 15$; petroleum $\mu = 2.04$; xylol, two kinds, $\mu = 2.89$ and 2.86.

With respect to these results, it should be remarked that the values for amyl alcohol, ethyl alcohol and water, seem high; moreover, the law of Maxwell, connecting specific inductive capacity with refractive index, does not seem to hold good. On the other hand, the liquids experimented with have a much higher conductivity than those which have hitherto been submitted to actual experiment.

W. KOHLRAUSCH—CONNECTION BETWEEN MAGNETIC PERMEABILITY AND ELECTRIC CONDUCTIVITY.

It has been shown in the abstract of Ledeboer's paper that at a certain critical temperature iron appears to undergo a sudden molecular change, which influences its magnetic permeability; and the author has endeavoured to investigate if any corresponding change takes place in its conductivity.

The experimental iron bar was hung by two suspensions, one near either end, in a zinc box which could be filled with hydrogen or some other inactive gas, so as to prevent oxidation of the heated bar. The iron bar was heated by

passing through it the current of a dynamo machine, which was carefully measured and varied from 5 to 84 ampères. The resistance of the central portion of the bar was calculated from this current and from the potential difference of two points in it, the latter being measured by a high-resistance galvanometer joined to two platinum wires, which were securely fastened to the bar at a known distance apart. The magnetic permeability was determined by observing with a telescope and scale the amount by which the freely-suspended iron bar was attracted by an electro-magnet placed to one side of it and opposite the middle point.

These experiments show that the specific resistance of ordinary iron wire, cast steel, chemically pure electrolytic iron, and nickel at first increases slowly with a rise in temperature, then very much more rapidly than is the case with non-magnetic metals up to a point where the magnetic permeability suddenly ceases. At this point the curve of resistance bends at a sharp angle, and the resistance once more resumes its slow degree of increase for increase in temperature. It therefore appears that in the case of the above-mentioned metals there is some connection between the magnetic permeability and the electric conductivity.

For ordinary temperatures the specific resistance of cast steel, ordinary iron wire, and pure iron have the mean values 0·194, 0·149, 0·119; but at the temperature where the magnetic permeability ceases these values become 1·09, 1·07, 1·18.

L. DONATI—NEW FORM OF QUADRANT ELECTROMETER.

(Beiblätter, Vol. 11, No. 12, 1887, p. 823.)

The novelty consists in a new damping arrangement for the needle. To the stem carrying the needle is attached a rectangular plate or a cylinder of aluminium with a very small moment of inertia. The plate or cylinder swings between the poles of a powerful steel magnet; and the damping is the effect of the Foucault currents set up.

L. DONATI—NEW FORM OF ACCUMULATOR.

(Beiblätter, Vol. 11, No. 12, 1887, p. 833.)

A lead plate is built up of alternately smooth and corrugated strips of lead 3 to 4 cm. thick, with spaces between made by inserting small pieces of lead, the ends of the strips being soldered together. This plate is placed in a bath of nitrate of lead as anode, the kathode being a copper plate in a solution of sulphate of copper. The plate thus formed is then placed in dilute sulphuric acid together with plain lead plates. On charging, the E.M.F. at first rises very quickly, and then slowly to 2·5 volts; on discharging, the E.M.F. sinks rapidly from 2·5 volts to 2·2 volts, but afterwards falls slowly.

J. KLEMENCIC—MICA AS A DIELECTRIC.

(Beiblätter, Vol. 12, No. 1, 1888, pp. 57, 58.)

The following are the chief results of the experiments. The specific inductive capacity of mica plates obtained from Rafael, of Breslau, is 6·64. The capacity of a condenser formed of such plates is independent of the value of the potential used for charging the condenser. On altering the duration of the charge from 0·002 to 1,200 seconds, the discharge always being made after 0·007 second, the capacity altered by 1·8 per cent. Mica condensers have an exceptional insulation and specific resistance, which is apparently more than 6×10^{21} times greater than that of mercury. This is, however, only true so long as the mica is thoroughly dry; and it is possible that the discordant results obtained by various observers may be due to insufficient care in working with perfectly dry plates.

F. UPPENBORN—E.M.F. OF THE VOLTAIC ARC.

(Beiblätter, Vol. 12, No. 1, 1888, p. 83.)

The experiments were made with a hand-regulated lamp, working with a current of 7·7 ampères and with 10 mm. carbons. The values for a and b in the equation for the potential difference in terms of the length of arc L, viz., $P = a + b$ L, were found to be $a = 35·4$ to $45·4$; $b = 1·74$ to $3·2$.

Further experiments were made (a) with currents of 1·3 to 5·2 ampères, and with 7 mm. carbons at top and 5 mm. carbons at bottom; (b) with currents of 24 to 39·4 ampères and 30 mm. carbons; and it was found that a increased with the current density from about 25 to 45, while b diminished. The constant a may be replaced by the expression $s + y$ I, where s and y are new constants depending on the quality of the carbons, and I is the current. Since a decreases both for an increase in the current, as well as for an increase in the section of the arc, the effects are probably due to a surface resistance rather than to a counter electro-motive force.

Dr. F. VOGEL—CALCULATION OF LIGHTNING CONDUCTORS.

(Elektrotechnische Zeitschrift, Vol. 9, Jan., 1888, pp. 48, 49.)

In most text-books, when mention is made of the section of the conductor, it is assumed that, starting from a sufficient section of some metal, as iron, the section of any other metal can be calculated from the ratio of the specific resistances. This assumption, however, leaves out of account the specific heat of the various metals.

Suppose E coulombs to traverse a conductor with a resistance of R ohms for t seconds, the heat developed

$$Q = 0·14 \frac{E^2}{t} R \text{ gram-calories};$$

or substituting the geometrical dimensions of the conductor for its resistance

$$Q = 0·14 \frac{E^2}{t} \cdot \frac{s\, l}{q}$$

(where s is the specific resistance, l the length, and q the area of cross-section).

If S is the specific gravity of the metal, the weight of the conductor is
$P = s\,l\,q$ (length in metres, area in square millimètres).

If Σ is the specific heat, and θ the rise of temperature, the heat developed

$$Q = \Sigma\,P\,\theta = \Sigma\,s\,l\,q\,\theta.$$

Hence equating the two expressions for Q,

$$\Sigma\,s\,l\,q\,\theta = 0{\cdot}14\,\frac{E^2}{t}\cdot\frac{\theta\,l}{q};$$

$$\text{or } \theta = \frac{0{\cdot}14\,E^2\,\theta}{\Sigma\,s\,t\,q^2}.$$

Arago has stated that an iron rod 144 square millimètres in section will carry safely the heaviest possible lightning discharge. Starting with this value, and fixing θ for each metal somewhat below the temperature of fusion, the author obtains the following table of equivalent values for different metals :—

METAL.	Area. Sq. mm.	Weight per metre in grammes.	Diameter. Mm.	Cost per metre, in shillings.
Silver	83	869	10·2	118·00
Copper	72	642	9·6	0 61
Aluminium	112	291	12·0	23·00
Iron...	144	1,123	13·54	0·10
Platinum	128	2,726	12·8	24534·00
Brass	190	1,634	15·6	2·00

Dr. WIETLISBACH—THEORY OF TELEPHONE CONDUCTORS.

The chief factors influencing the propagation of the rapidly-altering undulations with which we have to do in telephonic currents, are the resistance of the line, its self-induction, its capacity, and the leakage to earth or to neighbouring wires, to which may be added the polarisation of the dielectric ; this latter becomes of importance where we have to deal with portions of a line which for any special reason may have to be placed underground.

The good quality of a telephonic transmission is determined by the strength and clearness of the sounds reproduced. By suitable construction of the apparatus it is always possible to insure sufficient strength. In order to transmit speech well, it is necessary that the several notes which go to make up one sound should be reproduced with great clearness, otherwise the sounds get mixed, and mistaken one for another. Now the four previously mentioned factors influence the clearness of the transmitted words, chiefly on account of their action on the phase of the undulations. Their influence may be summarised thus :—

1. The greater the resistance and the leakage, the smaller is the strength of the received current.

2. Self-induction favours high notes.

3. Capacity favours low notes.

4. The resistance diminishes the effect of self-induction and increases the effect of capacity.

5. Leakage diminishes the effect of capacity and increases the effect of self-induction.

6. In a conductor having both self-induction and capacity, the relative intensity of the undulations increases and decreases periodically with the rise in the height of the note.

7. The magnetic permeability and the polarisation of the conductor destroy the clearness of the tranmission.

It follows that the best possible conductor is one in which resistance, leakage, capacity, and self-induction are each as small as possible. A looped circuit, consisting of a copper wire three mm. in diameter, seems best to fulfil the several conditions. The resistance of such a wire is about five ohms per kilomètre; the self-induction is about 0.001; the capacity 0·01 microfarad; and by the use of suitable porcelains the insulation resistance may be brought up to 100 megohms.

The question naturally arises if it is not possible to counteract the defects of underground wires, the capacity and polarisation of which must be made greater than in the case of aërial lines. From the laws 2 and 3 it would appear that improved transmission would result from connecting up a line with much capacity to one with large self-induction. As, however, the capacity can only be altered within narrow limits, and the self-induction only admits of treatment by using iron instead of copper wire, while the leakage may vary, according to the state of the weather, from 10 megohms to 1,000 megohms, such a joining up of lines with different properties cannot be carried out practically. Nor can artificial lines be used with any better result. It is well known that the capacity of condensers and the self-induction of solenoids can compensate each other; but their application to telephone lines only makes matters worse than before.

A glance at the physical meaning of these two quantities will make this clear. The dimensions of self-induction are $[l^{-1}, s^2]$, those of capacity $[l]$, in electrostatic units; the product of the two is therefore thesquare of the unit of time. The action of self-induction is instantaneous, and the effects are produced simultaneously at every point of the conductor; consequently the current is the same in every point at any definite instant. The capacity, on the contrary, acts gradually on the current, so that it is less at the far end of the conductor than at the beginning; consequently in a conductor having appreciable capacity the current is different in every point at any definite instant.

Due attention seems now to be paid to the necessity of reducing the four main disturbing elements to a minimum in building long lines; but in the network of conductors in towns this is not the case. The resistance is often very great and might be reduced to one-third; the capacity of telephone cables is a maximum instead of a minimum; the leakage, which should be as equal as possible, varies from 1 to 1,000; the self-induction of the lines is often unnecessarily increased by the introduction of electro-magnets.

LIST OF OTHER ARTICLES
RELATING TO
ELECTRICITY AND MAGNETISM,
Appearing in some of the principal Technical Journals during the months of
FEBRUARY and MARCH.

(Philosophical Magazine, Vol. 25, No. 153, February, 1888.)

O. HEAVISIDE—Electro-magnetic Waves, and the Forced Vibrations of Electro-magnetic Systems.

(Vol. 25, No. 154, March, 1888.)

PROF. S. P. THOMPSON—The Price of the Factor of Safety in the Materials for Lightning Rods. **T. GRAY**—The Application of the Electrolysis of Copper to the Measurement of Electric Currents. **E. von AUBEL**—Experimental Study on the Influence of Magnetism and Temperature on the Electrical Resistance of Bismuth and its Alloys with Lead and Tin. **O. HEAVISIDE**—Electro-magnetic Waves.

(Journal Télégraphique, Vol. 12, No. 2, February, 1888.)

ROTHEN — Telephony *(continued.)* **F. EVRARD** — Observations on Lightning Strokes in Belgium.

(Vol. 12, No. 3, March, 1888.)

ROTHEN—Telephony *(continued.)* **ZETSCHE**—Duplex Telegraphy by the Method of Dividing the Two Bobbins of the Receiving Instrument. **F. EVRARD**—Observations on Lightning Strokes in Belgium.

(Journal de Physique, March, 1888.)

R. BLONDLOT—Electric Double Refraction. **GOUY**—Quadrant Electrometer.

(Bulletin de la Société Internationale des Electriciens, Vol. 5, No. 45, February, 1888.)

A. VERNES—Safety of Theatres Lighted by Electricity. **Anon.**—Regulations for placing Conductors for Electric Lighting in the Streets of Paris.

(Comptes Rendus, Vol. 106, No. 6, 6th February, 1888.)

G. ROBIN—Distribution of Electricity induced by Fixed Charges on a Closed Convex Surface.

(*No. 8, 20th February*, 1888.)

GOUY—Electrostatic Attraction of Electrodes in Water and in Weak Solutions.

(*No. 9, 27th February*, 1888.)

F. LUCAS—Electrical Determination of the Isodynamic Lines of any Polynome. **G. BERSON**—Experiments on the Changes produced by Blows in the Magnetism of a Steel Bar.

(*No. 10, 5th March*, 1888.)

F. LUCAS—Immediate Solution of Equations by means of Electricity. **E. BOUTY**—Conductivity of Strong Nitric Acid.

(*No. 11, 12th March*, 1888.)

P. JOUBIN—Measurement of Magnetic Fields by Diamagnetic Bodies. **P. DUHEM**—Magnetisation of Diamagnetic Bodies.

(*No. 12, 19th March*, 1888.)

E. DUTER—Passage of a Current through Sulphur. **ADER**—Submarine Telegraphy by the Phono-Signal. **L. OLIVIER**—The Radiograph.

(*La Lumière Electrique, Vol.* 27, *No.* 5, *4th February*, 1888.)

C. REIGNIER and P. BABY—Coefficient of Self-Induction. **E. WUN-SCHENDORFF**—Submarine Telegraphy (*continued*). **E. ZETSCHE**—Cherley's Translator for Permanent Currents. **A. PALAZ**—Photometric Standards. **E. MEYLAN**—Experiments on Vibratory Magnetic Call Apparatus. **J. KAREIS**—Popper's Alternate Current Apparatus for Electrical Measurements.

(*No. 6, 11th February*, 1888.)

G. MANOEUVRIER and P. H. LEDEBOER—Use of Electro-Dynamo-meters for the Measurement of Alternating Currents. **E. DIEUDONNÉ**—Some New Arc Lamps. **E. WUNSCHENDORFF**—Submarine Telegraphy (*continued*). **G. RICHARD**—Details of Dynamo Construction.

(*No. 7, 18th February*, 1888.)

G. RICHARD—Steam Engine Indicator. **E. WUNSCHENDORFF**—Submarine Telegraphy (*continued*). **C. E. GUILLAUME**—Calculation of the Resistance of a Mercury Column. **J. WETZLER**—Use of Carbon as Negative Electrode in Batteries. **J. WETZLER**—Professor E. Thomson's Apparatus for Studying Undulatory Currents.

(*No. 8, 25th February*, 1888.)

G. PLANTÉ—Electricity one of the Causes of Earthquakes. **P. H. LEDEBOER**—Primary Batteries for Electric Lighting. **E. WUNS-CHENDORFF**—Submarine Telegraphy (*continued*). **E. MEYLAN**—Lahmeyer's System of Regulating Difference of Potential at Distant Points of a Network. **A. PALAZ**—Recent Experiments on Jullien and Brush Accumulators.

(No. 9, 3rd March, 1888.)

E. ZETSCHE—Efficiency of the Hughes Apparatus compared with other Systems. A. PALAZ—Photometric Standards *(continued)*. C. REIGNIER and P. BARY—Theory of Coefficients of Induction. E. MEYLAN—New Transmission Dynamometers.

(No. 10, 10th March, 1888.)

J. MOUTIER—E.M.F. of Thermo-electric Couples. A. HILLAIRET—Loss of Charge in Parallel Distribution. A. PALAZ—Photometric Standards *(continued)*. E. DIEUDONNÉ—Portable Electric Light Plants. C. DECHARME—Effect of Chemical Actions on Magnetism.

(No. 11, 17th March, 1888.)

A. PALAZ—The Voltaic Arc and Arc Lamps. A. D'ARSONVAL—Thermo-electric Arrangement for Studying Animal Thermogenesis. E. ZETSCHE—Efficiency of the Hughes Printing Telegraph as compared with other Systems. HENRIQUE—Ratio between Candle-power and size of Filaments of Glow Lamps. C. DECHARME—Effect of Chemical Action on Magnetism. E. MEYLAN—Recent Experiments on Transformers by G. Ferraris. E. DIEUDONNÉ—Application of Electricity to Organs. J. KAREIS—Measurements for Determining the E.M.F. of Various Batteries. J. WETZLER—The Use of Dynamos in the United States for Working Telegraph Lines.

(No. 12, 24th March, 1888.)

G. RICHARD — Horse-power Dynamometers. L. OLIVIER — The Radiograph. E. ZETSCHE—Efficiency of the Hughes Printing Telegraph as compared with other Systems *(continued)*. A. D'ARSONVAL—Direct Reading Galvanometer. P. MARCILLAC—Analysing Seismograph. E. MEYLAN — Menges' Thermo-magnetic Machines. J. WETZLER—Paillard's Non-magnetic Watches.

(No. 13, 31st March, 1888.)

P. H. LEDEBOER—The Practical Measurement of Coefficients of Self-Induction. C. REIGNIER—Dynamometric Brake. E. ZETSCHE—Efficiency of the Hughes Printing Telegraph as compared with other Systems *(continued)*. A. PALAZ—Photometric Standards *(continued)*. MICHAELIS—Bœrnstein's New Electric Meter.

(Annalen der Physik und Chemie, Vol. 33, No. 3, 1888.)

K. R. KOCH — Electric Discharge from a Glowing Electric Body. F. STREINTZ—Experimental Researches on Galvanic Polarisation. A. v. ETTINGSHAUSEN and W. NERNST—Thermic and Galvanic Behaviour of some Alloys of Bismuth and Tin in a Magnetic Field. A. FOEPPL—Conductivity of Vacuum.

(*Vol.* 33, *No.* 4, 1888.)

B. LECHER—Opposed Electro-motive Force in Galvanic Light Phenomena. **S. ARRHENIUS**—Conductivity of Illuminated Air.   **F. NARR**—Behaviour of Electricity in Gases.   **A. GOCKEL**—Note on the Peltier Effect.

(*Beiblätter, Vol.* 12, *No.* 2, 1888.)

W. OSTWALD—Researches on Contact Electricity.   **G. JÄGER**—Conductivity of Solutions of Neutral Salts.   **G. JÄGER**—Calculation of Size of Molecules derived from above.   **S. ARRHENIUS**—Effect of Light on the Conductivity of the Haloid Salts of Silver.   **A. BATTELLI**—Thomson's Phenomenon in Nickel.   **F. BRAUN**—Convenient and Simple Mirror Galvanometer.   **KÜLP**—Magnetic Coercive Force.   **A. RIGHI**—Magnetic Circular Polarisation.   **E. VILLARI**—Difference in Resistance of Metallic Circuits traversed by Currents from Condensers and from Batteries.

(*Vol.* 12, *No.* 3, 1888.)

P. WALDEN—Calculation of the Size of Molecules of Salts from the Conductivity of their Solutions.   **S. PAGLIANI**—A Phenomenon of Crystallisation of Salts on Electrolysis of their Solutions.   **W. C. L. van SCHAIK**—Maxwell's Formula for the Electro-magnetic Dispersion of the Plane of Polarisation.   **E. DORN**—Some Apparatus for the Study of Atmospheric Electricity.

(*Elektrotechnische Zeitschrift, Vol.* 9, *Pt.* 3, *February*, 1888.)

O. FRÖLICH—Damping of Measuring Instruments.   *Anon.*—Line Tools and Line Erection.

(*Pt.* 4, *February*, 1888.)

LAHMEYER—New Types of Dynamo Machines.   **E. LIEBENTHAL**—Photometric Experiments with v. Hefner Alteneck's Amyl Acetate Standard Lamp.   **R. PETSCH**—Construction of Telephone Lines.   **MÜLLER**—Cold Jointing of Phosphor Bronze Wires.   **J. MATTHIAS**—Localisation of Faults.

(*Pt.* 5, *March*, 1888.)

v. HEFNER ALTENECK—Reply to Lahmeyer.   **O. CANTER**—Measurement of Battery Resistance.   **W. KOHLRAUSCH**—Calculation of Lightning Rods.   **R. PETSCH**—Construction of Telephone Lines (*continued*).

(*Pt.* 6, *March*, 1888.)

O. FRÖLICH — Peculiarities of the Universal Wheatstone Bridge.   **W. GROSSE**—New Compensation Photometer.   **H. SESEMANN**—Use of Telephones for the Detection of Faults in Cables.

The One Hundred and Seventy-seventh Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, April 12th, 1888—Mr. EDWARD GRAVES, President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to be suspended.

Donations to the Library were announced as having been received since the last meeting from Messrs. J. B. Baillière et Fils ; Messrs. De La Rue & Co.; C. H. W. Biggs, Member ; and R. H. Krause, Member ; to whom the thanks of the meeting were heartily accorded.

The following paper was then read :—

CENTRAL STATION LIGHTING :

TRANSFORMERS V. ACCUMULATORS.

By R. E. CROMPTON, Member.

The present paper is the outcome of the discussion which took place on Messrs. Kapp's and Mackenzie's papers on transformers, recently read before this Society. I was asked to give facts and figures in support of the statement I then made, that I believed the distribution of electricity by transformers offered no special advantages over other methods, particularly over distribution by means of accumulators used as transformers.

However, after compiling the statement of facts and figures necessary for my purpose, I found that I had undertaken a task of a much heavier nature than I originally intended ; and I must apologise for the present paper having a much wider scope than its title would warrant.

In fact, I find it necessary to commence by discussing some of the conditions of the supply of electricity from electric stations which are common to all systems of distribution, and about which little or nothing has hitherto been written or said.   In England central station lighting has been such an affair of yesterday that it is hardly to be wondered that what little knowledge exists on the subject should be confined to the few who have lighting stations under their immediate charge ; and when we look to the Continent, or to America, to supplement this meagre stock of knowledge, we find the conditions of demand for light differ from those of a large English town, such as London, so that they do not much help us in our investigation.   However, during the past few years I have done my best to make myself acquainted with the main facts which appear likely to govern the future commercial success of such undertakings.

I must at once divide the schemes for electric supply into two classes : 1st, those confessedly carried out in a temporary manner, and for the purpose of popularising the electric light, and inspiring the confidence of the public in supply companies as a form of investment ; and 2ndly, those intended to be of as permanent a nature as the gas supply, with which they come into competition. I think to the first class we must relegate such systems of supply as those at Brighton, Eastbourne, the present supply from the Grosvenor Gallery, in fact all those which distribute by means of overhead wires, picking their customers here and there, and which, although they have served a very useful turn in familiarising the public with the use of the electric light, cannot be considered as in any sense fitted to develop themselves into a complete system of supply when the time arrives that more than half the houses may be expected to take the light.

In order to get an adequate idea of the nature of the demand, or what I call the load diagram of a system of supply, we cannot

do better than refer to the diagrams of a similar nature taken by the gas companies. Having recently had the advantage of carrying out electric supply stations for several gas companies, I have had access to such load diagrams for gas lighting, and from these and from the experience already obtained of electric supply in London, I have constructed a load diagram which I believe will be found a fairly representative one for an ordinary London residential district, consisting partly of private houses, partly of shops, with the usual sprinkling of hotels and public buildings.
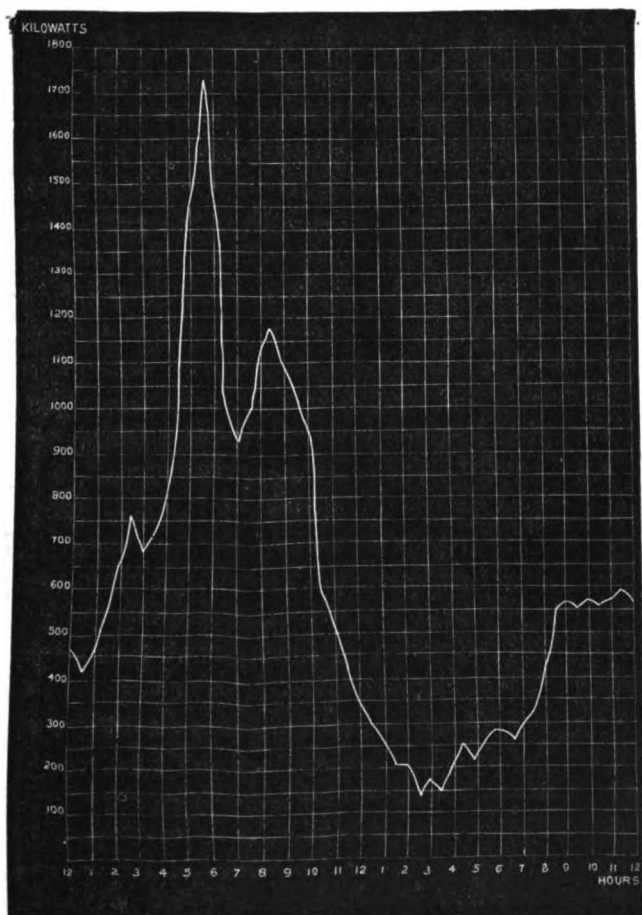
Boston.

No doubt a district containing a very large number of theatres would have a somewhat different diagram, but not differing so much as to materially affect the arguments that I am about to found on these load diagrams. Through the kindness of Dr. Fleming, I am able to lay before you several load diagrams obtained from lighting stations in America, which you see coincide very fairly with the one I have prepared for London. We learn
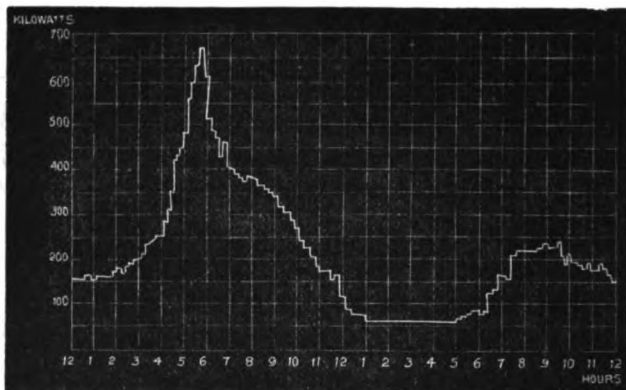
CINCINNATI.

from these load diagrams—1st, that the average daily outputs in units throughout the year is about three and a half times the maximum load in kilowatts; in other words, that a station having
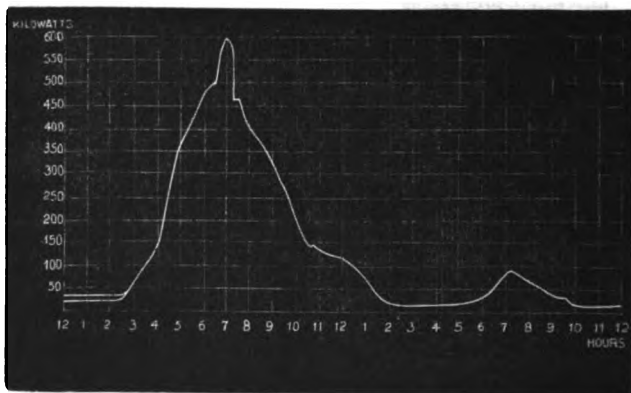
LONDON.

a plant equal to the supply of 600 kilowatts—that is to say,
10,000 lamps, of 16 candle-power, each simultaneously alight—
could only calculate on selling an average quantity of 2,100 units
per day.   This fact, which has such an important bearing on the
possible profits to be obtained from a station of a given size,
appears to have been completely ignored by the writer of an
article on central stations in *Industries*, dated the 2nd Decem-
ber, 1887, who estimates that a plant of this size would be able to
supply daily 3,600 Board of Trade units, which is 73 per cent. in
excess of the figure I have given above.   The next point we have
to note, common to all systems of distribution, is the extent of
underground mains necessary to distribute a given supply of
electricity, and the probable cost of such mains when laid under-
neath the footways of our streets.   Starting once more on the
above quoted 10,000-light plant basis, I find, from careful note of
the amount of electricity used by various classes of houses, that
in order to sell an average of 2,100 units per day our distributing
mains must reach not less than 1,000 houses.   We may take the
average width of the London house at 10 yards; to this we must
add 50 per cent., on the supposition that only two houses out of
three will take the light for some time to come ; and on to this
we must again add 30 per cent. for such proportions of the roads,
such as crossings, dead walls, &c., traversed by the mains which
do not front houses taking the light.   We thus find that every
house requires 20 yards of distributing main ; or to distribute the
daily average supply of 2,100 units, 20,000 yards of main.   In
addition to these distributing mains we must contemplate the
probability of the generating station being placed at a distance of
2,000 yards from the centre of the district supplied, so that in all
we have to provide 22,000 yards of mains of the two classes.
For convenience of nomenclature I call those mains which form
the primary wire to the transformers, or carry the charging current
to the accumulators, " charging mains ; " and those from the trans-
formers to the houses, or from the accumulators to the houses,
" distributing mains."   Next, as to the cost of these mains.
I have prepared two tables, which show how wide of the mark is
the popular idea that the cost of the mains is greatly influenced

by the cost of the copper conductors themselves.   I have stated before this Society that much nonsense has been talked on the subject of the cost of copper; by this I did not intend to be disrespectful to any particular speaker, but to point out in strong language that the habit of basing estimates of distribution on the cost of copper (with but a small percentage added for insulation and laying) was one to be avoided, as it has led to erroneous conclusions, on which have been based arguments far too favourable to systems using currents of high E.M.F.

No doubt this habit of considering the cost of copper as such an important element in the calculation has arisen from the fact that, although this question of electrical distribution has been for years under discussion, and has had volumes written about it, yet our experience in laying and repairing such conductors has been until quite recently almost nil.   The tables now before you are based on the little experience that I have gained during the last three years, and I trust they may be of some service to the members of the Society.   Many of the figures are the actual prices paid out on executed contracts, and the other ones are calculated from them; so that it is quite certain that the figures quoted in the table are not far wide of the mark.   You will notice that, apart from the price of the copper itself, the additions we have had to make for the insulation, troughing, cost of laying, providing and fixing surface boxes, digging and guarding the trenches, taking up and replacing the flagging and wood pavements, are almost a constant quantity depending only on the length of the roadway that is disturbed, and, consequently, when considered in relation to the cost of copper itself, are at a maximum when the section of the copper is small and decrease rapidly as the section augments.

Table No. 1 is for laying copper in the form of insulated cables in the old and well-tried way.   Table No 2 is for a form of main which I have successfully adopted during the past year, and which consists of a bare copper conductor, supported and insulated in concrete subways or culverts by porcelain insulators.   I am not at present submitting the latter system for your criticism.   In some respects it is not yet perfect; but I mention it as I am employing it to a considerable extent myself, and believe that it will prove a very valuable form of conductor for whatever system of

distribution is used. The completed main consists of such very durable materials, and the facilities for repairs and renewals, or for increase in the section of the conductor, are so great, that the annual charge for the upkeep and depreciation of these mains is not likely to exceed 25 per cent. of that of the older form of cables insulated with the continuous covering, whatever material may be employed for that covering.

With these considerations of the load diagrams, the extent of distributing system, and the cost of the mains before us, we can now proceed to compare the merits of distributing the electricity by two systems—the first being by alternating currents of high E.M.F., transformed to currents of low E.M.F., suitable to the glow and arc lamps used by the consumers, and which I hereafter call the A.T. system ; and second, the supply by means of direct currents of medium E.M.F. used to charge accumulators arranged in series, the current being taken off the accumulators at the lower E.M.F. required for the lamps, and which I hereafter call the B.T. system.

All modern systems of distribution aim at the reduction of the first cost of the distributing plant—that is, of the charging and distributing mains, and all appliances belonging to them, such as transformers, regulating devices, street surface boxes, house connections, &c., providing that such reductions shall not be at the expense of the efficiency of the system. It is difficult to fix an exact percentage of the loss allowable in the distributing plant, as so much depends on the length of the working hours, but I take it for granted that in no case must this loss exceed 10 per cent. I further take it for granted that the extreme variation allowable in the E.M.F. in the distributing mains must not exceed 4 per cent. So much has recently been written on the A.T. system that I will not lengthen this paper by describing it further than by stating that for purposes of my comparison I take the system as generally described by Mr. Kapp in his paper on transformers ; or by Mr. Mackenzie, in his paper on transformers ; and in an article in *Industries,* dated December 2nd, 1887, for which I believe Mr. Kapp is responsible. and which gives very complete data on the first cost of an installation for 10,000 60-watt lamps, burnt simul. taneously. On the other hand, it is necessary for me here to briefly explain the B.T. system, which I propose to compare with it.

Table No. 1.

COST OF LAYING 100 YARDS OF DOUBLE CONDUCTOR UNDERNEATH THE FOOTWAY OF A LONDON STREET.

	Single. No. 16.						Two Sets. 1⅜	Four Sets. 1⅜	Six Sets. 1⅜
Area, square inch ...	·0082	·0225	·0778	·1618	0·25	0·5	1·0	2·0	3·0
Area, square millimètre ...	2·06	14·6	50	104	161·25	322	645	1,290	1,935
Weight per 100 yards run ... lbs.	7½	53¾	184¾	392	576	1,158	2,806	4,612	6,918
Cost of Copper at 7½d. ...	£0 4 10	1 14 6	5 18 0	12 13 0	18 15 0	37 5 0	74 10 0	149 0 0	224 0 0
Cost of Insulation ...	1 3 2	4 8 6	11 2 0	24 17 0	35 17 0	70 15 0	141 10 0	288 0 0	424 0 0
Total cost of Cables ...	1 8 0	6 3 0	17 0 0	37 10 0	54 12 0	108 0 0	216 0 0	432 0 0	648 0 0
Casing, Bitumen, and Cement ...	5 3 0	5 5 0	8 0 0	12 10 0	12 10 0	16 0 0	22 0 0	40 0 0	55 0 0
Labour Laying ...	8 0 0	4 0 0	5 0 0	5 0 0	6 0 0	10 0 0	18 0 0	35 0 0	50 0 0
Trenching and Repairing ...	25 0 0	25 0 0	25 0 0	25 0 0	25 0 0	25 0 0	25 0 0	30 0 0	35 0 0
Surface Boxes and Connection ...	5 0 0	7 0 0	10 0 0	10 0 0	10 0 0	10 0 0	10 0 0	10 0 0	10 0 0
Engineer and Superintendent ...	3 0 0	4 0 0	5 0 0	5 0 0	6 0 0	10 0 0	10 0 0	20 0 0	25 0 0
Total... £	42 11 0	51 8 0	70 0 0	95 0 0	114 2 0	179 0 0	301 0 0	567 0 0	823 0 0
Add extra if Copper, at 9¾d...	0 1 1	0 8 0	1 7 0	2 17 0	3 5 0	8 10 0	17 0 0	34 0 0	51 0 0
Total, laid complete	42 12 1	51 16 0	71 7 0	97 17 0	117 7 0	187 10 0	318 0 0	601 0 0	874 0 0
Cost of Copper per lb., laid complete	5 18 6	0 19 4	0 7 9	0 5 0	0 4 1	0 3 8¾	0 2 8¾	0 2 7¼	0 2 6¼
Current in ampères ...	1·2	8·1	28	58	90	180	360	720	1,080
Cost per ampère ...	35 10 0	6 8 0	2 10 6	1 13 9	1 6 0	1 1 0	0 17 6	0 16 8	0 16 1

Table No. 2.

COST OF LAYING 100 YARDS OF DOUBLE CONDUCTOR OF BARE COPPER CARRIED ON INSULATORS IN A CULVERT.

	0·25	0·5	1·0	2·0	2·55	3·00
Area in square inches	0·25	0·5	1·0	2·0	2·55	3·00
Area in square millimètres	161·25	322·5	645	1290	1645	1985
Weight of Copper in lbs. per 100 yards	576	1158	2306	4612	6125	6918
Cost of Copper at 7¾d. per lb. £	18 15 0	37 5 0	74 10 0	149 0 0	190 0 0	224 0 0
Laying	9 0 0	9 12 0	9 12 0	9 15 0	9 15 0	10 0 0
Insulators	0 4 6	0 4 6	0 4 6	0 4 6	0 4 6	0 4 6
6 Surface Boxes and Connections	10 0 0	10 0 0	10 0 0	10 0 0	10 0 0	10 0 0
Culvert, 18 inches × 12 inches, for 2 lines Conductor, in Brickwork and Cement, Replacing Pavement	53 8 0	53 8 0	53 8 0	53 8 0	53 8 0	53 8 0
Engineers and Superintendence	6 0 0	10 0 0	10 0 0	10 0 0	10 0 0	15 0 0
Total £	97 7 6	120 9 6	157 14 6	282 7 6	263 7 6	312 12 6
Extra for Copper at 9¾d. per lb.	8 5 0	8 10 0	17 0 0	34 0 0	43 10 0	51 0 0
Total £	100 12 6	128 19 6	174 14 6	266 7 6	306 7 6	363 12 6
Cost of Copper per lb. laid complete	42d.	27d.	18·2d.	13·8d.	12d.	12·6d.
Current in ampères	90	180	360	720	910	1,080
Cost per ampère	1 2 3	0 14 5	0 9 8	0 7 5	0 6 9	0 6 8¾

At Vienna the arrangements proposed by Mr. Monnier, and successfully carried out by myself and others, consist of charging from a central station groups of batteries of accumulators arranged in series.   Each group of accumulators consists of 52 cells, giving the 100 volts required for the lamps, with a comfortable margin. The total E.M.F. required to charge the four groups of batteries in series varies from 430 volts, at the time the batteries are giving off work, to 480 volts, for the short time during which the charge is being completed. This includes the loss in the mains, the generating stations being situated 1,400 yards from the distributing station. During five hours of lighting about two-thirds of the current comes direct from the dynamos; but during this time for short periods the demand of current often increases to such an extent that these proportions may be reversed, and the batteries supply two-thirds of the total.   As this system has now been in use for upwards of nine months without any hitch or trouble whatever in connection with the electrical work, we may consider it proved there is no danger or difficulty in using currents of such high E.M.F. as 480 volts connected permanently to the distributing mains.   The system may, of course, be united with three or two groups only in series.   An example of the last is the central station belonging to the Le Mans Gas Company.

I now propose to commence my comparison of the two systems from four points of view.

1st,—As to trustworthiness, steadiness, and quality of the lighting;

2nd,—As to first cost of the plant; and

3rd,—As to working cost and maintenance;

4th,—As to the probable output, and hence maximum income, that can be safely obtained from the sale of electricity in each case.

I think it will be allowed that the commercial success of an electric light undertaking depends on these four points. I do not propose to dwell much on the first point. It is generally admitted that a system which uses storage must be superior to a system in which the continuity of the lighting depends entirely on moving machinery.   There are many reasons why I should not

make invidious comparisons on these points between existing systems. No one is prepared to dispute the facts that the A.T. systems hitherto installed have been remarkable for frequent interruptions in the lighting, whereas the reverse has been the case with the B.T. system. It remains for us to consider whether the undoubted great advantages which the B.T. system possesses in this respect is not purchased too dearly; and this matter will occupy the remainder of my paper. The comparison I now make is for a system of electrical supply for London for 10,000 60-watt lamps, or their equivalent, simultaneously alight at hours of maximum demand.

> 1st,—By alternating currents reduced from 2,000 volts to 60 volts by transformers.
>
> 2nd,—By direct currents reduced from 440 volts to 110 volts by accumulators used as transformers.

Cost of Generating Station.—For the A.T. system I adopt the figures given by the writer in *Iudustries*, on page 600, of the 2nd December, 1887. He therein proposes to provide for the 10,000-lamp plant dynamo power including reserve equal to 865 kilowatts, and he fixes the price of those as £5,540. As regards motive power, I do not propose to take advantage of the writer's far too liberal estimate of its cost at £12 per I.H.P., but take it at the lower figure of £8 12s. per I.H.P., thus reducing the cost to £12,500. I have estimated the cost of the building, chimney stack, water tanks, connections for water, drains, all measuring instruments and other accessories, at £11,000, thus bringing the total cost of the generating station of the A.T. system to £29,040. For the B.T. system I propose to provide plant of exactly similar type to that I have already supplied to Vienna. As I have said before, I propose that the dynamos should supply two-thirds of the maximum net output of 600 kilowatts. We therefore require about 448 kilowatts at the terminals of the accumulators; or, adding an 8¼ per cent. loss in the charging mains, 490 kilowatts in the generating station; 20 per cent. of reserve power will bring this up to 600 kilowatts, equal to 1,000 I.H.P. Such a plant may be conveniently divided into six sets of combined engines and dynamos, each of 100

kilowatts. Adopting the figure given above, viz., £8 12s. per
I.H.P., as the cost of the motive power, and that given by the
writer in *Industries* at £8 per kilowatt, or £4,800 for the six sets of
dynamos, and estimating that the cost of the building and other
accessories, as before detailed, for the A.T. system will, in this
case, be only £8,000, we find that £21,500 is the cost of the
generating station for the B.T. system.

The cost of the alternating transformers is a very difficult
matter to arrive at. The Grosvenor Gallery Company, I believe,
with few exceptions, supplies every consumer with his own trans-
former ; but Mr. Kapp, in his recent paper, proposes to lay down
a network of mains of much the same character as I use for the
B.T. system, and to supply this network by a certain number of
larger transformers, placed at frequent intervals in the network.
It is difficult to see how any saving can be made by this arrange-
ment. It will be noticed that, although it reduces the number of
transformers, it introduces a new element of expense and difficulty,
and that is, the providing of places to fix these large transformers.
It may be that such places can easily be found, but my experience
has been otherwise. It is not easy to find a place for any piece
of machinery of a size between that which can conveniently be
got into a surface box placed in the pavement not exceeding one
foot in depth by two feet square and a sub-station of the size I
require for my battery transformers. In most cases there is no
space either under the footways or under the street itself to form
a water-tight chamber of sufficient cubic capacity for the purpose.
Another difficulty and cause of additional expense which I think
would tend to counterbalance any saving from the decreased
number of transformers would be that the charging mains laid *to*
the transformers, and the distributing mains *away* from the
transformers, would have to be laid in the same trench or
culvert, and this would be no small source of danger to those
at work connecting houses on to the distributing mains. As
I have not been able to obtain any reliable data on this modified
system of Mr. Kapp's, I am obliged for the present to return to
the system using one transformer for every two houses, and
I think, for the reasons I have given above, the advocates of the

A.T. system will not be able to make any great saving by adopting Mr. Kapp's suggestions.

Table No. 3.

COST OF 10,000 LIGHT, OR 600-KILOWATT, PLANT.

A.T.—ALTERNATING TRANSFORMER DISTRIBUTION.	£	B.T.—ACCUMULATOR TRANSFORMER DISTRIBUTION.	£
Generating Station, Buildings, Chimney Shaft, Water Tanks, and General Fittings	11,000	Generating Station, Buildings, Chimney Stack, Water Tanks, and General Fittings	8,000
Dynamos and Exciters — 865 Kilowatts, including spare sets, divided as convenient ...	5,540	Dynamos — 600 Kilowatts, in 6 sets of 100 Kilowatts each...	4,800
Motive Power, *i.e.*, Engines, Boilers, Steam and Feed Connections, Belts, &c., at £8 12s. per I.H.P.	12,470	Motive Power, *i.e.*, Engines, Boilers, Steam and Feed Connections, &c., at £8 12s. per I.H.P.	8,600
500 Transformers, *i.e.*, one to every pair of houses, at £15 each	7,500	4 Groups of Accumulators, in all 240 cells, in series, at £40 per cell, including Stands ...	9,600
2,000 yards Primary or Charging Main, exterior to area of supply, at £308 per 100 yards	6,160	2,000 yards Charging Main, at £306 17s. 6d. per 100 yards (*see* Table 2)	6,137
20,000 yards Distributing Main, 50 m/m. sectional area, at £91 7s. (*see* Table 1)	14,270	20,000 yards Distributing Main, 161·25 m/m. sectional area, at £100 12s. 6d. (*see* Table 2) ...	20,125
Regulating Gear	500	Regulating Gear	2,500
	£57,440		£59,762

Accumulators to be used as transformers must be of sufficient capacity to supply 200 kilowatts per hour for the hours during which the demand exceeds the 400 kilowatts supplied by the dynamos. The load curves show that this period of maximum supply varies from about two hours in summer to four hours in winter.   If I provide four groups of accumulators, each of which has a total capacity of 259 kilowatts, or 1,000 kilowatts for the four, I provide for five hours of extreme demand, which is a condition of things extremely unlikely to occur, even in times of a fog.   For it must be noted that the demand for light during a fog in the daytime (in a district partly residential and partly supplying shops) is of a far lighter nature than the demand from the same district during the

ordinary hours of maximum lighting; the reason being that, during the hours of maximum lighting, not only the lamps in the basement, passages, and a certain portion of the dwelling rooms require to be simultaneously alight, but all upstair passages and a certain proportion of the bed rooms also require light. This is not the case during a fog; only the basement, lower passages, and living rooms take the light, and these to not nearly the same extent as they do in the evening. In order to meet a case of possible interruptions to the charging current, which might take place at the time of maximum demand, we require these accumulators to be capable of being discharged without damage to themselves, and at a fair rate of efficiency, at a rate not less than 650 kilowatts. It is this latter requirement that has so long delayed the introduction of accumulators as transformers. Up to a recent date far too much attention has been paid to the capacity, and too little to the rate of possible maximum discharge. This state of things has lately been remedied, and I am told there are several makers of accumulators who can supply them to fulfil the above-mentioned conditions, and at the reasonable price named by me in my estimate. It must be remembered that such accidental stoppages of the lighting current as are likely to take place would not probably last for more than a few minutes, that is to say, if due provision has been made for spare machinery, and if the arrangements for working the machines parallel and interchangeable have been properly carried out. Time will not permit me in the course of this paper to describe fully the switch-board and regulating arrangements necessary for the B.T. system. These require to be so arranged that the loss due to the difference of E.M.F. between the charge and discharge of the batteries may be as small as possible. It is quite possible to work the batteries parallel to the charging current during the hours of maximum demand with little or no loss from this cause, the loss being confined to the hours when the batteries are being finally filled up before the engines are stopped. The sub-stations, which contain the accumulator transformers, can in almost every case be placed on " mews " property, and I have ascertained from accurate inquiry that there is no difficulty in obtaining accommodation for

them in such property, with but few exceptions, within 300 yards radius of the houses to be supplied. In each case the sub-station consists of a battery room and a living room for the man in charge. The plant in each sub-station consists of the batteries themselves, the regulating appliances above mentioned, voltmeters and district meters as may be thought necessary.

Distribution.—In the case of the A.T. system the charging mains, as far as they lie within the area of supply, are in reality the distributing mains; in other words, we require 20,000 yards of distributing mains to reach the houses. I am informed that the section usually employed for the distributing mains of these 2,000-volt circuits, when the transformers are placed in parallel, is 50 millimètres cross section, or 19 strand, No. 15 B.W.G. From Table No. 1 you will see that this costs £71 7s. per 100 yards. I have not worked out the cost of so small a section laid on my own plan in the culverts, but it would certainly not be less than £96 per 100 yards. However, adopting the lower figure, the distributing main will cost £14,270. The writer in *Industries* says that 720 kilowatts will have to be generated at the generating station for transmission to the area of supply; dividing this by 2,000 volts, we get 360 ampères as the current in the charging main. Adopting Professor Forbes's tables for calculating the section of this conductor, we find that it ought to be about 645 millimètres, and if laid in the cable form, as in Table 1, but without surface boxes, would cost about £308 per 100 yards, or in all £6,160. It will be asked why I do not give to these charging mains the advantage of the somewhat lower cost of laying them as bare copper, as in Table 2, but I do not think that it would be safe to leave conductors carrying currents at 2,000 volts thus unprotected in the culverts, as the risk to workmen employed on the district system would be too great, to which must be added the great risk to the consumers, of making an accidental contact from the charging to the distributing mains. I should here remark that the very confident tone adopted by many supporters of the A.T. system rests on very slender foundations ; they appear to forget that the great success obtained in America and elsewhere has been with overhead wires used as the primary or charging mains. We all know what a high

degree of insulation is possible with overhead wires; we also
know what extreme difficulty has been met with in America in all
attempts to obtain sufficient insulation for wires carrying currents
of high E.M.F. when laid underground. As a matter of fact,
failure has been the rule, and success the exception. I believe
that the difficulties of the A.T. system will commence when the
contractors have to guarantee a perfect insulation with under-
ground cables carrying currents at an E.M.F. of 2,000 volts. Let
those who think otherwise read Prof. Elihu Thomson's remarks at
the recent electric light convention in America. He used the
following words:—" Distribution of current to groups of incan-
" descent lamps, arc light, or other high-potential lines should
" always be undertaken with great care, and it is thought that such
" work should only be undertaken when exceptionally favourable
" conditions for avoiding accidental leakages exist, especially
" should the potential of the line at the dynamo exceed 1,500
" volts, or an amount which would' sustain 30 arc lamps in
" series. One of the important elements of safety in such
" installations is simplicity and ease of inspection of the wiring,
" and no concealed wires should be used in them." Again, he
says, " For house-to-house and general incandescent lighting,
" where the distances from the station are not too great,
" the direct low-potential systems possess most of the advan-
" tages, the difficulties of leakage from the defective insulation
" being of course at a minimum; but where the distances
" are so great as to make the required outlay for copper in
" the conductors practically prohibit the extension of the system,
" a transformer or converter system, if of good design and
" economy, may be made to yield excellent results." Now the
writer of these words is one of the greatest authorities on electric
lighting matters in America, and a strong supporter of the A.T.
system. I think, therefore, that I have not dwelt too strongly on
the grave difficulties likely to be encountered by those who so
glibly talk of laying underground charging mains, carrying cur-
rents at 2,000 volts potential; and I think that in all probability
the additional precautions that will have to be taken to avoid
failure of the insulation, will be so expensive as to bring the total

cost of the 22,000 yards up to practically the same sum as is required for the 2,000 yards of charging main, and 20,000 yards of distributing mains of the B.T. system, for which no such special precautions are necessary.

I work out the cost of the latter as follows:—I use throughout bare copper mains of ·25 inch section for the distributing mains; that is to say, for the 20,000 yards at the rate given in Table 2 of £100 12s. 6d. per 100 yards, they will cost £20,125. The charging main will have to carry 910 ampères, and the extreme loss allowable being 8·25 per cent., its resistance must be 0·04 ohms. The double conductor will weigh about 45 tons; this can be laid as in Table 2, in brickwork in cement culvert, at the rate of £306 17s. 6d. per 100 yards, or £6,137 10s. I have put down an item of £500 for the regulating gear for the A.T. system, and another one of £2,500 for the regulating gear for the B.T. system. The totals thus arrived at, and which you see before you on the wall, are £57,440 as the total cost of the A.T. system, and £59,762 for the B.T. system. These figures appear high, but I cannot see how they can be reduced, unless under very exceptional circumstances. In both cases a very large portion of the cost lies in the distributing plant, and of this, as you will gather from my Tables 1 and 2, the copper plays but an insignificant part, the real heavy charges being those inseparably connected with underground cables, viz., the cost of insulation and those connected with disturbing and replacing the surface of the streets.

Comparison of Working Cost and Maintenance; Coal Bill of the A.T. System.—The generating plant on the A.T. system has to run continuously, that is to say, that the larger engines will be run for an average of three hours per day; and if it be found practically possible to subdivide the plant into a number of engines, and work them parallel, as is the case with the direct-current system, we may be able to credit the engines and dynamos with an efficiency of 75 per cent. during these hours. In order to meet the average demand of 2,100 units per day, the load diagrams show that they will vary during these hours from 400 to 720 kilowatts. We have as yet been furnished with no actual data as to the actual efficiency of the

alternating transformer system at various rates of output, that is, the E.H.P. actually delivered to the lamps, divided by the E.H.P. at the terminals of the dynamos. It must be recollected that, as the transformers must be large enough to supply the maximum demand of each house, whereas the maximum demand that the station can supply is probably not more than one-third of this, the transformers during this period of three hours cannot be more than one-third loaded, and therefore it is not probable that their efficiency is more than 70 per cent. It follows that, during the hours when the plant is working at 500, 400, and 300 kilowatts, the total efficiency, that is, E.H.P. delivered at the lamps, divided by the I.H.P. in the engine cylinders, including loss in mains, will not be above 54 per cent. Supposing that the steam engines can work at $2\frac{1}{2}$ lbs. of coal per I.H.P., the net quantity required per E.H.P. will be 4·65 lbs., or 6·2 lbs. per kilowatt. The output during these hours may be considered as 60 per cent. of that of the whole day, or say 1,260 kilowatts, therefore about 70 cwt. will be used during these three hours. During the remainder of the 21 hours the transformers will be working at a very small rate of output. It is probable that their efficiency will be reduced to 30 per cent., and the efficiency of the steam engines and dynamos to 50 per cent., or a total efficiency, that is, E.H.P. at the lamps, divided by the I.H.P. of the engines, of 15 per cent. only; there-fore, during this period it seems probable that 17 lbs. of coal will be used per E.H.P. or 22·5 per kilowatt. Therefore, the coal used in producing the balance of the daily output of 840 kilowatts will be about 8 tons 8 cwt., or, making a small allowance for the lighting up of boilers, 12 tons per day. The oil, water, and petty stores may be calculated at 7s. 6d. per hour for three hours during the day, and at 1s. per hour during the remainder of the time. Therefore, the total cost of material used in running this station will be £4,849. The B.T. system, on the other hand, will show great saving on this. For the output of 2,100 units, it will be sufficient to work the generating plant for about 5 hours at approximately its greatest rate of efficiency. I have had ample experience with this class of plant, and know that we can in actual practice produce the E.H.P. at the terminals of the

dynamo with the expenditure of $3\frac{1}{2}$ lbs. of coal. Taking the maximum loss in the accumulators, the efficiency of the system, that is, the E.H.P. at the lamps, divided by E.H.P. at the terminals of the dynamo, will be not less than 80 per cent.; therefore, we may say that each unit at the lamps can be produced for 5·75 lbs. of coal, or 5 tons 8 cwt. per day, or allowing a 25 per cent. margin for contingencies, 2,550 tons per annum. The water and petty stores may in this case be taken at 5s. an hour for 1,400 hours; the total cost of the material for producing 2,100 units by the B.T. system will be £2,517, or only about 57 per cent. of the cost of material by the A.T. system. *Turning now to the labour in the engine-room*, the table before you shows that, as in the case of the A.T. system the hours are continuous, three shifts of men will be required, it is necessary to have two foremen, six engine-drivers, and nine firemen; whereas in the B.T. system there is only one shift of men, one foreman, four drivers, and three firemen are employed. I have only estimated for two extra men in the engine-room for the A.T. system, but for the B.T. system I have allowed for four accumulator foremen in charge of the four accumulator stations, and two messengers. The totals thus arrived at for labour for working the systems are £1,389 8s. 8d. for the A.T. system, and £975 for the B.T. system. The salaries are £1,220 for the A.T., and £1,020 for the B.T. system. The last item we come to is one for *repair and renewals*. In both cases I have allowed at the rate of 10 per cent. on the motive power of engines and dynamos, 5 per cent. on the buildings and fittings, 10 per cent. on transformers, 15 per cent. on accumulators, $7\frac{1}{2}$ per cent. on the insulated mains that have to carry 2,000 volts, $2\frac{1}{2}$ per cent. only on the bare copper mains, the majority of which only have to carry 110 volts, 10 per cent. on all regulating appliances. Thus, nearly the heaviest item, the *maintenance*, comes to £4,683 5s. in the A.T. system, and £4,086 10s. for the B.T. system. The gross cost of producing 776,500 units by the A.T. system would be £11,939 13s., and by the B.T. system would be £8,598 10s., or 3·75 and 2·7 per unit respectively. In order to make a fair comparison between the two plants, when worked in the most economical manner, if the generating station of the B.T. system is worked

longer hours it can actually produce the daily average of 3,600 units named by the writer in *Industries*, and in order to do this the sole increase in the working expenses would be in the materials, to which £1,800 would have to be added. The cost, then, of 1,314,000 units would be £10,398 10s., or 1·9 per unit, by the B.T. system.

Table No. 4.

WORKING EXPENSES AND MAINTENANCE OF 10,000-LIGHT, OR 600-KILOWATT, PLANT.

	A.T.		B.T.	
	£ s. d.	£ s. d.	£ s. d.	£ s. d.
Materials—				
Coals: 4,380 tons at 17/-	3,723 0 0		...	
,, 2,550 ,, 17/-	...		2,167 0 0	
Oil, Water, and Petty Stores: 1,500 hours at 7/6 + 7,250 hours at 1/- ...	925 0 0		...	
,, ,, 1,400 ,, 5/- ...	...		350 0 0	
Total Cost of Material		4,648 0 0		2,517 0 0
Labour—				
2 Foreman Drivers at 45/- Drivers at 30/-; 9 Firemen at 24/-; Sundry labour	1,388 8 0		...	
1 Foreman Driver at 45/-; 2 Drivers at 30/-; 3 Firemen at 24/-; Sundry labour	...		975 0 0	
Salaries—				
1 Chief at £500; 2 Assistants at £200 each; 4 Clerks at £80 each...	1,220 0 0		...	
1 Chief at £500; 1 Assistant at £200; 4 Clerks at £80 each	...		1,020 0 0	
		2,608 8 0		1,995 0 0
Maintenance of Plant—				
Motive-power and Dynamos: 10 % on £18,010 ...	1,801 0 0		...	
,, ,, 10 % on £13,400 ...	..		1,340 0 0	
Buildings and Fittings: 5 % on £11,000	550 0 0		...	
,, ,, 5 % on £8,000	...		400 0 0	
Transformers: 10 % on £7,500	750 0 0		...	
Accumulators: 15 % on £9,600	...		1,440 0 0	
Mains: 7½ % on £20,430	1,532 5 0		...	
,, 2½ % on £26,262	...		656 10 0	
Regulating Gear: 10 % on £500	50 0 0		...	
,, ,, 10 % on £2,500	...		250 0 0	
		4,683 5 0		4,086 10 0
		11,939 13 0		8,598 10 0
2,100 units × 365 days = 766,500 units. Cost per unit	...	3·75d.	...	2·7d.

It will be seen, therefore, that while by both systems there is a probability of a fair return on the capital expenditure, yet in almost every respect the B.T. system is a more economical one. This arises from the fact that the main cause of expense in

the A.T. system, depending as it does on continually moving machinery, is the heavy cost of material, labour, and superintendence during the long hours in which the machinery is working very lightly loaded. The B.T. system shows very favourably in this respect, as even at the latter increased output of 3,600 units per day the machinery need not work on the average more than ten hours, so that one shift of men can do the work, and there is ample time for repair and inspection of the machinery.

In conclusion, I hope that I shall be credited with the endeavour to be absolutely fair to the two systems. I have throughout charged the lowest figures that I believe possible to the A.T. system, whereas, no doubt, as a contractor, I may at no distant date be called upon to substantiate the figures I have given as to the cost of the B.T. system. I have been compelled to put these figures sufficiently high to enable me to see my way to carry out the work at a reasonable profit; so that I am prepared to substantiate these figures in the only way that a contractor can do—that is, by offering to carry out work on prices based upon them.

Although I could have considerably strengthened my arguments by quotations from the numerous articles and papers which have recently appeared in the American and Continental journals dealing with this matter, I have preferred not to encumber my paper with such extracts, but have confined it to arguments based on data obtained either from the actual practice of my own business or from those installations from which I have been able to obtain information which I consider equally trustworthy. However, I think you can judge from what has been written by others that my opinions are very widely shared by a number of practical men in America. I have already given my reasons for not enlarging on the subject of the accumulators themselves; but there is one personal explanation with regard to accumulators which I wish to make. It has been stated in discussion here and elsewhere that I am a recent convert to the use of accumulators. No statement could be wider of the mark. On the contrary, I was one of the first persons to propose the use of accumulators for

electric lighting purposes. I believe I was actually the first man
in England, in course of the discussion which followed Mr. Lane
Fox's paper at the Institution of British Architects, to call
public attention to Mr. Faure's great improvements in their
manufacture, and from the very first I had the highest hopes
from their use. I put them into the large installation at the
Law Courts, which, as you know, was one of the first large
installations carried out in England; and I subsequently took part
in opposing Mr. Lane Fox's disclaimer to one of his patents, and
which had the object of claiming for Mr. Lane Fox the sole use
of accumulators in connection with central station lighting.

I do not deny that I have frequently expressed my dissatis-
faction with the accumulators which were at that time manufac-
tured. This was solely because of their unsatisfactory behaviour;
and I think nobody who had had so much to do with them as I
had could have avoided the use of strong language in connection
with them. But accumulators have been steadily improving in
the hands of those who have spent an immense amount of
thought and energy on their development, and I know that they
are now in a condition to perform the not at all onerous duty
required from them when they are acting as transformers for a
system of distribution.

Accumulators in private installations are far more unlikely to
be well managed than when in the hands of the skilled servants
of a large central station for public supply. In proof of this I
tell you that the large battery of 13,500 E.P.S. accumulator cells
at Vienna are kept in more perfect order, and have had far fewer
percentage of failures in plates, than any other battery that I
know of. This is simply because that in this case the accumu-
lators are of such importance that a small staff of men is kept
perpetually at work attending to them and preventing the
commencement of petty defects which, if neglected, would lead
to heavy repairs. You will see, therefore, that I have substantial
facts in support of my statements that the upkeep of accumulators
worked under such conditions is not necessarily a heavy item. I
believe that the E.P.S. Company will be the first to admit that
they could greatly improve on the pattern of accumulators now

in use at Vienna. In conclusion, I would strongly impress upon
the meeting that accumulators can, and at the present moment
do, perform this central station work remarkably well; and that
the percentage entailed by their loss, when used for transforming
in the manner proposed by me, is exceedingly small, particularly
during the hours of small demand, during which, at all events in
residential London, the alternating transformers will be working
at very little over 3 per cent. of their maximum capacity, and
therefore at their lowest efficiency.

Mr. GISBERT KAPP: If the makers of batteries had searched Mr. Kapp.
from one end of England to the other, they could not have found
a better advocate in their interest than Mr. Crompton. When,
some four or five years ago, I had the advantage of working
under him, we planned out, amongst other work, the Victoria
Electric Light Station, and even then Mr. Crompton intended to
use accumulators. A floor was to be reserved entirely for them,
but they have not been put in, very likely for the reason Mr.
Crompton mentioned, viz., that they were not reliable at the time.
I am very glad to hear Mr. Crompton say that they are reliable
now, though it is a surprise to me. Anyhow, this paper of Mr.
Crompton's must have the effect of inducing more public con-
fidence in accumulators, and for this reason I think the makers of
these appliances may be congratulated.

I need hardly say that, although I may in the course of my
remarks have to differ from Mr. Crompton, I nevertheless admire
the able and plucky way in which he has fought the battle of
accumulators against transformers. He has made an impression
upon my mind, but he has not convinced me. I shall only be fully
convinced when I have seen the installation described in his
paper actually carried out and successfully working.

Mr. Crompton thinks that 3,600 units per day is too much
for a plant which can supply a maximum of 600 kilowatts at the
lamps. According to the diagram for London, he may be right
in stating the output at only 2,100 units; but when you look at
the other diagrams you will see, without making any calculations,
that the output must be much greater. The reason why the

Mr. Kapp. London diagram is so unfavourable is that there are very few offices in this district, and not many theatres or public halls, which consume large currents over long hours. Taking a district where the lighting is varied, I think you will find that the daily output with 600 kilowatts maximum is rather more than 2,100 units, and may easily reach 3,600; but this must be determined separately for every locality.

I quite agree with Mr. Crompton that people often overlook the fact that a conductor consists of something more than copper. The practice of putting on simply a percentage for insulation is wrong. You ought to calculate in all cases what it costs to lay down the mains, and what the insulation itself costs. But, going through Mr. Crompton's figures, I find that he has put down a higher cost for the finished cable and for the laying than necessary. For instance, a very high-class lead-covered cable of half a square inch section costs 13s. per yard, or, for the twin main, £130 per 100 yards. This price is at the present increased cost of copper, and for comparison I must also take Mr. Crompton's price with the addition of £8 10s. for copper.

Mr. R. E. Crompton: That is the main lead.

Mr. Gisbert Kapp: Yes; this lead cable can be laid straight into the ground without anything in the shape of troughing, the only protection being a rough plank, charred and tarred, laid over the cable, so that when at a future time anybody opens the streets he knows, from the presence of the plank, that there is a cable underneath. The work of digging the trench and running in the cable from the drum would not cost more than 3s. a yard; so that the total cost, inclusive of £10 for surface boxes, would be £155, and not, as shown by Mr. Crompton, £187.

Mr. R. E. Crompton: That is what it does cost—what we have paid for it—and the work cannot be done under, that is the unfortunate fact.

Mr. Gisbert Kapp: The insulation of this lead-covered cable will be reckoned by a few hundred megohms; the insulation of the other must be reckoned by a few hundred, or, perhaps, a thousand ohms per mile.

Mr. R. E. Crompton: That is a guess.

Mr. GISBERT KAPP : No, it is not a guess; I am quoting what Mr. Kapp.
has been actually done. A cable has been laid for the Milan
Edison   Company,   lead-covered,   giving   750   megohms   per   .
kilomètre, or about 460 megohms per mile. For the Thomson-
Houston plant in Milan a cable has been laid, two years ago, .
carrying 1,500 volts, which before laying had a resistance of
1,200 to 1,500 megohms, and after it was laid and had all the
joints in it the insulation was still 600 megohms per kilomètre,
or say about 370 megohms per mile. Another cable, carrying
alternating currents under 2,000 volts pressure, was laid in Milan
nearly two years ago. This was a Siemens concentric cable, and
was at first used for several weeks for lighting one of the
theatres.   The following year the cable was used for several
months for the lighting of the Milan International Milling
Exhibition, held in the vicinity of the theatre; and during this
time it did not undergo any alteration in its insulation resistance.
Later on, when experiments were being made with multiple-arc
alternate-current dynamos, the insulation suddenly broke down,
establishing a short-circuit; the defect was, however, immediately
repaired, and after that the insulation was found to have suffered
only a slight permanent diminution. The regular lighting of
two theatres through this cable was inaugurated after the
exhibition, and it has been in continuous operation with 2,000
volts since then without the slightest hitch, and no change has
appeared in the insulation.

A Berthond-Borel cable of 150 square millimètres costs, at the
present increased price of copper, £40 per 100 yards.

Mr. R. E. CROMPTON : I took £54.

Mr. GISBERT KAPP : And allowing for laying, the twin main
comes to £105 ; whereas Mr. Crompton's main comes to £100.
Here we have a saving of 5 per cent. Is it worth while to save
5 per cent. in the one case, and 14 per cent. in the previous case,
by sacrificing insulation resistance ? If Mr. Crompton's mains
were to prove in practical work perfectly reliable, even a small
saving would be worth having ; but I would rather not be the
engineer in charge responsible for keeping up the insulation of
the naked conductors he puts underground.

Mr. Kapp.   Mr. Crompton said that in an alternate-current plant what he calls the " charging mains " should at the same time also form the network going through the whole district.   This is not a good plan, because you would in this way have some 22,000 yards of high-pressure cable.   It is, morever, not the plan I advocate : I advocate having the long lines forming the network a low-tension cable, and laying down only a few high-pressure feeders to stated points.   By this means, instead of having 22,000 yards of a high-pressure cable, you would only have perhaps 5,000 yards, and the rest of the cable would be for low pressure.   I do not pretend that the distributing network would be cheaper than that proposed by Mr. Crompton, but you would have the advantage of comparatively short high-pressure feeding mains.

Now as regards cost.   This depends, amongst other things, on the size of the distributing mains.   Mr. Crompton takes mains of a quarter square inch section, and feeds these from four local batteries which would be installed in mews as near as they could be got to the line of lighting.   I think that either he has too few batteries, or he has too small a section of distributing main.   It is evident that the fewer the points at which the network of mains is fed, the larger must be the section of this network to be able to take the current without serious loss of pressure.   Mr. Crompton allows 4 volts as the maximum variation.   I think that is rather too much.   You must remember that if you allow 4 volts difference in pressure it means something like 10 or 15 per cent. difference in light, and I doubt whether the British householder will put up with such a large variation.   He will want something better than gas, and we can give it him by limiting the variation at his lamp terminals to 2, or at the outside 3, volts out of every 100.   It must also be remembered that, no matter how perfect the regulating appliances may be, they cannot regulate with mathematical accuracy : a little latitude must be allowed or a possible error of at least 1 volt ; so that, starting with 4 volts loss in the mains, we may have 5 volts variation at the lamps, which would be objectionable.   I think the variation should not exceed 3 volts at the outside.

It is impossible to settle off-hand the size of main required

with a given amount of current in a district; it depends to a Mr. Kapp. great extent on the configuration of the district, because you would connect the network in as many points as possible, so as to take advantage of the carrying capacity of the cable through cross streets; and therefore it is impossible to say off-hand whether a quarter square inch main is enough for a 600-kilowatt output or not. We can, however, test this by reference to an installation actually about to be established; and for this purpose I take an installation which is just now in everybody's mind, viz., Bradford. You have seen the tenders and specifications for this work, and by the map of Bradford you can very easily verify what I am going to tell you now. In Bradford there are four feeding centres, and from these will be served something like 3,000 lamps. The whole of the current will have to be supplied to these lamps from the four feeding centres, making thus 750 lamps depending upon each feeding centre. The network main to be used varies from ·15 to ·25 square inch, or, say, has an average section of ·20 square inch area. The variation of pressure will be within the limits that I have just mentioned as admissible. Now with a main of a quarter square inch it would be possible to supply at Bradford 950 lamps from each feeding centre, or, altogether, 3,800 lamps from the four centres. For 10,000 lamps simultaneously alight we would require at least nine feeding centres, if the distribution were on the simple two-wire system. Mr. Crompton's system is, however, a five-wire system, with batteries parallel to the lamps, and here we cannot take advantage of the interlacing of the network as we can in the simple two-wire system, because we have four distinct potentials, and can therefore only interlace such parts as are of the same potential; so that we shall require with this system of batteries rather more feeding centres, to make up for the loss of carrying capacity of the interlacing links in the network. Thus we would require about ten feeding centres, and not four, as suggested by Mr. Crompton, in order that the variation of pressure may not exceed 2 or 3 volts. It is therefore necessary to have, not four attendants for the local batteries, but ten attendants, which, at £100 a year, makes an extra £600 in

Mr. Kapp. the working expenses.   There is, however, another way out of the difficulty : we can retain the four centres, provided we make the mains large enough to carry the current without a greater drop in pressure than 2 volts.   In this case the section will have to be increased in the ratio of $\left(\dfrac{10}{4}\right)^2$; *i.e.,* instead of a main of a quarter of a square inch, we should have to use a main $1\frac{1}{2}$ square inches. This main, according to Mr. Crompton's table, would cost about £200 per 100 yards.   That is exactly twice what Mr. Crompton took in his estimate ; so that the amount for mains alone would have to be increased by £20,000.   The maintenance would also have to be increased by £1,532 ; and it will be probably cheaper to subdivide the battery into ten local centres.

I should like to ask Mr. Crompton whether he has included in his estimate any item for the rent of rooms where the accumulators are to be fixed.   I do not see this item, and I think it is doubtful whether he could get accommodation for his battery centres at a lower rental than £50 per annum.   For the ten centres this makes another £500, which I do not see provided for in the estimate.   Adding the wages of the six extra men to look after the batteries, we have £1,100 to be added to the working expenses, and this item alone would put a very different complexion upon the comparison of the cost of the two systems. Suppose, now, that for the A.T. system we adopt Mr. Crompton's quarter square inch mains, and feed them by ten transformers, each capable of supplying a maximum of 1,000 or 1,200 lights. It will not be necessary that the total capacity of the transformers should be larger than the total capacity of the station, because they feed a common network, and if in one portion of the network the demand is small, the current put in at that point will flow to other portions where the demand is larger.

Mr. Crompton says he is ready to supply the batteries at the price he gives.   On the other hand, I know several contractors who would be perfectly willing to supply the ten transformers, each having a capacity of 1,000 to 1,200 lights, for £3,000. That is a reduction in the A.T. estimate of £4,500.   The space occupied by each transformer would not exceed about 20 cubic

feet, and it would not be very difficult to find accommodation Mr. Kapp. under the pavement for the transformer, placed in a cast-iron box, which could be made watertight.   Or the transformers might be put on mews property. If you could put batteries containing sulphuric acid into mews, I do not think it would be difficult to find a little cupboard somewhere in the mews where a transformer could be put.   The cupboard would, of course, have to be fire-proof and provided with an iron door.

If Mr. Crompton has over-estimated the cost of transformers, I am rather inclined to think that he has under-estimated the cost of the batteries.   In making this remark I am not disregarding what he told us just now, viz., that he is willing to supply the batteries at the price given in the paper ; but I should be sorry if he had to do it at a loss.   Now my authority for the statement as regards the insufficiency of the estimate for the batteries is Mr. Crompton himself.   A few months ago I asked him, as a question of scientific interest more than anything else, what, in his opinion, would be the cost that should be allowed for batteries which should have a very rapid discharge ; and from the figures he was kind enough to supply me I have taken an average, and, putting the most favourable construction upon these figures, and allowing for very large cells, I found that the cost would be £30 per kilowatt.   That was Mr. Crompton's information to me a few months ago.   According to his paper the cost per kilowatt is only £15, for he allows £9,600 for the cost of batteries and stands, from which he expects to get 650 kilowatts output.   If £30 per kilowatt is right, the battery should not cost £9,000, but £18,000, which puts another £9,000 on the top of the author's estimate.

I should be glad if Mr. Crompton would tell us in his reply if I am right in interpreting his figures to mean that he will supply, or he knows somebody who will supply, a battery costing, say, £150, and giving 10 kilowatts output for a reasonable time and with fair efficiency.

I should like to say a word or two about the efficiency of alternating-current transformers.   Mr. Crompton says that they give 70 per cent. efficiency when loaded to one-third, and that when loaded less they will give less than 30 per cent.   Now some

Mr. Kapp. of you no doubt remember that Professor Ayrton gave the result of experiments with a two-horse-power transformer, which, when working at 318 watts—that is, 21 per cent., or say a fifth of its full output—gave 80 per cent. efficiency; at a third of the full output the efficiency was 85·8 per cent.   I maintain that it is not necessary to work transformers at less than a third, or at the outside a quarter, of their output.   If you adopt the plan of working a common network, and feeding it by ten transformers, if your supply falls to less than a half, turn off one or two of those transformers and keep the rest to do the work; if the supply falls to a tenth, turn off eight and let only two supply the work; and in this way you will be able to work the transformers always at a fairly large output.   The efficiency of the transformer does slightly decrease when the load decreases, but the efficiency of the feeding mains increases.   Say you allow 20 per cent. loss in your feeding mains at maximum output.   This might be a very economical way of working.   The maximum output only lasts for two or three hours, and during that time the mains absorb 20 per cent. of your power.   You have, then, this 20 per cent. to draw upon when the transformers are working with smaller output, so that there is a certain tendency for the deficiency or loss in one part of the apparatus to be balanced by the gain in the other part; and, on the whole, if you work the transformers with a reasonably large output, you will find that the efficiency does not vary very much. It will be a little less when working light, but it will not be so very bad as Mr. Crompton makes out.

I should like to conclude my remarks by asking Mr. Crompton a question regarding the regulation.   His system is really a five-wire system, with accumulators placed parallel with the lamps. Now, when we work batteries in private installations, we reckon $2\frac{1}{2}$ volts per cell for the charging current, and when the batteries feed the lamps we reckon that we get only 2 volts; so that the E.M.F. at the terminals of the battery must be 20 per cent. greater when they are being charged than when they are discharging.   How is this difference of pressure kept from the lamps?   How can Mr. Crompton ensure a constancy of potential whether he is charging or discharging?   I can only see two ways

in which it can be done—either by rheostats such as are used in Mr. Kapp. any Edison station, and which, as you know, require continuous watching and adjusting, or by shifting the terminal on the battery.   In this case the end cells would receive a different charge from that given to the other cells of the battery.   I would like to ask Mr. Crompton how this treatment affects their durability and efficiency.

The PRESIDENT : Before I call upon any other gentlemen to The President. join in the discussion, I think I ought to mention that we shall certainly have to adjourn it for a special meeting to be held next week, when, however, it should be finished, allowing time for Mr. Crompton to reply, as I understand he cannot be present afterwards ; and therefore I ask gentlemen who will not be present next week to speak during the remainder of this evening. We have fallen into rather a bad habit of late in sitting beyond the usual hour, and I should like to adjourn the meeting to-night at 10 p.m.

I understand Mr. Parker, of Wolverhampton, is present, and would like to speak.

Mr. T. PARKER : I certainly did feel inclined to speak this Mr. Parker. evening, but this being my maiden effort in this hall I feel rather new.   I have listened to Mr. Crompton with a great deal of pleasure in some part of his efforts to make it clear to us as to the cost of the cables and various parts of installations with alternating currents, and also for continuous currents ; but I would rather have heard more of the experience that has been obtained by past practice than have listened only to Mr. Crompton's personal experience and speculations in this matter.

There will be no necessity for rejoicing in respect to myself with regard to batteries.   I am not a recent convert.   I have seen about seven years of life in an environment of batteries ; and as we are turning out now about 5,000 cwt. of batteries per annum, I have had some little to do with them.

I would prefer to hear something from Mr. Crompton on the mode of regulation which has been referred to by Mr. Kapp.

The figures as to the cost of batteries in Mr. Crompton's estimate I do not much take exception to ; I believe they can be

Mr. Parker. worked out at the price he names if a practicable mode of regulation can be adapted for the purpose. He speaks of what he does at Vienna, but does not tell us what he does there in respect to this regulation; if he works the system in parallel, there is the difficulty Mr. Kapp has referred to.

Then, again, the matter of 400 or 500 volts does not seem to be at all a sufficient pressure to work batteries with, if they are to compare with the alternating-current system.

The effort which was made some years ago at Colchester seems to have fallen out of notice. Mr. Kapp merely mentions it in his paper, and passes on. I was to a considerable degree associated with the effort at Colchester, being called in to design dynamos, which took the place of the Brush dynamos, to run the installation, and I do not think anything was ever published about them. They were designed to give 1,000 volts and 22 ampères, and they did this work satisfactorily. They were used in series in that installation from 1884 to the close of the installation, giving 2,000 volts and from 22 to 36 ampères at that time, and there was no difficulty with them. Double sets of batteries were employed, and the lamps only ran from the batteries. The manipulation of the batteries was simple—taking out a charged set and putting in a discharged set—had the mechanism been practicable and reliable. This is a costly mode of installation, as it makes no use of the main charging station for lighting direct, and the efficiency return is low, being only the efficiency of the battery, whilst the cost of batteries is double what it may be by properly operated automatic means. The reasons as to the failure of that installation do not seem to have been carefully studied. Its failure seems to have been due to the insufficiency of the number of lights for a central station, the mode of insulation used with the cells, and heavy first cost, with low energy return. The cells, too, at that day had not been perfected to the extent they have now.

Mr. Crompton said something to us about the wonderful perfection of cell that he has arrived at, and I should like to see or hear something more of it. I should be very pleased to send some cells from Wolverhampton to any place to be tested against

anything Mr. Crompton can supply or find, and supply them for Mr. Parker.
the interest that you gentlemen may have in this question. We
have just lately been able to carry out some improvements in
cells, which we shall shortly be able to make public, which will
give a much greater life to accumulators, and will enable us to dis-
charge them at a much more rapid rate. I think the Colchester
experience in regard to batteries was the longest; it certainly was
a much longer experience than Mr. Crompton boasts of, viz., nine
months; and I do not see that Mr. Crompton has done anything
more at Vienna than has been done in many private installations.

In regard to cost, my experience, although I have not read all
that has been published, enables me to say that the battery
system could be carried out very much at the same cost as the
alternating-current system, and possibly may be preferable.

As to the dangers and difficulties of the alternating-current
system, perhaps one of the most unique instances of experience
with it, and which may answer some of the questions which are
troubling the minds of many people at the present time respecting
the risks of high pressure, &c., is the Eastbourne installation. In
that installation we have an instance in which two or three elec-
tricians—hard-working, good men—have carried out for the first
time in their lives, and mainly designed, an installation which has
been running nearly two years. There has been no accident with
it, no mishap, no stoppage of the machine; there was no duplicate
plant, and it has run from the first minute it was put there until
now without any stoppage or cessation of the light. That is a
very creditable thing to say of the people engaged on the work—
chiefly Mr. Lowrie and Mr. Hall. I designed the dynamo and
put it there. These are matters of history and experience, and
are the right and property of us all as electricians. I say these
things although I do not wish myself to throw up any fireworks
for "Elwell-Parker, Limited;" but still, as Mr. Crompton dwelt
upon his experience, and did not make it a matter of discussion
of the whole subject, I thought I might tell you these things. I
see there are gentlemen here hoping to speak this evening, and
as I think I have said as much in effect as if I had spoken for an
hour, I will sit down.

Mr. Preece. Mr. W. H. PREECE : It appears to me that the broad question that Mr. Crompton has brought before us is rather lost in a cloud of details and in the criticism of certain estimates that he has brought before us.   There is no question that occupies us, as electricians, at the present moment of more paramount interest than that of the proper distribution of electricity over areas that want to be lighted.   There are over this country at the present moment some thirty or forty corporations and communities who are anxious to introduce the electric light.   Some of them are guided and controlled by consulting engineers, and these consulting engineers are in the unpleasant position of having to postpone their fees by advising their clients that the time has not yet arrived when they can say to them, " Go ahead ; I advise you to adopt this system, or I " advise you to do that."   I am in that position, and have held back for the past two or three years some very large installations because it has been impossible to decide whether the best system to adopt is that of secondary generators or that of secondary batteries ; and I think I shall fulfil the purpose of Mr. Crompton if I give some of the reasons that have kept me in the balance like a see-saw between secondary generators one day and secondary batteries the next.   If I have any prejudice, or any feeling at all, it would be in favour of secondary batteries.   I do not suppose there is any man present in this room who has had more practical experience of the working of secondary batteries than I have had myself.   There is no form of secondary battery that I have not tried, there are few forms of secondary battery that I have not burst up, and there is therefore nobody who perhaps can speak with greater confidence of his opinion of the performance of secondary batteries.   In the first place, I commenced at the same time as Mr. Crompton did.   I went over to Paris when M. Faure first brought out his battery, before it was made public, and I formed, as I often have formed before, a wrong opinion about it.   Shortly afterwards I found others who were rather more far-sighted than myself, and now it is one of the most beautiful instruments that an electrician has at his disposal.

My experience of the secondary battery is threefold.   I have

used it in my own house since the early part of 1884, and for Mr. Preece. the last two and a half years I have never seen such a flicker in the light in my house as we see every night we come here in this hall lighted by gas. There has never been a fault, there has never been a disturbance, and the reason is simply that the batteries have been well taken care of. If anybody imagines that a secondary battery can be put into a cellar, and that a man can go round once a month and look at it and see whether it is all right, and that the battery will remain in working order, he makes a very serious error. A battery is a thing that wants incessant and constant attention. You must regulate the current going into it, you must regulate the current going out of it, and if you treat your battery as you would treat a well-dressed infant, that battery will grow up and will show you in its old age that you have trained it up in the proper way it should go.

There is one point that Mr. Crompton has omitted in his remarks, and I think it is a very serious and important one connected with batteries. It is that you cannot take out of a battery all that you put into it; the efficiency of a battery is a variable quantity. I have had batteries that have given me 90 per cent. efficiency; i.e., I have succeeded in getting out of a battery 90 per cent. of what I put in; but that is only the case when the battery is charged with a small current and when a small current is taken out of it. I mean that, supposing you charge, say, a 15-L. E.P.S. cell with 15 ampères, and you take out of it 10 ampères, I believe you will get about 90 per cent. efficiency; but if you take such a battery and put into it 30 ampères, and expect to get out of it 25 ampères with such an efficiency, you will be very much mistaken. The efficiency in such a case is very much less than 90 per cent.; in fact, I do not think you can calculate, when you put in a strong charging current and take out a large output, upon an efficiency of more than 60 per cent. I am afraid that in his figures Mr. Crompton has not quite taken notice of that particular point.

Another point that I should urge very strongly in favour of batteries is their extreme safety. There is something in a battery that is absolutely reliable. A failure may occur, from causes that

Mr. Preece. have been mentioned, in a single cell, and in a set of 25 cells the failure of one cell may drop the potential by about 2 volts, but it does not make very much difference; the battery will maintain itself for a good many years in perfect working order. I say "a "good many years." I ought to draw the line somewhere: there is an end to a battery, but when that end comes I do not know. I have had, as I said just now, batteries of various kinds, but I have now in the Post Office a set of 25 cells of the original form of Planté cell, manufactured by my friend opposite, Mr. Parker; I have had them four or five years, and they are perfectly good at the present time. But they have been treated carefully; if they had not been treated in this gingerly fashion, I think they would have tumbled to pieces long ago.

I have had in my house a set of 25 E.P.S. cells, and they are as perfect to-day as they were two and a half years ago when supplied to me. During the whole of that two and a half years I have only had two faults, each of which happened during the night-time, when the light was not in use. One arose from a plugging of the negative plate falling across and short-circuiting one cell. It was removed the next morning, and without any trouble, because my batteries are examined every morning; the density of every cell is measured by a hydrometer every morning, and they are looked over to see that there is nothing wrong. When you test them with a hydrometer you know at once by the density of the cell whether anything has gone wrong.

The second fault arose from the falling of the peroxide. The peroxide of all these batteries falls like a gentle shower when the battery is charged and discharged; there is a gradual accumulation of this stuff at the bottom, until at last the plates are connected through it; and in this particular cell I took the precaution to have the amount of peroxide at the bottom, that rose up to short-circuit it, weighed, and I found that it weighed 2 lbs. Very well; now, if in two and a half years 2 lbs. of peroxide have accumulated at the bottom, how long will that battery last? There is a rule-of-three sum that anybody who knows the weights of peroxide in the positive plate, or the weight

of material in the battery, can easily calculate for himself.   At Mr. Preece.
the moment I forget the weight.

Mr. T. PARKER: About 16 lbs. in the positive plate.

Mr. W. H. PREECE: At any rate I have come to this con-
clusion—that if a battery be carefully tended and carefully looked
after there is no earthly reason why it should not last for ten
years.

As to depreciation, that is one of the points of greatest
importance in comparing the relative performance of batteries
and transformers.   I say that the depreciation of batteries may
be taken at 10 per cent.   I do not raise any objection at all to
Mr. Crompton's figures; I think he is extremely fair.   He has
given all he possibly can in favour of secondary generators, and
he has erred, if at all, on the wrong side, as far as he is concerned,
with regard to batteries.   It is not at all a question of the
relative prime cost of the two; it is not so much a question of
the relative annual cost of the maintenance of the two; but I
think there are some points that he has not given due weight to
that ought to be taken into consideration.   One is that which I
have referred to—depreciation; the other is the loss of energy
in batteries.   But there are other losses.   There is the loss in
the leads, there are losses in the engine-house itself; and I think
you will find, on adding up the two, that the losses in favour of
the secondary transformer are very much less than the losses
in favour of the secondary battery.   Therefore, if we take loss of
energy, I give my vote in favour of transformers.

Batteries, however, have the enormous advantage over
secondary generators — I cannot get out of the old term
" secondary generator ; " some people call them " converters," and
some " transformers ; " I always think of them as " secondary
" generators," and I am obliged to use that term—that with a
secondary battery you always have a store of energy to draw
upon, night or day, in season or out of season, in winter and in
summer, whatever may happen out of doors ; men in charge of
the sewers may tear up the streets and break down your con-
ductors, but you have got your batteries independent of all.
Your engines may break down at the central station, but you

Mr. Preece. have your batteries to give your store, and so you are always independent. Again, in the future there is no doubt that motors will be used to a very large extent: motors for small trades, for household purposes, for sewing machines, and for all things of that kind, will be very largely used. With batteries you have always got a · store of energy to work your motors; with transformers, where are you? You are dependent upon something that is to come in the future, and that is a motor to work with alternating currents. While on this point, I may remark that in nearly all the papers that I have read on this question that have been brought before the societies in America, they always argue on what is going to be done in the future. For instance, there is a capital paper that Mr. Crompton did not refer to by name, but did refer to in substance—a paper just read by Mr. Leonard before the Electrical Institute in America, in which he argues in favour of the three-wire system as against the transformer system, and nearly the whole of his argument is based on what is going to be done in the future in the improvement of the glow lamp. *If*, says he, the glow lamp can work with $1\frac{1}{2}$ watts per candle, then the direct-current system, as shown by the Edison three-wire system, will lick creation. Those are not his words, but they are his argument. Now, therefore, the two great reasons in favour of batteries as against transformers show that batteries give an invariable store of energy and a means of working motors. But secondary generators have got something on their side. Secondary generators enable you to use very high differences of potential, and high electro-motive force leads to cheap conductors. This has not been shown to the advantage that it might have been shown by those who advocate the transformer system. The use of high electro-motive force also means very small loss of energy in the conductors, and enables you to go to great distances; and that is a great point, because by going to great distances with small currents the local authorities are quite out of it: they cannot buy up your system, for if you carry your conductors through various parishes, districts, and local boards, when the 21 years, or, as we hope, 42 years, come, there will be no local

authority and no local board who can possibly buy up a system *Mr. Preece.*
that is distributed over a very large area.

With due regard to you, Sir, I should like to adjourn this
discussion. I have not said all that I wanted, but I feel that I
have talked too much; and if you will allow me to open the
adjourned discussion next meeting, I will promise not to occupy
more than ten minutes with my further remarks.

The PRESIDENT: The only thing is, I wish it to be understood *The*
by all speakers next meeting that the discussion must finish in *President.*
time to allow of Mr. Crompton to reply that evening, and I know
there are many gentlemen who wish to say something.

I have now to announce that there will be an Extraordinary
General Meeting held on Thursday next, April 19th, for the
adjourned discussion on Mr. Crompton's paper.

A ballot took place, at which the following were elected :—

Foreign Member :
Joseph Wessely.

Member :
The Hon. Charles Algernon Parsons.

Associates :

George Bailey.	Frederick Thomas Schmidt.
Walter Lewis Copp.	Denys Leighton Selby-Bigge.
John Henry Campbell Hewett.	William Winstanley Strode.
Walter Hibbert, F.C.S.	O. Ricketts Swete.
Hugh Alexander Irvine.	George Gatenby Turvey.
William Rawlins.	

The meeting then adjourned.

An Extraordinary General Meeting of the Society was held at
the Institution of Civil Engineers, 25, Great George Street,
Westminster, on Thursday, April 19th, 1888—Mr. EDWARD
GRAVES, President, in the Chair.

The minutes of the previous meeting were read and approved.

The
President.

The PRESIDENT : We will now resume the discussion upon
Mr. Crompton's paper, for which purpose this Extraordinary
General Meeting has been called. I have already received the
names of ten gentlemen who are desirous of speaking; and, as
there may possibly be nearly as many more who wish to take
part in the discussion, but have not announced themselves, I
would impress upon all the desirability of as much brevity as
possible.

Mr. Preece.

Mr. W. H. PREECE : Last time I said nearly all that I could
say in favour of batteries, and I pointed out that the three
principal arguments that were adduced in favour of distributing
by means of batteries were—firstly, that the battery system offered
a very convenient means of storage, so that we might have our
light always available ; secondly, that we could utilise the
currents for motor purposes ; and, thirdly—most important,
perhaps, of all—the fact of distributing currents by means of
batteries enabled the whole 24 hours to be used in the day, a
smaller plant and a smaller power being used during those hours
of pressure when, as is seen from the diagrams on the board, the
load line rises to very high peaks.

Those being the three arguments in favour of batteries, there
are three other arguments that have been adduced, and probably
can be adduced, in favour of transformers. First, and perhaps the
most important argument in favour of the use of transformers, is
that they allow the use of much higher electro-motive force, and
therefore a much smaller current can be used by means of
transformers than by batteries. Secondly, the transformer itself
being such an extremely simple apparatus, it can be stowed away

Mr. Preece.

in any odd corner, or even, as Mr. Kapp suggested, be laid underneath the pavement in a dry well. The transformer involves considerable economy in labour and supervision ; the battery is a very good thing in itself, but it requires a considerable amount of supervision and personal attention. The third reason in favour of the transformer as against the battery is that unquestionably the efficiency of the transformer is very much higher than that of the battery. I pointed out, and I think Mr. Crompton has agreed with the figures, that you cannot calculate or rely upon getting more than 60 per cent. out of the batteries ; but with transformers, although theoretically and experimentally as much as 92 and 95 per cent. has been obtained, practically I do not think you can say that the return is quite so great, but at the same time you can certainly conclude that the efficiency of a transformer is higher than that of a battery. The argument that the battery enables us to utilise a small motive power as against the transformer is about as broad as it is long. We must provide something to give us energy when our load diagram attains those Alpine peaks ; and whether our capital be invested in additional machinery at the central station, or whether it be distributed over the area lighted in the shape of batteries, is a matter of little consequence to the financial people, for whether £ s. d. are expended in engines or in batteries, it comes to the same thing.

There has been rather a tendency in this paper, and also in the early part of the discussion by one or two members, to decry the good that has been done by the alternate-current system as employed for transformer purposes. The experience of the past in this country and in other countries cannot be ignored, and we cannot too much admire the pluck and the determination with which Gaulard, assisted by his colleague Gibbs, pushed, against the opposition of all the electrical world, this secondary generator or transformer principle. That principle has been taken up by the Grosvenor Gallery Company in this country, who, as far as we are concerned, have been the pioneers of electric lighting by its means, and have been benefactors to the history of electric lighting. They have carried the light to places that certainly

Mr. Preece. would not have been reached if the transformer principle had not existed. They have been able to carry their wires—in spite of Acts of Parliament, in spite of Vestries, in spite of every opposition—to houses and homes, to clubs and hotels, all over London; and not only has this been done in London, but it has been done in other parts of the country. The secondary generator principle is at work at Eastbourne; and I see Mr. Hammond is present to-night, and I have no doubt he will be able to give some account of what they have done there. We also know that this principle has been extended largely on the Continent, but to a still greater extent in America; and I am sure we are all very much pleased indeed to find at our table to-night Professor George Forbes, who only returned from America two days ago, and from whom we hope to hear something of the progress made on the other side of the Atlantic. One argument used by Mr. Crompton was that the secondary generator system involved considerable consumption in steam. He pointedly asked, on a previous occasion, Mr. Mackenzie to give us some information as to the tons of coal used per lamp in the Grosvenor Gallery installation. Of course that implied that there was an enormous waste of coal and waste of steam in carrying out the secondary generator principle; but while there may be a great deal of truth in the basis of Mr. Crompton's suggested questions, I should like to point out that any excess of consumption of steam in a central station is not necessarily the consequence of the secondary generator principle as opposed to the secondary battery principle, but it is due to the engineering principle that has been adopted in carrying out the engine working of the central station. One of our American friends has very pointedly put this in that terse way which they are so fond of putting. He says, " The " money is lost or made between the coal-shovel and the belt." He means that any pecuniary loss that is incurred in working an electric light installation is in the engine-room, and not in the distribution of the current.

This raises a question quite apart from the question we are discussing, viz., of the relative advantages of batteries and transformers. It is a question of the relative advantage of putting all

Mr. Preece.

your power in one or two big engines, or distributing it amongst
a series of small engines that will enable you to work at different
times in the day, according to the load diagram, with a different
number of engines. The very considerable advances that have
been made in the economy, in the construction, in the price, in
the character of high-speed engines by Mr. Willans, who is here
to-night, justifies us in hoping that the solution of the question
of the proper construction of a central station is in the near
future a very simple question to solve indeed. If economy is
desired the power must be divided amongst a series of small
engines and small dynamos, rather than by concentrating all your
eggs in one basket and having 5,000 horse-power working a 4,000-
unit dynamo.

There are two other questions rather apart from the subject
we are discussing that deserve consideration on our part. A
question not raised by Mr. Crompton is as to the relative advan-
tage of 30-watt or 10-candle-power lamps as opposed to 60-watt
or 16-candle-power lamps. I am a very great advocate for the
use of the small candle-power lamps. The use of 30-watt lamps
enables us to at once double the capacity of our dynamos; and the
human eye and people are so curiously constructed that if you
give them first a 10-candle lamp they are perfectly satisfied with
it, but if you give them a 16-candle lamp and then substitute for
it a 30-watt or 10-candle-power lamp they begin to growl and
grumble, and you are soon obliged to go back to the 16-candle-
power lamp. Another question requiring consideration, and
one upon which we, the Society of Telegraph-Engineers,
ought to take some stand, is that of the variety of voltage at
which lamps are issued: we have 20, 30, 42, 50, 60, 80, 90, 100,
and 110 volt lamps; in fact, one rarely goes to two installations
and finds lamps of the same voltage and of the same character
used. There ought to be some law laid down; it ought to be
definitely decided whether we should always use 100 volts, or
50 volts, or whatever it may be. In America they are coming
down very much to the use of a 50-volt lamp, because it is found
there—I do not know how far it is true—that a 50-volt lamp has
a longer duration than a 100-volt lamp, owing to the fact of its

Mr. Preece. having a thicker filament. On this point we may perhaps hear something from Professor Forbes. The point that I wish to urge to-night is this—that the question before us is rather a question of the distribution of energy. It is not so much a question of the arrangement of the central station. We generate at a central station a certain amount of energy in the form of horse-power ; we distribute that energy over a large area, and we expend it in rooms and halls and theatres in the form of light. Which is the most economical? Is it most economical to distribute that energy by means of batteries or by means of secondary generators? We practical men must be governed by results. We always prefer that others should carry out experiments for us when the cost of them is pretty considerable. At the present moment men like myself, who have often to decide on what shall be done here or what shall be done there, are obliged to leave our decision dependent on the result of experiments. If at the present moment I were asked to design a system of distributing in any particular neighbourhood from a central station, I should first of all consider the compactness of the neighbourhood. Suppose it were a case, for instance, like the City of London, I should, without the smallest hesitation, say that in a close-ringed neighbourhood like that the best means of serving it would be the Hopkinson-Edison three-wire system. If you give me a large district like the district that I reside in—Wimbledon—where a central station must be down by the river's brink far away, and where your mains will go up to a distance of ten or twelve miles, I say, with our present experience, with results of what has been done elsewhere, I should have no hesitation in recommending the system of secondary generators. But that does not prevent or exclude me, while the transformer see-saw, as I said the other night, is up in the air, from keeping my opinion and my mind open on the question of secondary batteries. As you know, I am a great advocate of secondary batteries. Mr. Gordon, at Whitehall Court, is carrying out a gigantic experiment in this direction. Until we know the results of that experiment,—until we know the results of the general distribution of electricity by batteries to be perfectly practicable,

perfectly feasible, and, I am not quite sure, perfectly successful,— Mr. Preece.
until that success is assured by practical experiment,—I, as a
practical engineer, will withhold my opinion upon them, and give
it now in favour of secondary generators.

Sir DAVID SALOMONS: Mr. President and gentlemen,—I must Sir David Salomons.
apologise for speaking at this early stage, but circumstances
compel me to do so. It is said, I believe by Talleyrand, that
every man ought to leave a son, plant a tree, and read a book.
I have had the good fortune of having done all three; and it
is the third, the book, only which interests us here. I mention
this because, having published my views, I will try and avoid
reverting to my methods and views afresh, and taking up your
time unnecessarily.

I completely approve of Mr. Crompton's plan of lighting by
means of batteries, but I think in naming his paper he has put
the "cart before the horse:" it should be "Accumulators *versus*
"Transformers." Mr. Kapp, on the last occasion, took objection to
Mr. Crompton's estimates at which accumulators could be pro-
cured. I have examined a large number of price lists of leading
manufacturers, and from experiments made at home I find that
the figures are quite within the mark. I was pleased to see that
Mr. Parker, who spoke at the last meeting, also acknowledged
that £16 per kilowatt was not at all an unreasonable price. The
efficiency was placed at 60 per cent., but 70 per cent. might very
fairly be taken as a reasonable amount.

Mr. Preece mentioned his experience of the paste falling at
the rate of 1 lb. a year. I think that is very high. Since last
week I have been examining some cells about eleven months in
use, and find that there was not 1 oz. of paste lost; and in some
old ones, which have been in use for three years or more, there was
not so much as 1 lb. loss; so that the probability of their lasting
many years is far greater than people imagine, and is entirely
due to the fact that the management of a battery is better under-
stood, and that the old rule never to overcharge, which was
upheld and acted on by all authorities, has become a dead
letter, with the result that the plates can now be kept in good
order.

Sir David
Salomons.] The variation of 4 per cent. in the distributing mains, allowed
by Mr. Crompton in his paper, is far too large.   I find—and
probably it is within the experience of all here—that even 2 per
cent. is very considerable, unless the lamps are run very high.

I am sorry that he has made no reference in his paper to two
great attempts which have been made to carry out the very thing
that he now wishes to do on a large scale.   One ended in failure,
and the other has only passed the printer's hands ; still the matter
was put forward, and anything that has had a trial, or been
brought before the public to enlighten it on the subject, I think
deserves some credit.   This omission may be an accident.   I refer
to the B.T.K. system, tried at Colchester, and the E.P.S. pamphlet,
which contained a good deal of information for those who intend
spending money in these matters : I say " spend " because it is
most probable that the promoters of early installations will never
see a sixpence back.

One of the chief differences between transformers and cells—
and I am not so bigoted as to say that transformers have no use
whatever ; they are probably invaluable in certain places—to use
a very familiar comparison, is very much like having a gasworks
without a gasometer.   Even in the case of the Grosvenor installa-
tion it has been my lot to hear from, I will not say the promoters,
but those behind the scenes, of the wonderful satisfaction that it
gives to customers.   I have also seen numerous customers, and
have heard their opinion, based on experience.   I have also seen
the lights jump about and go on in a manner that would probably
injure the eyesight in time ; so that I can speak honestly that
the public is not satisfied beyond the novelty of the light.   I
know of one case where only some half-dozen lamps are installed,
and these frequently go out for a few seconds and then light up
again most brilliantly, but on the return of the light some one or
more lamps had been destroyed.   I think it an advantage to make
this public here : it is between ourselves.   It is a great pity that
the electric light promoters will not look to their own short-
comings and remedy them, instead of praising everything up in
order to draw money out of the public, creating distrust in the
cause they advocate.

There is one thing I want to call your attention to which Mr. Sir David Salomons.
Crompton seems to have overlooked.   He bases his estimates upon
the curve he has placed before us, which is probably approximately
correct; but he entirely overlooks that the day will come when
motors will be used in every house, and that curve will then be
completely altered, and the possibility of a far greater output,
which will be paid for, will then be accomplished; and if that
curve does not become a straight line like that of a good dynamo,
at any rate it will be a fairly steady one, the output being double
or treble that now assumed, and this will alter the estimate
considerably.

You will forgive me mentioning an experiment I made, and,
having no interest in any lighting company, I do not mind
mentioning it for the benefit of those who wish to experiment on
the subject for themselves.   By a simple apparatus I converted a
direct current into an alternate one, and took an ordinary shunt
motor, which I was able to rotate with alternate currents; the
only difficulty was its enormous heating.   I mentioned this to
Mr. Elwell, who was at that time in the firm of Elwell-Parker,
and requested him to try the experiment on a larger scale for me,
and to alter a motor in accordance with some suggestions that
occurred to me in order to carry out the idea; and it was proposed
that the magnets should be made with wire, so that the reversals
should not heat them.   I am going to try the experiment again,
removing the iron of the magnets completely.   Of course the
efficiency will in this case be very low; but I think it is worth
while for any gentleman present to try the experiment of taking
an ordinary shunt motor and altering it in this way, and to put
an alternate current on without being afraid of the result.   As far
as I have been able to ascertain, no one has before had the idea
of making the experiment, and it is sometimes by trying very
absurd things that you get at a very rational result.

Another point bearing on Mr. Crompton's paper that I would
mention is the absolute necessity of placing batteries used in a
central distributing station in parallels of two or more, in order
to prevent all possibility of a breakdown.   Recently I altered the
installation at my house from a large series battery of cells placed

Sir David
Salomons. two and two in parallel to two batteries placed in parallel.   By
this method you have the advantage that if anything is done to
one battery the other would be working, and the circuit is always
supplied with current.   An accident happened the other day
which might occur in any installation, and which proved the use
of this arrangement.   It was a good thing that it did happen,
because it brought a new point to light which was not thought of.
One morning, on entering the battery-room, I found that one of
the cells had broken and the acid had run away, so that the
battery containing the broken cell was cut out, but the second
battery prevented a breakdown ; and whether this had happened in
a private installation or in a public one depending on one series
battery, it is perfectly evident that a breakdown would have
occurred, notwithstanding the talk about the certainty of accumu-
lators against such chances.   I mention this to enable such an
eventuality being taken into account.

Mr. Kapp raised a question as to the regulation—a point which
has been entirely passed over by Mr. Crompton.   My own belief
is that some kind of regulation is essential.   The suggestion on
the part of Mr. Kapp of cutting out certain cells—i.e., including a
less number of cells between the distributing lines—the whole
battery charging during the time, might be practicable, but not
unless a very large excess of current were passed through the
excluded cells ; and I understand that the Howell cell will allow
such an excess of current without being injured.   I have not seen
that cell myself, and I think that very few people have had that
privilege.   Before long I trust that I may receive an invitation
to see an installation fitted with it, and have an opportunity of
judging for myself.   The method that I have always adopted is
more scientific as well as convenient—that is, of charging all the
cells equally and putting a counter electro-motive force in the lines
to reduce the pressure to that required for the lamps ; because you
must waste this extra E.M.F. when current is going into the cells.
You cannot get over that ; it is only a question of the best way to
do it.   I believe the counter E.M.F. method is the cheapest and
simplest way, because an inexpensive regulator can be used to put
in and out the source of counter electro-motive force as require-

ments may demand, and in this way you can work to within one
per cent. of pressure variation on the distributing lines.

I have only two other words to say. One is that Mr. Preece
has placed before you his credentials on accumulators, and I under-
stand him to say that he has destroyed more cells than probably
any man living.

Mr. W. H. PREECE: Destroyed! No.

SIR DAVID SALOMONS: He has, perhaps, destroyed all except
those he has kept for his own house, or he intended saying "ex-
"perimented upon." There is only one difference between that
gentleman and myself. The·chances are—I do not say this in
any insinuating way—that he has not paid for those cells, and does
not intend to. Although manufacturers have treated me very
well, still I have had to pay something for the cells, and the
result has been to establish once more the old story that
"necessity is the mother of invention." I have not only
destroyed a large number in gaining experience, but I have been
able to find a way of renovating them; and it may be truly said
that any cell under the sun, however bad it may be, can by a
suitable means be restored again for an expenditure of one per
cent. of its original cost.

I will make no further remarks, although there is a great deal
I should like to say. But brevity has been asked for, and I
respect the President's wish, and will now leave the discussion to
other members.

The PRESIDENT: Before I call upon the next gentleman who
wishes to take part in the discussion, I may mention that Mr.
Crompton wishes to add a few words in explanation of a further
diagram which he has introduced.

Mr. R. E. CROMPTON: The remarks of several speakers suggest
to me that my paper is incomplete on one or two points. For
this I must apologise, as I did not wish to unduly lengthen my
paper. But as evidently so much is going to be said on the
subject of regulation, and as possibly a few words from me at
this stage may prevent several speakers from repeating the same
question, I think it better to clear up this question of the mode
of regulation I employ without further delay. The regulation

of groups of batteries placed in series is a very simple matter, and has been carried out successfully at Vienna and elsewhere in the manner that I am now about to describe to you. The diagram now before you represents four battery stations arranged in series as described in my paper. If we suppose that the current enters at the left-hand lower corner, passes through the first battery with the lamps parallel to it, passes from the end of that battery to the commencement of the second, and so on to the third and the fourth, and that this current is kept constant or varied at will at the central station by means of adjustable electrical current governors, we have to see how we can keep the potential constant in each of the four groups of lamps shown. In each accumulator one terminal is permanently connected to one of the discharge mains and to one of the charging mains. The other terminal attached to the other mains can be shifted from cell to cell, according to the E.M.F. required in the corresponding lamp circuit, by a movable contact forced through a series of contact rings. I have endeavoured to represent this movable terminal by the bunch of lines shown at the extremity of each of the accumulators. As a rule, the terminal for charge and discharge at this, which I call the regulating end of the battery, neither receives nor gives off current, so that there is no loss of energy
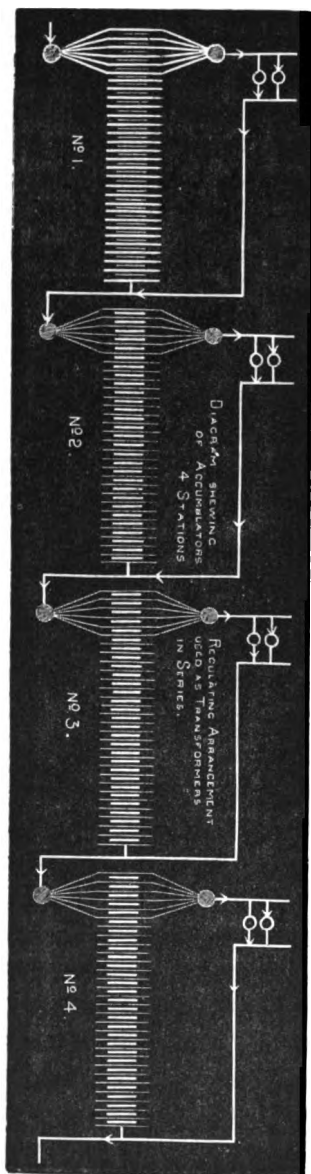
in the shape of E.M.F. The contact rings fixed on the Mr. Crompton.
slate base now before you formed the first set of this ap-
paratus I made.. This set has been actually used for a year
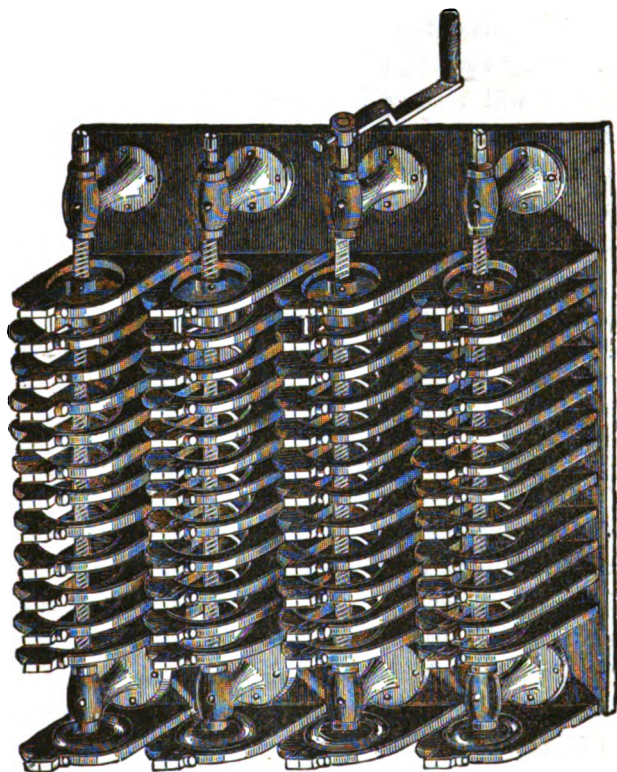
at Kensington, and has been only lately replaced by a larger
ring contact regulator of the same type having double rings.
The apparatus requires little further explanation. You will see
that the ring contacts are arranged in line in such a manner
that a circular ring contact made of thin sheets of copper can
be forced through them in turn by a central screw spindle.
The double form which I now use is convenient, because we
can, whenever we desire to fill up the end cells, charge into a
larger number of cells than we discharge from ; at such times
only is there any loss of E.M.F. in the regulating apparatus.
The two forms before you have been for some time in use, give

Mr.
Crompton.

no trouble, and can be worked by any labourer or youth who can turn a handle. The arrangement for coupling up to the apparatus is very simple : the mains for charge and discharge are attached to the fixed disc contacts on the central screws ; the regulating cells of the battery are coupled to their respective contact rings by sockets, which you will see at the back of the board. You will readily understand that apparatus of this kind lends itself to automatic regulation, and Mr. Willans has promised me that whenever I require it he can give me a governor to operate the rings automatically, and it is evident that he will not have any difficulty in carrying this into effect ; but hitherto we have preferred that the accumulator attendant should work these accumulators by hand. The changes that take place in the course of the evening are not very frequent, and as these occur with fair regularity at fixed periods it is not necessary that the attendant should be at hand the whole evening. There seems also to be a desire that, in spite of the remarks I made at the close of my paper, I should enter more fully on the subject of accumulators themselves, more particularly describing those used by myself. I did not think this desirable, as unnecessarily extending the scope of the paper. I have already said that the large central station at Vienna is worked by E.P.S. accumulators, with the construction of which you are familiar ; to this I may add that the central station in Le Mans is worked by Schenek-Fahrbaky accumulators manufactured by Messrs. Getz & Odendall, whereas the one at Kensington is worked by the Howell accumulators manufactured by my own firm. The latter have been at work for sixteen months, and I have never refused admittance to anyone seeking to see them or the generating station itself. I think that this ought to be sufficient answer to those who have stated that the Howell accumulator is an invisible one.

Mr.
Swinburne.

Mr. J. SWINBURNE : I would like to point out how valuable Mr. Crompton's paper is, and that its value largely depends on the data being gained by experience, and not assumed paper calculations ; but the Society's time should not be wasted in compliments and platitudes.

Mr. Crompton appears sceptical about running alternating
machines in parallel, and, as far as I can learn, he appears to
have got that scepticism from me. The question of coupling
alternating machines in parallel cannot be discussed generally;
it depends entirely on the machines. Some of these can only be
coupled in parallel when doing no work, such as the Ferranti.
Other types can be made which will go together perfectly with
large variations of load without getting out of step. The West-
inghouse is about the medium, and those run very well in
parallel.

There is a diagram on the wall, on the upper right-hand
corner, in which is shown a method which will prevent machines
getting out of step, even if they have not been constructed
properly. The numbers 1, 2, 3, 4, are supposed to be diagrams
of alternating-current machines. There is a switch to each, and
the top and bottom lines are the mains. The first machine
(No. 1) is simply put direct on the mains. The second machine

Mr.
Swinburne.
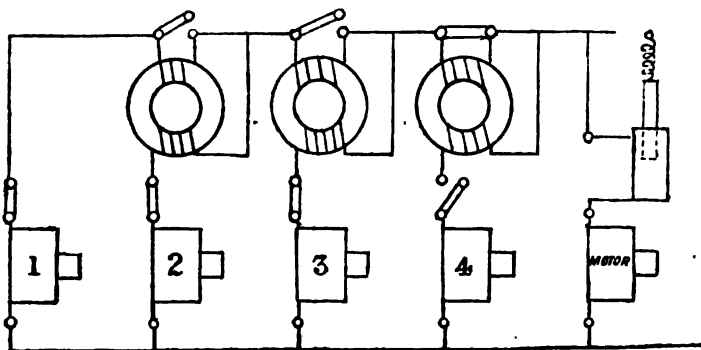
works through a device like a transformer, the wires of which are
wound so that when the machines are perfectly in step the core
is not magnetised at all; but immediately they begin to get out
of step the transformer, or controller, stops them. The third
machine is put in the same way, the controller being specially
designed with the right proportions of wire. By this means any
number of machines can be put in parallel. Even Ferranti
machines will do, and will work with large variations of load.
In fact, the steam can be shut off one engine without putting it
out of step, which could not be done in the ordinary case. The

Mr.
Swinburne. question of keeping the machines in step to start with is very
simple.   A great many methods have been brought out, especially
in America ; but the problem is so simple and so generally known
in America that it is not worth while going into the matter
further.   The controller arrangement may be arranged otherwise.
You may take the machines in pairs : between the first pair there
is a controller ; two pairs are then coupled through another con-
troller, and so on ; the total number of controllers comes out the
same in both cases.   The problem of running alternating-current
motors, once you have got them started, is very nearly the same
as running dynamos in step and varying the loads, except that
the variation in load is, so to speak, very much more important.
I have shown, therefore, a motor which is supposed to be driven
off the main to the right, marked " Motor."   In that there is
what I think Mr. Gordon calls a choking coil, except that the
core is movable.   Directly the load comes on that motor from
power being put on the belt, it tends to get out of step (a
Ferranti will begin to get out of step, and a lot of power will be
spent in its armature) ; immediately that happens, the core of the
choking coil is sucked down, in spite of the spring, and this
checks the tendency at once.   In that way a motor can be worked
with very large differences of load, which will be very difficult by
any other means.

As to the starting of motors.   A great deal has been said about
the difficulty of doing that.   I do not believe the difficulty really
exists.   There are many ways of designing motors which are self-
exciting, which will start themselves ; and one very simple way
is to make the exciter itself with a laminated field, which Sir
David Salomons has mentioned as a doubtful arrangement,
though there is no reason why it should not be done.   The
machine and exciter are then started by passing the alternating
current through the exciter until the speed is attained, and then
the connections are switched over suddenly.   The reason why
motors are not made like direct-current dynamos, with laminated
fields, is that the motor will have to be very large to give an
output ; that is the only objection to such machines.   This does
not matter in the case just explained.

Mr.
Swinburne.

When we deal with conductors carrying very large alternating currents with very small period, we cannot do either with solid rods or what are ordinarily called cables, in which each wire is always the same distance from the centre; we have to use cables made up of little ropes, so that if all the wires are followed along their length their average distance from the centre is the same. That will, of course, add to the expense, but it is a point worth mentioning, because I do not think it is generally realised. Of course the time for considering whether it is worth while going into this depends on the alternations and current that the cable will have to bear.

Much has been said as to the efficiency of transformers. I do not think it is really the point whether you lose 1 per cent. of your power in the transformer or 2 per cent.; the point of importance is that you want your lamps to run at the same electro-motive force, and you cannot make an alternating transformer compound up. You can make a dynamo compound up for your loss; and the only way that can be got over is by making the whole installation compound up half-way. I do not know who thought of it first, but it is well known among practical men. Supposing you have a 100-volt installation, at low loads you put, say, 98 volts on the terminals, and at high loads you put 102 perhaps, and if the people all turn up their lamps at the same time, of course they go all right; but you will have to allow for the few who have only a few lamps on, and these few lamps get burnt rather high, and people with heavy loads get burnt rather low, but on the whole you strike a medium as nearly as possible by that means; and I think that later on, when lamps become cheaper, as I hope they will soon, that will be found very important.

As to the regulating. At present the method of regulating is to alter the exciting current of alternating dynamos. I believe, however, that there is no difficulty in making alternating dynamos really compound up without any alteration in the field. Messrs. Crompton & Co. are making a large machine which is calculated to give lower mean volts when the current increases, but to give higher virtual volts, because the curve alters so that the area with

Mr
Swinburne.
the ordinates squared is greater. The curve is peaked towards
the end really.

I do not think we have very clear data as to what insulation
will stand with alternate currents.   I do not think it at all follows
that you need the same insulation with alternating currents,
because your strain is varying in direction ; and it is quite
possible that an insulation that will break down under 2,000
volts in one direction will not break down under 2,000 volts
alternating.

Of course the difficulty in comparing Crompton's system and
Kapp's system, or other systems, is that people always talk about
different things.   The alternating-current machine and the
transformer system is perfect, of course, if you want to distribute
to one little installation in one place and another installation in
another place, all the lamps being scattered about in very small
groups ; but when you reduce it to local sub-stations it becomes
another matter altogether.

Mr. Crompton's difficulty, I think, will be in introducing his
400 or 500 volts into the houses.   I am afraid that will be a great
trouble.

As to accumulators, I do not quite know what to say.   I would
rather like to quote Mr. Mordey's dictum : " The great advantage
" of an alternating-current system is that people cannot stick
" batteries on it."

It must be remembered, in discussing the question of alter-
nating machines, that we have been making direct-current
machines on scientific principles for some years, and are only just
beginning to do it with alternating-current machines.   Even now
alternating-current machines can be made to run at quite slow
speeds, giving electrical efficiencies of about 98 per cent., and I
believe a commercial efficiency of about 95 per cent.   I also
believe that the alternating machine will be much cheaper than
the direct-current machine, and that should be carefully borne in
mind.

Turning to Mr. Crompton's system, I cannot help thinking
that the real solution of the problem lies, not in having 400 or
500 volts, but in the use of what may be termed " dynamotors."

They have the doubtful advantage of allowing accumulators on Mr. Swinburne. the system, and several others. You can do everything you want with alternating-current machines, except that you have brushes to look after—a small thing, as they do not spark. There is a man to look after the accumulators, and it is nothing for him to look after the brushes. The dynamotors can be made to compound in the secondary, even with a varying electro-motive force in the primary. That may not matter much when working with sub-stations, but when working large districts it becomes very important. If the batteries are charged in the local stations on the primary circuit, the dynamotor may be run on to the secondary. I do not know whether that will be an economical arrangement, but it will need no regulation, because the battery will vary and the secondary E.M.F. will be quite constant.

As to safety. There are several ways of guarding against accidents. It would be better in many cases if an installation had an arrangement by which an earth contact was made momentarily, first with one terminal of the dynamo and earth, and then with the other and earth, by a little apparatus, and then, if there is a leak, a current works a little electro-magnet and rings a bell and disconnects the circuit and leaves the bell ringing. It is really a little automatic contact fixed on the engine-shaft, which first touches one end of the installation, then the other end, and then the middle. The result is that if there is one leak anywhere it will do no harm, because it will be immediately found out. If a man happens to touch the other end of the circuit he will of course get a shock; but if he touches the other end of the circuit before you have got your breakdown, and makes a contact himself, he will get an instantaneous shock, and then this apparatus will short-circuit, so to speak, round him, so that he cannot possibly get anything further. With alternate currents a little wheel is needed making contacts and synchronising with the dynamos so that the currents are in the same direction, so as to avoid what might be called "capacity shocks."

Major-General C. E. WEBBER, C.B., (Ret.) R.E.: The one Maj.-Gen. Webber. point to which I wish to draw your attention is the system by which Mr. Crompton proposes to lay down his underground

Maj.-Gen.
Webber.

conductors. We have not had with his paper a diagram of the construction of his subways, but we had one accompanying Mr. Mackenzie's paper. We thus know that Mr. Crompton proposes to make subways with an interior cross section of about two superficial feet, and to suspend from the roof of such subways insulators to carry his bare conductors. I do not know whether the construction of these subways includes any system of making it comparatively watertight, but I can hardly conceive that that would be possible. Brick walls, a concrete floor, and flagging to cover the trench would certainly not be, in any sense of the word, watertight. I can readily believe that for the first year or two not much trouble, as regards the insulation of his conductors, would arise ; but having myself done a good deal of work in my life in connection with telegraph underground lines, and having inspected a large number of sewers and subways, I feel sure that before two years had elapsed his conductors and their insulators would be covered by detritus of various kinds. The brickwork at the sides of his subway would begin to wear away; the coverings and floors would probably leak extensively ; water from time to time, supposing, of course, that the floors would always be on an incline, would pass along ; insects would overrun the insulators; and in various ways the state of his conductors, which are inaccessible unless the footway is taken up (with due submission to the views of our friends), would, I think, become such that the insulation would disappear. Again, one can readily imagine that these nice little roads under the footway would become a sort of Rotten Row for a large population of rats—an additional complication that might give rise to awkward results. It is perfectly clear that the copper is uncovered, but it is also clear that without taking up the roadway no means exists of obtaining access to it, except at the flush-boxes or other openings. Under these circumstances I hope that if electrical engineers wish to follow this plan they will first look to the many years' experience of the telegraph companies, who spent large sums of money in laying down conductors which afterwards they had to take up from beginning to end, I being, amongst others, engaged in doing a great deal of that work.

I regard the maintenance of underground conductors as Maj.-Gen. Webber. only possible when they can be withdrawn with ease. The " drawing-in and drawing-out " system is so well understood that I need not enlarge upon it; its advantages are perfectly evident to every engineer, and therefore I need give no description of it now.

As regards a few of Mr. Crompton's calculations, I should like to make one or two remarks. That gentleman meets us by a most clinching argument in favour of all his calculations—that he will do the work himself at any time for the money. Of course that is a very good and a very complete argument; and I think that when he estimates his means of distribution on the large scale of the estimate which he has given to us, we must all feel that, from the point of view of the proportion it bears to the total of his estimate, he is not extravagant. When we look at that great map of the London Hydraulic Power Company and consider that little or no difficulty has been found in finding capital for the distribution of motive power which is infinitely more costly in every way, and represents a very much larger sum to do a work of a far less popular, a far less interesting, and we believe a far less lucrative purpose in the end, we must feel that Mr. Crompton's calculations should be very encouraging to investors.

Allow me to remark on one or two of those figures. Mr. Crompton, I see, allows 20 yards of main for every house, or 20,000 yards for 1,000 houses. I think that figure is susceptible of some correction. Although it is a comfort to find an estimate laid before us with so large a margin, anything that will reduce it will be an encouragement. In a district with which I am perfectly well acquainted, where the streets are wide and where the frontages of the houses are palatial to some extent, I have found it to be certainly well within 17 yards. Of course that would reduce his 20,000 yards to 17,000 yards.

This, then, brings me to the main feature—that of the chief economy which he claims in his underground system, and considers, no doubt, a very tempting bait; that is, the saving which he makes in " insulation." It is not clear, without knowing

Maj.-Gen.
Webber.
the details of his calculations, what he includes in his figures; but I think it is clear that, if you just deal with the example in which the cross-sectional area of the copper is half an inch, you will see that in one case he makes the insulation cost £35 17s., whereas in the other—that is, his own subway—it is only 4s. 6d. That is, of course, a very attractive figure, but it is not quite a safe one, I think, because the 4s. 6d. is too small, and the £35 17s. is certainly too large.   He gives the cost of the copper in the first instance at £18 15s., or £54 12s. for the insulated cable, which, divided by two, makes £27 6s. for the 100 yards. It is also very evident that this would not cost more than £22; however, as I said before, a good margin in such a calculation is a most desirable thing.   This brings me to the question of the system with which he makes comparison, namely, the Callender-Webber system ; and as my name has been mentioned in connection with it, I may be allowed to say a few words about it. I might add that personally my interest in it is purely of a scientific character as an engineer, and, so far, unremunerative; and my opportunities of trying it have been entirely due to my connection with the Anglo-American Brush Corporation, in whose yard at Borough Road it was exhibited during a period of over six months.   The use of the bitumen concrete, I am glad to say, has been adopted to a certain extent by Mr. Crompton in his installation at Kensington Court.   Every instance in which it gets fair play and is properly used will bring to the front several of its conditions which I believe to be invaluable.   You all know what it is.   The essential feature of the system is that of being able to withdraw the conductors when desirable.   Whether it will succeed or not remains to be seen, but there are advantages in its use which (that is to say, in the use of the tube or casing) are paramount as against the subways.   The example of which I spoke showed that in laying this down it could be bent with great facility on the spot so as to meet all the varying conditions, which are certainly not so great in the general streets of London as they are in the City.   But if you take the City as an example, although it is an exceptional one, it would be impossible for Mr. Crompton's subway to be put down.   Other systems have

been described elsewhere, and it is not for me to say anything Maj.-Gen. Webber. more. I will put in my remarks, without troubling you to hear them read, some tests and calculations which Mr. Callender and Mr. Butler were good enough to make for me, showing the breaking strain, crushing weight, specific gravity, and other data connected with the concrete casing; and I think that when they are seen and read by those who will receive the account of this evening's meeting, they will be so surprised at the results as to afford much encouragement to further trials of this arrangement.

TESTS OF "CALLENDER-WEBBER" BITUMEN CONCRETE CASING FOR UNDERGROUND ELECTRICAL CONDUCTORS.

Test for Breaking Strain.

A case 6 feet long and 8 by 5½ inch section, made about six weeks previously, having six 2-inch ways and half an inch of material between the ways at the nearest point of section, was supported at two points 3 feet apart, and weighted in the middle of the narrowest face.

Two trials were made, as follows :—

	1st Trial. lbs.	2nd Trial. lbs.
First indication of strain	2,124	2,130
Final break	2,306	2,457

Temperature, 50° Fahr.

The break was sharp and clean, the ways not having lost their shape.

Crushing Strain.

A case similar to the above was subjected to the crushing test required by local boards for cast-iron pipes, namely, of 3,300 lbs. This it withstood, and, further, did not show any effect when subjected to a crushing weight of 3 tons; and only when the weight amounted to 10,738 lbs. was any crack or flaw visible.

A 6-inch solid cube of the material was also tested, and was not in any way affected by a crushing weight of 8 tons.

The specific gravity of the material is about 11·73.

Mr. SYDNEY F. WALKER : I take it, Sir, that the object of this Mr. Walker. paper, and of the one that was read a few weeks ago, is to help us in the onward progress that we are hoping to make with electric

Mr Walker. light installations. We are all leading on to larger and larger installations, and therefore all these discussions on transformers *versus* accumulators help us very considerably by the side-lights that they throw even upon installations of less than 10,000 lights. I think, in the first instance, that before either transformers or accumulators can help us on this road, progress must be made with the lamp itself. If Mr. Swan, or the gentlemen who are working with him, will produce a 200-volt lamp, everybody here knows that it will immediately quarter the cost of the copper in the cable itself; and if they will produce a 400-volt lamp it will quarter it again. I think I am not exaggerating when I say that if a 200-volt lamp is used—which, I take it, is perfectly safe; there will be no danger in using it in the houses—and if at the same time they will reduce the cost of the lamp and increase its efficiency, they will do more for the progress of electric lighting generally—for central station and for every other kind of electric lighting—than any system of transformers or accumulators can possibly do.

With regard to the point mentioned by Mr. Preece about the various voltages, so far as I understand the question I am afraid it is unavoidable. I think he will have followed the matter in the same way as I have. In the early days we had only 48-volt lamps: the company could not make a lamp of a higher voltage; they did make a few, but they would not stand. Then we got to 60 and 65, then 80 and 100; and I hope they will get by-and-by to 200, as I have said. But, I take it, it is merely a question of time, and that the different voltages that are in use are merely the result of the progress that has been made, and that we cannot well help it.

As the matter stands now I certainly think that Mr. Crompton has taken the right line, and for this reason—I have gone into it very carefully—that, as he provides an attendant at the accumulator stations, you can have an absolutely efficient service if you will pay for it; and I do not see that you can have this in any other way.

You cannot be certain of uninterrupted working with the transformer system, because you are never quite certain that your

2,000 volts will not get through into the secondary circuit. Mr. Walker. Those of us who have studied the matter know perfectly well that the accumulator is wasteful, that it feeds on itself, that its plates go on forming more and more active material, and that they are expensive. It may be very doubtful whether Mr. Crompton's 15 per cent. for depreciation is sufficient; but after you have said all that, you come back to this—that you can have a practically · certain supply if you will pay for it; and my experience is that the first thing that you have to study is what we understand in commercial life as an efficient service You have got to make the thing work properly, and if you do that you will always get paid for it. There will always be a sufficient number of con- sumers to pay you for going on, but they will not put up with breakdowns every now and then, such as Sir David Salomons described to us. You may hold forth to a consumer that you have 99·9 per cent., and he will be very much interested; but if his apparatus fails—if he has his light out for five minutes every now and then—he will vote that it is a marvellous nuisance. Again, one great objection that, I take it, exists with regard to transformers is that you have this 2,000 volts to deal with; and —I say it without fear of contradiction—before you can deal properly with a service using 2,000 volts, to take it into what I may call everyday life, in the same way that gas is taken, you must first face the problem of providing efficient insulation for that 2,000 volts. I do not think that any cable or any dynamo at present in existence is properly insulated for 2,000 volts. It is not a question of being insulated for to-day or to-morrow, or this month or next month; it is a question of being insulated practically for all time—for as long as the thing is to work. Again, I think Mr. Crompton is quite right in pointing out that the difference in cost between the heavy cables which must be used with low-tension currents and the lighter cables that may be used with higher-tension currents is not so great as it seems, if it is not actually on the wrong side. If anybody will take any cable manufacturer's price list, and compare the light insulation— which is, of course, intended only to avoid actual contact—with the heavy insulation which would be used for underground work,

Mr. Walker. he will find that the cost of the heavy insulation is ordinarily double that of the light insulation, the copper being the same; and that heavy insulation is not an insulation for 2,000 volts. So that you will want something very much more than that when you come to underground work with 2,000 volts, with the cables close to each other, and with all the danger of getting connection with your secondary circuit. With regard to Mr. Crompton's plan of suspending his cables naked in troughs underground, the same point struck me that occurred to General Webber, and as he has dealt with it very fully I need not enter into the matter, except that I would point out that I think a similar plan for laying telegraph wires has been introduced in this room a great many times. I remember such a plan being introduced some fifteen years ago, in which the wires were laid in a trough and asphalte run in; and the great objection was that if anything happened to one of the wires it was lost entirely. Of course a No. 18 or No. 16 telegraph wire is not on the same footing as a cable of 37 No. 10 wires; but you will have leakage and other things, and you have your large currents and your comparatively high voltage, so that you will be very much, I think, in the same state if anything goes wrong; and I would suggest myself—I give the suggestion for what it is worth—that in place of using very large cables it would be better to use a number of smaller ones, so that, as you intend to use a number of smaller dynamos and smaller engines, each dynamo can be connected to its own cable instead of delivering into the mains.

My experience with continuous-current dynamos working in parallel is that they do not work properly together. One dynamo, if it happens to be half a volt in advance of the other, will take a very much larger current than its fellow, supposing two are working together, for the simple reason that the second one cannot deliver until the first one ceases to compound.

The President. The PRESIDENT: Mr. Robert Hammond will perhaps tell us something about the Eastbourne installation?

Mr. Hammond. Mr. ROBERT HAMMOND: I am very much obliged to you, Mr. President, for the invitation to tell the Society something about Eastbourne, especially upon the point that has been raised

by Mr. Crompton in his paper, and which has been commented
upon by other speakers. He invites discussion of the one prob-
lem which confronts, or has recently confronted, all those who
have had to do with central station work. Eastbourne has been
mentioned, and I may explain, Sir, that in connection with that
installation we had some two years ago to light the Town Hall.
The problem was to put some 300 lights into the Town Hall on the
condition that any number of the lights could be turned down at
any time, 10 left burning, 20 or 30 in the committee rooms, or the
whole 300 burning at once. It was impossible to carry the work
out on the plan that we then had in operation, and we had to
consider whether it was best to put batteries into the Town Hall
and charge them with our high-tension current, or to put down con-
verters or transformers. Two years ago, as you in this Society
know well, the choice of transformers was much limited, and,
having carefully considered the question, and having decided
upon transformers, our engineer, Mr. Lowrie, had to practically
make his own transformer. He did so, with the result that the
installation in the Town Hall at Eastbourne is an eminently
satisfactory one, and one that strongly appeals in favour of the
transformer system. I view this question, as many of you know,
from the commercial point of view rather than the scientific, and
I look to such leaders of the profession as Mr. Crompton for
guidance; and I must say that on reading his paper I was
distressed to find on its first page that the first fact with
which he enlightened the Society was that the installation at
Eastbourne, which has been in operation for some six or seven
years, and that at Brighton, where Mr. Arthur Wright is the
hard-working managing director, were not honest attempts to
carry out permanent lighting, but were "confessedly carried
"out in a temporary manner, for the purpose of popularising
"the electric light and inspiring the confidence of the public
"in supply companies as a form of investment." This appearing
in a scientific paper surprises me, who do not pretend to be
profoundly scientific. Our balance-sheet at Brighton which was
issued some weeks ago shows our position; and if Mr. Crompton
had taken the pains to get the list of shareholders he would

Mr.
Hammond. have been prevented from writing what I cannot help thinking was a piece of unkindness, because in the preceding paragraph he says, "It is hardly to be wondered at that what little "knowledge exists on this subject should be confined to the "few who have lighting stations under their immediate charge;" and yet in the next paragraph he practically says, "Do not take "any notice of Eastbourne, though they have been carrying out "an installation for several years, nor Brighton, nor the Grosvenor "Gallery, because they are only a set of advertisers." That being so, I presume that in getting together the facts for his paper he has taken no notice of them; and therefore, if I may trespass upon your time for about five minutes, I will tell you one or two facts connected with these two central stations which I do not think you will gather from Mr. Crompton in his reply. In the first place, we are working with transformers at Eastbourne and in Brighton, on the group system ; it may be on a small scale— some 2,000 to 2,500 lights—but we have learned at Brighton the fact that in starting a central station one has to be contented with doing scattered lighting, and to gradually secure additions from intervening houses.

Taking up Mr. Crompton's arguments in detail, I will deal first with his load diagram. Mr. Crompton says in his paper that of course in a transformer station you must have three complete shifts of men. Well, I was at Brighton last night. I was there at the busiest time, when the lighting was fully on, when every stitch was going ; and waiting till the lighting began to drop off, I saw one engine after another put off, until there was only one running ; and the labour that was being expended there was absolutely proportional to the load that was required on the circuit : there were no complete shifts of men. I called in at the station this morning on my way to the train at Brighton, and did I find the same number of men working that I did at 7 o'clock last night ? No ; I only found two ; and those who pretend to say that with a direct system it is necessary to have three times the normal staff are talking of what they would know very much more about if they stepped down to Brighton and Eastbourne and ascertained the actual facts.

The last speaker said that the one thing that we all agreed Mr. Hammond.
on was that under a battery system we were perfectly safe and ·
could go to bed at any hour in the night, and could always turn
the light on.   How is this result attained in the battery system ?
What is the reserve force upon which you rely for the light ?
You have a chemical or an electrical storage : that is your
reserve.   Now, speaking in this hall, with many civil engineers
around me, I ask, Which is the better to rely upon—that chemical
reserve, or a *mechanical* reserve which can be thrown on at any
moment ?   I will venture again to appeal to experience, because
Mr. Crompton has thrown all experience aside on his first page,
and says, " We will have none of it."

At Brighton, if the burden of the work is sufficient to keep
only one engine going, one only does the work, but there is a
second one with the steam up, ready at any moment to be
thrown into work ; and, gentlemen, how many of you have had
experience of an engine properly looked after suddenly bursting
right up in the air without giving the slightest intimation that
it was going wrong ?   My experience of engines is that they give
a little warning.   We have heard to-night from Mr. Swinburne
of a nice little arrangement for ringing a little bell.   So with
engines : if they do not absolutely get up and ring a bell, there is
always a little leakage, and the practised eye of the man who is
in charge of the engine tells him there is something going
wrong.

There is plenty of time to put on a second engine, and we
have never at Brighton been so taken by surprise that we have
had a stoppage of the lighting from not being able to rely upon
our reserve force.   Indeed, we consider that the only perfect
method of securing reliable and continuous running is to have
your reserve of engine-power ready to be drawn on at any
moment, and not to put into the circuit these wretched accumu-
lators.

With regard to the efficiency of accumulators.   In making up
his figures Mr. Crompton says 80 per cent. is the output of the
perfect accumulator ; but we find from experience at Brighton,
where we have in a few isolated cases accumulators at work, that

Mr.
Hammond.
it is nearer 60 per cent.; and many of you will confirm these figures. My opinion is that his figures are a little mixed up, and that he has put the efficiency of the converter for the efficiency of the accumulator.

What does Mr. Crompton say with regard to the efficiency of the converters? He says that when the lighting is going on at pretty full swing you get about 70 per cent. efficiency in your converters, and only 30 per cent. efficiency at light loads. I can only suppose that he has not yet seen as perfect a converter as a battery. The battery that gives 80 per cent. must be an extraordinarily good one; but the transformer which is content to give 30 per cent., even on a very light load, must be an atrociously bad one, and I should like to show him converters at Eastbourne which give 95 per cent. efficiency at the full load.

I am glad to see that Professor Forbes is back from America. He will be able to tell us whether in experience good converters with a light load fall from their high efficiency down to 30 per cent. efficiency.

There is only one point more, and it is this: Our experience at Eastbourne is dead against Mr. Crompton's figures in reference to the necessity of putting aside 10 per cent. per annum for depreciation on converters. You do not want to put 10 per cent. aside for depreciation even for dynamos; and when dealing with this silent thing, that is doing no active work, has no moving parts and no chemical arrangements to get out of order, why put 10 per cent. aside for its depreciation and only put aside 15 per cent. on the accumulator?

Well, then, I say that on these four points our experience is dead against the figures put down in Mr. Crompton's paper. It is against him as to the theories drawn from his load diagram, as to reliability, efficiency, and depreciation. I should suppose that a stranger would gather on reading the first paragraph of the paper that Eastbourne had an overhead system. We are told that "Eastbourne, Brighton . . . in fact, all those which "distribute by means of overhead wires," &c.; but Eastbourne is an underground system.

I am very sorry indeed to differ from Mr. Sydney Walker, who

told us that of course the insulation would break down as soon as Mr. Hammond. you attempted to pass 2,000 volts through a cable. Well, at Eastbourne we have some seven miles of main wire underground; we do pass 2,000 volts through it—that is our charging current— and we live through it all. We are delighted to be underground there. We make careful tests, of course. We find no difficulty about it at all, and I am astonished that, in the face of the absolute experience that you have in a place within arm's reach, it should be said here that the great thing against the system is that it necessitates such a high current as 2,000 volts. I must say that, examining closely into the four or five points of this paper which I am able to check by absolute experience of lighting stations, small though they be, I do not place much confidence in Mr. Crompton's facts, figures, and fancies.

Mr. A. P. Trotter : Mr. Crompton has, I think, shaken the Mr. Trotter. faith of a good many of those who rely on transformers as being the system of the future. Many of us feel that he has thrown quite a different light on the matter, and I for one should not like to say that batteries will not be part of that system. But several points have already been raised which show that the time has perhaps hardly come for this. I do not know if the diagrams on the wall represent a station where motors[*] are being used ; but, as has already been pointed out, if motors are used, the daily con- sumption will certainly be as high, if not very much higher than what we see on the diagrams. However, we have not got alternate- current motors yet, and Mr. Crompton has made a great point in speaking of what has been already done as against what we hope to do. Perhaps one of the most serious objections to batteries is that of their efficiency and life ; but when the time comes for battery makers to give an absolute guarantee, and to undertake to repair and keep in order batteries for a sum, say 10 to 12 per cent., we shall be in a different position. People, I believe, will hardly like to sink so much money as Mr. Crompton recommends upon batteries, judging from the experience that we have had during the last three or four years.

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[*] Dr. Fleming has since told me that this is the case.—A. P. T.

Mr. Trotter   As to the efficiency, we know that 60 per cent. is about the
usual thing for a battery that is at one time charged and at
another is discharged.   Mr. Crompton has given us a very different
figure, viz., 80 per cent., and I hope in his reply he will explain
if it is due to the short length of time during which he is really
discharging from his battery that this very high efficiency is
attained.   The question of reserve has been already discussed,
and it seems much better to have separate dynamos with their
separate engines than a battery reserve, unless two sets are used,
and then no doubt troubles come in, and there is a double expense.
If you have six dynamos, 100 kilowatts each, you always have one
or two of them with the steam up and either just ready to start
or just having stopped, and except at the short time of full load
you will always have the opportunity of turning another one on
within a few seconds, and even at the full load there should be at
least one in reserve.   It is most discreditable in this country that
at the present time there are so few installations with a single
dynamo and engine in reserve at the time of full load, and perhaps
as much prejudice has been done to electric lighting on account
of the absence of such a reserve as from any other reason.   In the
case of waterworks, where they have perhaps three engines for
the full load, they have at least one in reserve; and in a central
electric lighting station with six or eight 100-kilowatt dynamos,
with one in reserve there will be ample time for overhauling
purposes during the day.

I would ask Mr. Crompton why he has put down 60 volts for
the lamps at the transformer station.   It makes some little
difference to the calculation; but what is more important, in
working out the comparison, is that he has assumed that two
out of every three houses will at no distant date be wired and
running.   This is rather unfair to the alternate-current system,
because, if we have for some years to come only two out of ten, it
makes an enormous difference in favour of the transformer
system.   Again, in speaking of the very high insulation required
for the high volts, is it necessary, in the face of these most
valuable tables that are given in this paper, to go as far as
2,000 volts?   A great deal has been said about the copper

varying inversely as the square of the volts, and so on, but considering the percentage of copper in the tables before you, its value comes to a ridiculously small amount; and if you take 3,000 volts instead of 2,000 volts, you get about 22 millimètres instead of 50 millimètres for the area, and you only save 22 per cent. of the total cost.   No doubt the proportion of copper is 4 : 9, but still you only save 22 per cent. in your total cost of £11,200, against £14,270; and if you reduce it down to 1,500, which is the point which Professor Elihu Thomson has suggested as the limit of high volts, you get 90 millimètres, which would cost £90 per 100 yards, making £18,000—an increase of 26 per cent. on the cost of the cables according to Mr. Crompton's figures.   I quite agree with Major-General Webber, who has pointed out that the bare copper seems likely to get into difficulties, and I should say much more so than a properly laid cable in a good tube or culvert; and it certainly seems hardly fair to give a higher rate of depreciation for the bare copper than for the insulated cable.

Mr. Kapp has spoken of large transformers which are " banked " on a common network, but he did not say what should be done. When one breaks down partially or entirely a greater load is thrown on the others.   But supposing these large transformers are put down at the present day, when two out of ten houses are taken up, when the time comes for two out of three surely we can afford to get a room in the mews, as Mr. Crompton suggests, and probably the room over the stable can be had when the time comes for exchanging them for batteries and an attendant.   It is quite possible that the time may come when batteries will be used to the exclusion of transformers, and then the only change will be in the dynamos and in the transformers, and these are not a very serious item in the whole; the houses will be ready, and the mains might be arranged so as to be available for either system.

A question has been raised about the difficulty of working with high volts with batteries, and I see no reason why Mr. Crompton should not go further with his high volts and put down yet more batteries, because nothing could be easier than to run several dynamos in series, for we all know that is not a difficult

Mr. Trotter. matter—certainly not half so difficult as to run alternating-current dynamos in parallel.   There should be no reason why in future, when we come to something more like unanimity on the subject, several stations should not be thrown into a general system ; or, at all events, one district could assist another, as is done by the waterworks in the North of England.

The PRESIDENT : Is Mr. Esson here ?

Mr. W. B. ESSON : Yes, Sir ; but as it is rather late I would ask you to consider my remarks as read, and I will send them in to the Secretary.

The President. The PRESIDENT : It is not according to the practice of the Society to allow a member's remarks to be sent as a written communication, unless the member is unavoidably absent, in which case the written remarks must be read to the meeting by some other member on his behalf. It would be better, therefore, perhaps, that you postpone what you have to say until the next meeting.

Mr. W. B. ESSON : I will do so if the discussion is to be adjourned.

The PRESIDENT : It will be.

Mr. W. B. ESSON : Thank you.

The PRESIDENT : As there are still several gentlemen who wish to address the meeting on the subject, and as the hour is getting late, it is evidently impossible to finish the discussion this evening.   I beg, therefore, now to adjourn it until the next Ordinary General Meeting, which will be held on April 26th. Unfortunately Mr. Crompton will not be able to be present then, but I think probably the evening will be fully occupied without his reply ; and as that reply will be of so much importance that to dispense with it would be like representing the play of " Hamlet " without the principal character, we shall have to hear it at a subsequent meeting.

The meeting then adjourned.

The One Hundred and Seventy-eighth Ordinary General Meeting
of the Society was held at the Institution of Civil Engineers,
25, Great George Street, Westminster, on Thursday evening,
April 26th, 1888—Mr. EDWARD GRAVES, President, in the
Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to
suspended.

The following transfers were announced as having been
approved by the Council :—

From the class of Students to that of Associates—
> A. Hewson Bate.
> Arthur C. F. Webb.

Donations to the Library of the Society were announced as
having been received since the last meeting from Professor D.
E. Hughes, F.R.S., Past-President; Dr. J. Hopkinson, F.R.S.,
Vice-President; and Oliver Heaviside, Esq.; to whom the thanks
of the meeting were unanimously accorded.

The PRESIDENT : Before proceeding with the business of the
meeting, I have to call the attention of the members to the
announcement of an occurrence with which, no doubt, they are
all familiar—that of the death of Mr. Thomas Russell Crampton,
who had been for many years a Member of the Council of the
Society. Mr. Crampton was well known as an engineer and
contractor for the construction of railways; and I may call attention
to the circumstance that he was also one of the earliest successful
pioneers of submarine telegraphy, and was the author of the
type of submarine cable now existing throughout the world. I
will ask Sir Douglas Galton to move a resolution.

Sir DOUGLAS GALTON, K.C.B. : Mr. President and gentlemen,—
As a very old friend of Mr. Crampton's I feel a satisfaction in
the duty having been placed upon me of moving this resolution—

"That the President, Council, and Members of this Society have
"learned with very deep regret of the decease of their much-
"esteemed Member Mr. Thomas Russell Crampton, formerly a
"Member of Council of the Society, and who was so intimately
"connected with the establishment and laying of the first suc-
"cessful submarine telegraph cable; and they desire to offer the
"expression of their sincere sympathy with his widow and the
"other members of his family on the great loss they have thus
"sustained." Mr. Crampton, as probably few of the members of
the Telegraph-Engineers are old enough to remember, was a born
engineer. He was connected with the early introduction of
railways; he devised a locomotive engine called the "Crampton"
engine, which to this day holds its place on many of the railways
on the Continent, and Mr. Crampton himself claimed that it was
a most economical form of engine. At all events, it has never
been removed from the Orleans Railway in France, and is still
very largely used on other railways. But the part of his career
which more closely appertained to the interest of this Society
was that of laying a submarine cable. Mr. Crampton, by his
energy, by his great science, by his great skill, was the person to
whom the first successful submarine cable, laid between Dover
and Calais, was due. An earlier line had been laid, which con-
sisted of merely a wire covered by insulating material, and that
was very soon destroyed. Mr. Crampton then came forward, and
both raised the money for and devised the cable which was
subsequently laid in 1851, and that cable was really the original
of the submarine cables which have been since laid. I say "the
"original," because this form has substantially been copied in
every cable which has been successfully laid and successfully
maintained until this day. Mr. Crampton unfortunately retired
from his engineering life and became a contractor, and in that way
his engineering skill, I think, was very much lost to the world, as
he retired literally into almost private life. I would only mention
that within the last six or seven years he devised a method of ex-
cavating the Channel Tunnel, and put this into use in a tunnel
near Sevenoaks, in which he excavated the material by means of
hydraulic machinery, and then used the water which was required

for working that hydraulic machinery in removing the chalk which was excavated. I merely mention that to show how very much his mind was always occupied with engineering problems, and to show the great ingenuity of his mind. I think I will not add any more now, but I will simply move the resolution which I have read to you.

Professor FORBES : In seconding the vote of condolence to the widow and family of Mr. Crampton, I have nothing to add to what Sir Douglas Galton has said, except to endorse—which I can heartily from my own knowledge do, as Mr. Crampton was a most intimate and valued friend of mine—every word that has fallen from Sir Douglas Galton's lips.

The PRESIDENT put the resolution to the meeting, which was carried unanimously.

The PRESIDENT : The discussion on Mr. Crompton's paper will now be resumed. I ought to say that it must close to-night. Mr. Crompton is unfortunately away, and will not be able to reply until the 17th of May.

Mr. W. B. ESSON : I am afraid that previous speakers have to some extent occupied the ground covered by the few remarks I wished to make. The paper of Mr. Crompton ought to be a valuable one, inasmuch as it professes to deal with the facts and figures obtained from actual working; but at the outset I would say that it would have been much more valuable had the author confined his attention solely to figures obtained at Vienna, without giving figures obtained, or rather expected, from a hypothetical installation which has not been carried out. It would be of great interest to know precisely the return which is obtained from accumulators when working on a large scale. As far as I know, the actual energy efficiency has not been shown to be higher than about 60 per cent. Of course we all know that the average accumulator requires to charge it a difference of potential of 2·5 volts, and gives in discharging about 2·0 volts. Assuming that we get the same number of coulombs out that we put into it, the efficiency is thus shown to be 80 per cent. But we never realise this assumption, and it is highly improbable that in actual practice the efficiency is more than 70 per cent., if, indeed, that figure is ever reached.

Mr. Esson. Mr. Kapp has already referred to the small size of the mains which Mr. Crompton proposes to use for distributing from four centres an aggregate current of 6,000 ampères, so that I need not refer to that save to question whether the £20,000 which Mr. Kapp proposes to add to the estimate will be sufficient to cover the extra cost. I shouldn't like to put down the mains for such a system of distribution for the price Mr. Crompton names if I had the Bank of England at my back.

I agree with Mr. Crompton, however, that the cost of the alternating-current dynamos would be much larger than he has given in his estimate. When I saw the proof-sheets of his paper, I ran out the cost of these dynamos, with exciters, and made it about £7,500. Mr. Crompton, it will be seen, has put down £5,540 for them. At the present time alternating-current dynamos cost a deal more to manufacture than continuous-current dynamos of the same output. Probably we may be able to construct them more cheaply by-and-by.

As regards the working expenses, I think there is very little to choose between transformers and secondary batteries. In transformers we have some fully loaded and some lightly loaded, and it may be that we never have, in consequence, a higher average efficiency than 70 per cent. At the same time it is questionable, even when a large proportion of the current is directly supplied by the dynamos, if the accumulator system gives more; and until we have the figures actually obtained from both B.T. and A.T. systems before us, our calculations must partake too much of the nature of a speculation to be admitted as evidence on which a fair comparison can be based.

As regards reliability, here again I think there is not much to choose between the two systems. It is quite true that there is an objection to moving machinery *per se*, but we can work a number of machines and engines in parallel, and with a spare set ready to switch into the circuit at any moment, I do not see that danger to the system as a whole is so be apprehended from a partial collapse at the generating station. I think that a great deal too much has been made of the reliability of the accumulator, but that on the whole there is not much to choose between

accumulator and transformer systems. As far as the generating Mr. Esson.
station is concerned, I should myself place as much reliability on
the alternating-current system as on the accumulator system.

When we come to the question of mains we touch a somewhat
different subject. There are two modes of A.T. distribution now
before us : one is to have a few large transformers and to dis-
tribute the current from them by mains at a potential of 100 volts;
the other is to have transformers in every house, or one for every
two houses, and to have a charging network at a potential of
2,000 volts. Now, when we come to a general system of distri-
bution, where we have a very complicated ramification of con-
ductors, I should view with distrust any scheme which entailed
the placing of a network of mains underground at a potential of
2,000 volts. Of course we have very little experience of the
behaviour of conductors underground fed with such high-potential
currents, but such experience as we have would certainly tend to
make us distrust the result. It seems that if we are to have
safety we must have a few large transformers and distribute the
current at 100 volts, when the cost of distributing mains will
come to pretty much the same thing for both A.T. and B.T.
systems. If there is the same number of accumulator stations
as there are transformer stations, the cost will cancel out, and we
have the same amount on each side. But the cost of the charging
mains will be greater for the B.T. system. Whereas the alter-
nating charging mains are at 2,000 volts, the accumulator mains
are at 500 volts, any increase in the accumulator stations more
than four being obtained by adding on fresh parallels. For
instance, if we required twenty accumulator stations, we should
work at 500 volts with five parallels, having four groups of accumu-
lators in series on each. The cost of the mains for charging the
accumulators would probably be five times greater than the cost
of the alternating-transformer mains. On going carefully into
the whole question, I think it will be found that the first cost is
about the same for both the A.T. and B.T. systems, with the
exception that the charging mains come to more in the latter.

I have before referred to the use of continuous-current trans-
formers, and pointed out how well they are adapted for use in

Mr. Eason. conjunction with accumulators. It is probable that some one will refer to this later on.

Professor Forbes. Professor FORBES: I think, Sir, that the paper which has given rise to this discussion is of a character which ought not generally to be read before the Society. I think it is unfortunate that a paper should have been given to us which is a purely hypothetical case. I think we ought generally to deal in our papers with accomplished facts as much as possible in the case of electric light installations, and with the actual results which have been obtained from practice; and I consider that the discussion which this paper has given rise to is probably the more valuable part of it, although the discussion has extended itself beyond the scope of the paper and become a general discussion on the distribution of electricity. I am very sorry that Mr. Crompton is not here. There are several things which I have to say this evening which I should have particularly wished him to hear me say, especially these introductory remarks. I think it is extremely unfortunate that he did not give us a paper on the installation which he tells us has been so successfully worked, and which would bear out the results of the paper itself, viz., the Vienna installation, and another one which he has at Kensington. He would have been able to give us some information about the action of accumulators which have been used in those two places—about the amount of current that was put into the accumulators and the amount that was taken out, and a great deal of information which would have been very valuable to us. It is all very well for him to say to us that we may judge of these (the Howell) accumulators which he uses at Kensington for ourselves; that we have every facility for examining them; that he will allow us to go to Kensington any evening we please, to see them working. I, for one, have not been able to find the means of determining the efficiency of an accumulator from simply seeing it at work and examining it in a central station while at work. Mr. Crompton stated at the beginning of his paper the reason for which he has written it. It is to support an assertion which he made on a previous occasion, to the effect that "I "believed the distribution of electricity by transformers offered

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" no special advantages over other methods ; " and then, instead
of discussing the various methods which might be used in
preference to transformers, he selects one—perhaps the least
advantageous—that of accumulators. But even if he proved his
case in that particular instance, he has not proved the assertion
which he made—that he sees no special advantages in transformers
over other methods—because he has taken a single case, a case of
a small station of 10,000 lights with charging mains 2,000 yards
in length ; and even if he proved his case that transformers were
not eligible there, he would not have proved his case that trans-
formers were not eligible in another case. Now, in the special
case which he has taken, I think it would have been much more
natural that he should have taken some other method of distribu-
tion. There are numerous other systems which he might have
adopted and have gone into the figures for; in the present case
there is not the slightest doubt in my mind which system would
have been found the most economical, and that would have been
far cheaper than either transformers or accumulators, and that
is the multiple-series system with alternating currents introduced
by Mr. Stanley in America, and which was mentioned in Mr.
Mackenzie's excellent paper on transformers, read before the
Society a short time ago. As an example of the cost of these
different. systems of distributing, I may mention the work which
I did a couple of years ago, after I had published the theoretical
work on the distribution of electricity. I took a special case in
London of the " club " district of Pall Mall, St. James's Street,
and St. James's Square. I got information from all the secre-
taries of the different clubs as to the number of hours of lighting
the different rooms, the number of lights in each room, and
other data. I then devoted a considerable length of time, with
the assistance of Mr. W. H. Snell, who was then in my office, to
work out the cost of the different systems. I simply mention
these results, as they may be of interest. The simple parallel
system cost per candle-power, per annum, 2s.; the three-wire
system, 1s. 10½d.; the multiple system, 1s. 10d.; the multiple-
series system, complying with the Board of Trade regulations
(about 200 volts difference of potential), 2s. ; simple series system,

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like Mr. Bernstein's, 1s. 10¾d.; secondary generator, 1s. 11½d.; and secondary batteries, 2s. 4d.; so that this was a case where secondary generators or transformers were not the most efficient system; but it does not prove, any more than Mr. Crompton's estimates can, the proposition that the transformer system has no special advantages over other systems. Now, as our Chairman has said that the time is limited, I do not think it is worth while to devote very much time to the figures which are given in Mr. Crompton's paper. There are two tables in his paper of the cost of installations and cost of maintenance. There are distinct errors in the two of them, and I will only trouble you by pointing out what are the errors which produce the fallacy—as fallacy it must appear to everyone who is acquainted with alternate currents—as to the figures which are arrived at as the result. In the first place, when we take Table 3, we find that there are 600 kilowatts as the maximum. From the remarks which have been made by gentlemen here, I fancy it will be of interest if I illustrate my remarks on the paper by the practice in America, of which I have had considerable experience, and which I have examined in all its details in the last few months. Now it is found most positively that the alternating-current transformer system, which is used there by Mr. Westinghouse, enables us to get 600 kilowatts in the lamps for 1,000 indicated horse-power. Mr. Crompton, for the 600 kilowatts station, has demanded 1,450 H.P., instead of 1,000 H.P.; that is to say, he has added on 50 per cent. to what is actually required as a maximum. That, he would say, was intended for reserve. It is enormously beyond what the reserve requires. This reduces also the figures which represent the cost of the " generating station, buildings, " chimney-shaft, water-tank, and general fittings." He has taken the alternate system at £11,000, and the accumulator at £8,000. There will be some reduction on the former figures. The cost of " dynamos and exciters " is perfectly correct in accordance with American figures, and the price which Mr. Crompton has given us would include everything—station-switches, resistance-boxes, voltmeters, ammeters, lightning-arrester, ground-detectors, compensators, synchronisers, besides spare armature,

ball-bearings, and brushes.   Again, in this table the price of the Professor Forbes.
transformers is given as £7,500, instead of £4,000.   The mains
are the point on which Mr. Crompton has made the greatest
error.   He has not appreciated the value of the transformer
system in economising mains.   In this scheme which he has
proposed he has given us £20,430 for the mains, and has sup-
posed that for a wretched little station of 10,000 lights we are
going to put down some 22,000 yards of underground mains.
Now these charging mains, which carry our current, carry only
300 ampères as the maximum; and are we going to dig up wooden
paving and concrete, and build a trough and fill it in again, and
put a wretched conductor of half-inch diameter at the bottom of it ?
It is simply absurd that we should take a case like that as illustrat-
ing the way in which the transformer system should be applied.
If we are to put down such a small station under these conditions,
it undoubtedly ought to be by overhead wires; but then the cost
immediately falls down enormously.   But with the other systems
which are at our disposal—the three-wire system, or any other
system which is at our better disposal—the current is enormous,
and is much greater than 300 ampères, and it would be impossible
to put a conductor carrying that current—a single conductor—
overhead.   The potential which Mr. Crompton uses, of 480 volts,
is a potential which is not allowed to us in this country, and the
current which he is carrying in the charging mains is 1,500 ampères.
He uses wires whose sectional area is 2·55 inches, and you cannot
have wires of that size swung all the way overhead.   Such massive
conductors must be put underground; and still more would it be
necessary if a system complying with the Board of Trade regula-
tions were adopted.   Consequently I reduce Mr. Crompton's
estimate for the mains in the alternating system in this case to
£6,000, and thus the total cost of the alternating system is laid
down for the supposed central station which he has proposed at
£39,140, instead of £57,440; and thus we have the comparison
between the accumulator and transformer distribution systems.
The cost of the former, assuming that he is correct, is £59,762;
and the transformer system is £39,140.   Having pointed out
now the sources of error in this paper when dealing with the

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alternate-current system, I will now point out one source of
error in connection with the accumulator system which is
sufficient to throw doubts on the accuracy of these results.
I speak of the efficiency of the accumulators. You will find
that he tells us, near the end of the paper, what is the efficiency
of the whole system—that is to say, the combined efficiency
of the conductor system and of the accumulators. He says:
"For an output of 2,100 units, it will be sufficient to work
"the generating plant for about five hours at approximately
" its greatest rate of efficiency. I have had ample experience of
" this class of plant, and know that we can actually in practice
" produce the electrical H.P. at the terminals of the dynamo with
" an expenditure of 3½ lbs. of coal " [per hour]. Good; granted.
Then he says: " Taking the maximum loss in the accumulators,
" the efficiency of the system—i.e., the electrical H.P. at the lamps
" divided by the electrical H.P. at the terminals of the dynamo—
" will be not less than 80 per cent.; therefore "(he goes on to say)
" we may say that each unit at the lamps can be produced for
" 5·75 lbs."—a Board of Trade unit for 5·75 lbs., i.e., 4·3 lbs. of coal
per H.P., which is simply 20 per cent. added on to the 3½ lbs. of
coal—3½ lbs. of coal for the work given out at the terminals of the
dynamo, and only 20 per cent. loss over the whole of the con-
ductors, and taking into account the working of the batteries as
well. He has told us in his paper that he will admit a loss of 10
per cent. in his charging mains and 4 per cent. in his distributing
mains : that comes to 14 per cent. As a matter of fact, I have
worked it out, and I find that his charging mains have a loss of
12 per cent. instead of 10 per cent.: that will make it 16 per cent.
altogether. But I do not know how his distributing mains are
worked, and I must adopt the 4 per cent. which he gives : thus
16 per cent. out of the 20 per cent. is due to the charging and
distributing mains during all the five hours that the principal
work of the twenty-four hours is being done, and hardly more
than 4 per cent. loss in the accumulators ; that is to say, he is
employing accumulators giving an efficiency of something like
96 per cent. The accumulators, he says, will discharge normally
at the rate of 416 ampères ; they are to be able to go on dis-

charging at that rate for five hours, giving a capacity of 2,080 Professor Forbes. ampère-hours; but while they are normally discharged in this way, they are to be capable of a very rapid discharge at an efficient rate—the rapid discharge of 1,352 ampères.   He has put the depreciation of accumulators at 15 per cent.   I know, from the experience which Mr. Preece told me the other day, that in his accumulators, which have been carefully watched, out of 16 lbs. of active material there is 1 lb. lost in the first year.   At what rate it is lost in subsequent years, or how many years it takes to destroy the cell, I cannot tell; but that makes me very certain that 15 per cent. for depreciation is not sufficient.   Finally, in Mr. Crompton's figures of these cells, he tells us that in order to make his system effective he must have a cell which will normally discharge at 416 ampères for five hours, which will have a possible rate of discharge of 1,352 ampères, which will give an efficiency of 96 per cent., and which will have a depreciation of only 15 per cent.; and that he will get these cells for £40 apiece.   I have said that it would have been better had Mr. Crompton given us the results of his own experience with accumulators at Vienna and at Kensington, and I am perfectly certain that the results would have been totally different from what he has claimed.

I also regret that in this paper he has not been able to give us actually the results of practical experience, and the facts with regard to accumulators, which he says are taken from what *he is told*.   After describing the nature of the accumulator he wants, he says : " Up to a recent date far too much attention has been " paid to the capacity, and too little to the rate of possible " maximum discharge.   This state of things has lately been " remedied, and *I am told* there are several makers of accumu- " lators who can supply them to fulfil the above-mentioned " conditions" (the conditions I have told you), "and at the " reasonable price named by me in my estimate " [of £40 per cell]. That looks simply like rumour : *he has been told it.*   Likewise, the facts given for the alternating-current system are stated to have been derived simply from hearsay information, for he says : " For purposes of my comparison I take the system as generally " described by Mr. Kapp in his paper on transformers; or by Mr.

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"Mackenzie in his paper on transformers; and in an article in
"*Industries*, dated December 7th, 1887, for which I believe
"Mr. Kapp is responsible, and which gives very complete data on
"the first cost of an installation for 10,000 60-watt lamps, burnt
"simultaneously." Thus, undoubtedly, the want of practical
experience with the alternating-current system is the reason why
he has fallen into the errors which I have pointed out. I am
sorry to see that in the early part of his paper Mr. Crompton
made a statement to the effect that the experience which has
been gained in America and in other places with the alternate-
current system "does not help us much in our investigation." I
should say that it would help us infinitely more than the reading
of any number of papers he may have laid hands on. I say, from
personal experience, that in the few months that I have been
studying the system in America I have gained more practical
information such as is applicable either to such a station as
Mr. Crompton has given, or to any other, than I could have
obtained from reading the papers of all the authors who have
written in this country on the subject.

I wish to revert to one point before I leave the question of
these estimates. Mr. Crompton has said that a great deal of
nonsense has been talked about the cost of conductors being
proportional to the cost of copper. I daresay that may be so: I
do not remember it; but do not let us talk any more nonsense
on the subject. Mr. Crompton says that his tables are sufficient
to show that we must not consider the cost of laying the con-
ductors as in any degree proportional to the cost of copper. I
say that in overhead wires you must consider that this is so;
and while I would not wish to see overhead wires laid in a
really large installation, still in small installations like this of
10,000 lights I think they ought to be laid. But what we want
to tackle in this country is a sound installation of a large number
of lamps. In most of my discussions on the distribution of
electricity I have generally dealt with an installation of 100,000
lamps; and that is the size that we shall have to deal with, I
should hope, in a town of a population of 4½ millions. Look at
the dimensions of the cables which Mr. Crompton gives us in

Table No. 2, and the cost of laying them. You will see that with the small sizes of cables the cost of laying them is out of all proportion to the cost of the copper; but when you come to 2·55 inches and 3 inches section you will find that the cost of laying is becoming gradually very small compared with the cost of the copper; and the total cost of copper and insulation and laying is nearly proportionate to the weight of copper when you come to cables capable of carrying the high currents required to supply the 100,000 lights. We may take it as absolutely assured that the total cost of laid cables is roughly proportional to the cost of the copper. I believe that Mr. Crompton has given indisputable proofs of the accuracy of his figures. He has said that he is ready to lay down an accumulator system on the lines indicated in his paper at the cost put down for it. I daresay he is, but he will not be tried, because it is not allowed in this country. He has introduced a high potential of 480 volts without attempting to gain much in the quantity of copper laid. He saves in his 2,000 yards of charging main, but his 20,000 yards of distributing main is the same size as if he had used low potential. On the other hand, I will tell you that I know plenty of people who will be perfectly willing to lay down an alternating-current plant to supply the district which he is speaking of at the figure which he has given—£57,440. It is perfectly certain that that can be done also at a much lower figure.

Here, Sir, my criticism of Mr. Crompton's paper ends. The discussion has, however, turned on the question of the general distribution of electricity. I intend some time to give, somewhere or other, a full account of the alternating-current system as used in America, and I had thought that possibly some account of it might have been interesting here; but you wish to close the discussion to-night, and I might take up the time of other speakers, and perhaps my account may be best left over.

One fact occurs to me as bearing upon Mr. Crompton's estimates—that is, as to the station attendants. The cost of keeping up a station in America struck me more from a commercial point of view than anything else—the small number of men that were required, and the small amount of labour and

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supervision required for a station. Denver has a capacity for
7,000 lamps: it retains one manager at 150 dollars a month, a
secretary at 80 dollars a month, and an office boy; it has two
dynamo attendants at 70 dollars each, two engine attendants, five
firemen, two inspectors who go over the lines, and one repair
man. That is the total staff required for the Denver installation
of 7,000 lights. That is a station where coal is used. In several
of the stations which I visited—for example, at Pittsburg,
Alleghany, and East Liberty—coal is not used. On going into
the boiler-room you wander about, and the first thing that strikes
you is the apparent absence of attendants, and at last you spy a
melancholy man sitting in a corner reading a newspaper and
attending to the whole of the boilers of a 10,000-light plant.
The furnaces are fed in this case by natural gas. The natural
gas and the air required to be consumed are led in through pipes
controlled by a valve which is controlled by the pressure of the
steam in the boilers, and the engine attendant has simply
nothing to do. The pressure in the boilers is able absolutely to
regulate the supply of gas and air. The engines chiefly used
with the alternating-current system are the Westinghouse
engines, which, although fairly economical, are not very
economical, but they give very little trouble, and the governor
runs in an oil bath. The dynamo machines which are used are
totally different from what have hitherto been the forms of
dynamos in this country. I do not know what progress has been
made in the last few months, but I presume that in this country
we have given up the type of alternating-current dynamos which
were the favourites lately—those without iron in the armature,
such as the Siemens and Ferranti. There is no doubt about it
that the iron in the armature is an essential feature of a good
alternating-current dynamo. The Westinghouse machine consists
of a large outside ring attached to the framework, and has poles
projecting inwards so as to form the field magnet. The armature
is of the drum form, consisting of a large number of thin sheets
of metal, and it is wound in a special way: lathes of wood about
1½ inches wide are laid along the periphery parallel to the axis
of the armature, and cleats come over the edges of the armature;

sixteen of these are placed round the armature, corresponding to Professor Forbes.
the sixteen pole-pieces; the wire of the armature is wound round
these pieces of wood, is bent over the end of the armature and
caught in a cleat; and when the winding is finished the armature
is wound round with fine steel wire, which is soldered here and
there, making a very mechanical and good armature. It
takes only seven hours to wind a full-sized machine. The
machines are mechanically and electrically excellent. I have
little doubt about that. The heating is much less than what we
would have expected from this type of dynamo, and is very little
indeed. The exciting power is 2 per cent. of the total power
developed by the machine. There are only three types of
machine made—650-light, 1,300-light, and 2,600-light. The
number of revolutions of the larger machines is 1,000 a minute;
the resistance of the armature of the large machine is ·15 ohm;
the weight is about 18,000 lbs. There is one defective point in
the machine—that is, that the position occupied by the lathes of
wood ought to be occupied by some useful material. At first
sight one would naturally suppose that the lathes should be
connected up in the form of a Paccinotti ring armature; but there
are enormous insulation and mechanical difficulties, which would
increase the cost of the armature of the machine to a great
extent. I have suggested, and I believe the suggestion is going
to be adopted by Mr. Westinghouse, the utilisation of these
vacant spaces by removing the lathes of wood and putting in a
complete additional set of winding which will form a separate
circuit and be a quarter of a period behind the phase of the
other, supplying a different set of feeders. By that means the
capacity of the dynamo, already very good in itself, will be
increased something like 25 or 30 per cent. With regard to
alternating-current machines, I should like to throw out the
suggestion that the most efficient type of machine of the future
seems likely to me to be a different type to that which has been
hitherto adopted—only on one supposition, however. Most of us
have been very much interested in the progress that has been
made by Mr. Parsons in his turbo-electrical generator. That
engine is now consuming 39 lbs. of steam per horse-power hour,

and it is being reduced gradually, therefore it is likely to be one of our economical engines. The large size—300-ampère machines —revolve at 8,000 revolutions per minute. I suggested to Mr. Parsons, and he has undertaken to act upon the suggestion, that he should take the commutator and put two insulated rings round it—the commutator is a long one—so that one ring should go round and be connected to any one bar of the commutator, and the other ring should be connected with the opposite bar of the commutator, leaving a small length of the commutator in action in the ordinary way to excite the field magnets of his dynamo. Since there are 8,000 revolutions per minute, there will be 16,000 alternations, which is exactly the number used by the Westing-house Company.

One word about transformers, or converters. I have had the opportunity of carefully testing the converters at Mr. Westing-house's works at Pittsburg, and I adopted the same method that Professor Ayrton described before the Society some time ago— that is, the calorimetric method, a method which has been used by Ruiti in the Gaulard-Gibbs apparatus, of passing water through. I made some careful tests, and the most important result which I got, and which will interest some of you, was that in his No. 6 converter, which is the 30-light machine, with a half load, I got 95 per cent. I must stop now, but I cannot stop without saying a parting word about the manufacture of the dynamos and converters at Mr. Westinghouse's establishment. There are only a certain number of types made—only three types of dynamo, and only five types of converter—and that plan is absolutely essential to the success of manufacture on a large scale in order to supply the public at moderate prices. It was quite sad to me to remember how very different a plan was followed by our manufacturers in this country, where the man who buys specifies the exact number of volts a machine should give. It is ridiculous to expect that we can get economical dynamos at this rate. It is only by doing it on a large manu-facturing scale that we can get things at a cheap rate. To make a single converter at the Westinghouse factory about ten different machines have to be employed—ten machines which are extremely

costly to make—but by their means he is able to turn out 50 or Professor Forbes. 100 of the parts of a converter which each has to do in a day. Therefore, by having these machines always ready to construct exactly to scale, he is able to produce the machines at an extremely low figure. Moreover, beyond the sensible plan that it is from the manufacturer's point of view—which I would urge on the manufacturers of this country—there is the important fact from the user's point of view that the parts of such machines are all interchangeable, and consequently, if your dynamo armature gives out, you have simply to telegraph for dynamo armature "No. 3," and it comes back by the next train, and so on, whatever the part is that is required. I have a few specimens of some of the smaller parts of Mr. Westinghouse's system of distribution placed on the table—switches, cut-outs, and other things which may be of some interest. The switch-board used in the central station is the most important part of the whole thing, and I should like to have given some description of it, but there is no time now, and to tell you of the simplicity and smallness of the parts of it as compared with the gigantic things which are used in this country. One is impressed by the security given by these switch-boards in working transformer systems. I have also placed on the table a card showing Mr. MacIntyre's system of joining wires together. He uses a double tube, into which the respective ends of the wires to be joined are pushed, and then, by the twisting of a wrench, the whole is very firmly pressed together, and a very sound joint is made. I showed it to Mr. Preece, who tells me that in time oxidation will take place between the metals. Upon that point you can judge for yourselves.

POSTSCRIPT.—Having been asked by the Society to add some further account of the manner of working converters in America, I will now briefly comply with that request.

Engines and Dynamos.—It is a mistake to suppose that one or two large engines are more economical than a number of small ones. A large engine working far below its power is very wasteful of steam.

One engine drives one dynamo by belt direct, without counter-

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Forbes. shafting. It is well to have each unit in a station of the same size, so that parts are interchangeable and the attendant has less to think of. The size of unit may be chosen so as to supply, at efficient load, the minimum current required in the twenty-four hours. One exciter is generally used for four machines in series, with an adjustable resistance. A shunt resistance largely made up of lamps may be put on each field for regulation.

Feeders.—These are generally overhead. Waring cable underground (lead-covered) is perfectly successful. Simplex wire is generally used for overhead work. Five or six feeders are often fed by one dynamo. A loss of 2 to 2½ per cent. of potential is generally adopted. Double bell insulators are used.

Converters.—These are made for 5, 10, 20, 30, and 40 lights. The wiring of a large house is best done with separate circuits for each 40 lights. It is unadvisable to " bank " or group in parallel a number of converters. These converters are very light, and have a smaller quantity of iron than might be thought good. They reduce the potential from 1,000 to 50 volts. The primary and secondary fuses are put inside the converter. The mode of manufacture of these has been described elsewhere. The insulation between the primary and secondary coils is indestructible.

Lamps.—It is found that 50-volt lamps last longer than those with thinner filaments, and are generally used. The potential in the primary is 1,000 volts. No man has ever been killed by a 1,000-volt alternating current. It is well here to mention that you cannot get an arc from the low-potential circuit when an alternating current is used.

Meters.—The Westinghouse Company has made such arrangements as will enable them to charge customers by means of the coulomb-meter.

Station Ampère-Meters.—The same instrument as Edison uses is adopted, but with a bundle of wires sucked into the coil. Both the bundle and the coil form part of an arc of a circle, and the former moves on an axis at the centre of the circle. The force of the current acts against the weight of a counterpoise, and an index indicates the ampères.

Station Voltmeter.—A " three-point converter " reduces the

potential either ten-fold or twenty-fold. A compound-wound Professor
Forbes. "compensator" reduces the measured potential to that at the centre of consumption. The voltmeter is solenoidal in action, and is true only when the speed of engine is quite constant. The wire bundles for these solenoids are ingeniously constructed. A metal cylinder is covered with varnished paper or muslin. Iron wire is wound round the cylinder, sticking to the varnish. The iron wire ring thus formed is cut through and a flat web of iron wire is produced, which can be cut into lengths and rolled into bundles.

Ground-Detector.—Two converters have a lamp on each secondary. The primaries are connected in series to the mains. The intermediate contact is connected to earth. If one of the mains has an earth its lamp goes out. A multiple switch is provided by which each circuit can be tested in turn.

Synchroniser.—To synchronise any two dynamos, secondary wires are led to a switch-board. A wire from one converter is connected to a wire from the other. The two other wires are connected through two lamps. If the connected wires are in the same phase the lamps go out. A switch is provided by which, as soon as the lamp goes out, the second dynamo is connected to the mains of the first. The dynamos work well in parallel from half to full load. Alleghany station sometimes calls to Pittsburgh, across the river, for assistance in current, and the synchronism is good. Synchronism is found to be quite satisfactory from half to full load, but not below.

Switch-Boards.—A switch-board is generally set up for each dynamo, and for five or six feeders. The conductors going to feeders run up and down ; those going to dynamos are horizontal. The dynamo wires are connected with three instruments in a vertical row —an ampère-meter, a converter, and a voltmeter. Each feeder has a vertical row of instruments, a three-point converter, a compensator, and a voltmeter. There are six switches for six feeders in a horizontal row, to switch any feeder into either of two pairs of dynamo conductors. One of these pairs is connected to the switch-board dynamo ; the other can be connected, by means of a switch at the side of the board, to the conductors of either of

the neighbouring dynamos. An adjustable resistance, with six lamps in series for additional resistance, is at the bottom and middle of the board, and acts as a shunt on the field magnets. Another adjustable resistance is in series with all the field magnets. Another switch connects either of two exciters with the field magnets.

There are some other devices connected with street-lighting and theatre-lighting which I am not at liberty to describe, owing to the English patents being not yet completed.

Mr. J. E. H. GORDON: After we have listened to Professor Forbes's most interesting speech, we feel that our friend Mr. Crompton, if he was here, ought to feel himself very much pulverised; but no doubt, if he was here, he would have a different view. Much has been said, not only by Professor Forbes, but by other speakers, about the disadvantage of not being able to obtain any accurate figures about accumulators, and in this I quite agree; and perhaps a few notes which I have of figures taken from actual contracts may clear up some of these points. Before going to those notes, however, I would like to say a word about the diagrams that Mr. Crompton showed last week, and in which I feel some personal interest, because I was, I think, the first to show them to the Society. There is one load diagram for London and two for America, and I want you to see how extraordinarily accurate they are; and this is shown by the trifling difference between them. In the London diagram the peak occurs at seven o'clock; at Boston it is a little after half-past five; and at Cincinnati it is a quarter to six; which just illustrates the fact that the average time for dinner in America is one and a half hours earlier than in London. Thus there is a coincidence which goes a good way to show the accuracy of the diagrams.

Really the stars in their courses seem to be invoked by Professor Forbes to fight against the accumulator system, for not only has it its inherent defects, but overhead wires are to be laid for the transformer system alone, which, I believe, are not going to be laid for anything else. We are hoping to get rid of overhouse wires, but they are stated to be necessary for the success of the transformer system on a large scale. Then we are to have a

Government which is to allow us to use 2,000 volts alternating, Mr.
Gordon.
but only 400 volts direct. We have not got that yet, but perhaps
we shall some day.   Then we are to omit all spare gear, in order
to keep the capital cost of the transformer system down.   I have
had a little experience in working without spare gear.   At
Paddington we have 50 per cent. spare gear, but we had a law-
suit, and had to make some alterations in the machines, one by
one; and for five months we had no spare gear.   Nothing
happened, but there was a great source of indirect expense, due
to what I might call the depreciation rate of the engineers, which
gets very high indeed when there is no spare gear.   In fact, I
think that if we were to go on much without spare gear, as the
industry develops the depreciation of engineers will go on at
such a rate that we shall soon have no engineers at all.

About the cells—what they will do, and what it is hoped they
will do.   Mr. Crompton has, I think, been at a little disadvantage,
because he has followed the understood rule of the Society—
which is hardly ever infringed—that a manufacturer should not
use these scientific discussions for advertising his own special
wares.   I have no interest whatever in any battery or other
electrical apparatus at this moment: I only go to the best
maker for the articles I contract to put down; and I can speak
upon the subject, perhaps, in a way that Mr. Crompton could not.
I will give you some details of the Howell cells—details taken
from a contract signed between Mr. Crompton and ourselves,
dated 22nd December, 1887.   The contract is to supply us with
492 cells—that is, 8 batteries of 60 cells each, and 12 spare ones
(for 110-volt lamps); and the contract price is £3,321, or £6 15s.
per cell, or £27 for four of these cells, which are equal to one
cell of the size referred to by Mr. Crompton, against £40 quoted
by him.   Here I acknowledge that Mr. Crompton has made an
error, for he has put the cell at £40, and supplies me with four
quarter-size cells equal to it at £27.   The normal discharge of
this quarter-cell is 105 ampères; the capacity is to be 525
ampère-hours.   At the end of such discharge the cells are not to
be so exhausted as to cause undue waste when recharging takes
place.   The next clause (I am quoting an abstract of the

Mr.
Gordon. contract) provides that the cells are to be capable of discharging at the rate of 200 ampères, and under those conditions they are to have a capacity of 375 ampère-hours. The efficiency at 105 ampères discharge is to be not less than 75 per cent.; the efficiency at 75 ampères discharge is to be not less than 85 per cent.; at 45 ampères discharge it is to be not less than 90 per cent. I must remind you that in practice, although we have a large discharge for a short time, the bulk of our work is low discharge for the longest hours. The cells are to be delivered fully formed. If, when the cells are put up, the capacity of the cells is not up to the mark, Messrs. Crompton have, at their own expense, to supply as many extra cells as may be necessary to bring up the total capacity of the battery to that specified for the 480 working cells; so that, if the capacity is one-half, they must supply as many cells again.

Professor FORBES: Is there anything in the contract about depreciation?

Mr. J. E. H. GORDON: I am coming to that. If the efficiency is not up to the specification, Messrs. Crompton must, at their own expense, supply as many extra batteries as may be necessary to reduce the discharge per cell to such an amount as shall bring the efficiency up to the contract figure. As to depreciation, Mr. Crompton has said here that he hopes to do it for 15 per cent. His contract with me is for five years; we are bound only year by year. That is to say, as long as we choose, we can compel him to keep the cells equal to new for this rate; but if ever we find that we are paying too much, and prefer to do it ourselves, we can stop the contract: he contracts to keep the cells in all respects equal to new for a payment of 12½ per cent. on the contract price, not 15 per cent., or 40 per cent. either. That, gentlemen, I think, disposes of the feeling—I will not say suspicion—that might be held that Mr. Crompton was afraid to speak out; because I think his reason for not mentioning this was that he felt he could not in this place go into a matter that might look like advertising his own manufacture.

Mr.
Sellon. Mr. R. PERCY SELLON: I congratulate Mr. Gordon on having had the boldness to throw himself into the breach as the advocate

of secondary batteries. In the course of my experience at the $\underset{\text{Sellon.}}{\text{Mr.}}$
Telegraph-Engineers I have never listened to such a one-sided
discussion as we have had on Mr. Crompton's paper. The
advocates of alternating currents have appeared in all their
force. The advocates of the secondary battery have not been
heard at all. Now I am sure we have all been extremely pleased
to get from Professor Forbes some definite figures on the working
of alternating currents in the United States; and on the
strength of them he takes exception to the figures in Mr.
Crompton's paper, and criticises them so unfavourably that
alternating-current transformers are made to appear relatively
far more economical for the purposes of extended distribution
than batteries. First of all, he says that in the United States
mains are laid overhead; and he infers from that, as I understand
it, that in this country we are also to be allowed to have mains
overhead. He thinks, therefore, that Mr. Crompton has put an
extravagantly large figure in his paper for the alternating-
current mains, and that that should be cut down to something
very much smaller. I was in the United States myself not many
months ago, and I am perfectly aware of the fact that throughout
the length and breadth of the United States alternating distribu-
tion is carried out by means of overhead mains; but then
Americans will accommodate themselves to circumstances in a
way that would simply not be tolerated in this country. I do ·
think that no sane electrical engineer in this country would
attempt to lay down with overhead mains an installation, not for
a matter of 10,000 lights, but, as Professor Forbes has said, for a
matter of 100,000 lights, which is the practical point we ought to
consider. That being the case, the moment that you have to
deal with an installation of that size—and you have to take into
account that your mains are going to be laid underground—
I say that it brings you to this point: that we are in total
ignorance of what the working of a system of high-tension
alternating mains would be, ramified right and left under-
ground. Mr. Hammond, in the course of a very eloquent speech,
last week pointed out to us his experience at Eastbourne and at
Brighton. He left us to infer that because he has got a few

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Sellon.
miles of mains with alternating currents and a few hundred lights at work there, we are therefore to draw the conclusion that when you come to lay down 100,000 lights the same absence of difficulty will be met as he has met with at Eastbourne and Hastings. Now I suppose that the company to which I belong has had more experience with high-tension currents than any other company in this country; and I must say that if we are to judge from the experience we have had with continuous currents of 2,000 volts, when alternating-current distribution comes to be carried out, with transformers in every house, and an underground network with 2,000 volts laid on, as they say in " John Gilpin," " May I be there to see." Again, the figures that Mr. Gordon has given us on the question of efficiency of secondary batteries, I think, will really settle that point in dispute. It is a very remarkable fact that even some of the most ardent advocates of alternating currents (and I take Mr. Kapp as an instance) virtually say: " We will put down " alternating currents for the present. Why ? Because, in our " opinion, batteries are not sufficiently reliable. When batteries, " as we look at it, do become sufficiently reliable, we will " pull out all our alternating-current plant and put down fresh " plant to work by means of continuous currents and batteries." Now this, to my mind, is the most absolute acknowledgment of the fact that a system which is based on the employment of batteries for the purpose of distribution is the ideal system to which to direct our attention; and that being so, I think Mr. Crompton is to be congratulated on his perspicuity in having at once addressed himself to developing a system in which secondary batteries are to be used, rather than addressing himself to a system which even its own advocates regard as a stop-gap system. What are the objections raised to batteries? The secondary battery, up to the present, has no doubt fallen a great deal into odium ; and I think it is to be accounted for in this way: When a dynamo machine or a converter goes wrong, it lets you know it; but with batteries you have a totally different state of affairs. People rely upon the fact that secondary batteries will not complain when going out of order: they see nothing going wrong;

Mr.
Sellon.

the batteries do not change colour, nor do they exhibit any remarkable phenomena, and the mischief is done before you know it.

Now when you come to distribute in · practice on the lines which Mr. Crompton advocates—that is, distribution from local centres—that difficulty will be entirely removed : the batteries will be placed under the care of somebody who is competent to attend to them ; and under these circumstances the main objection, as I understand the matter, to secondary batteries disappears.

There is, however, one point that I should like to ask Mr. Crompton with reference to his paper. His figures are based upon 480 volts as the E.M.F. of supply—*i.e.*, for the distribution of 10,000 lights, with four sub-centres with 2,500 lights each. Now, as Professor Forbes has said, the question to consider is not the distribution of 10,000 lights, but of 100,000 lights ; and when you come to that state of affairs, what is Mr. Crompton going to do ? Is he going to increase the number of local centres that he operates from a single source of supply and place them in series, or is he going to have a separate set of generating plant or a separate main generating station for every four sub-centres ? It is clear that if he is going to put a larger number than four, or substantially four, local centres in series with a single source of generation the total difference of potential will increase accordingly, and under these circumstances the danger arising from leakage and the inconvenience arising to the householder will become so great that he must take precautions that charging and discharging will not be carried out at the same time ; and he must either have two sets of batteries, or, as in the Beeman Taylor and King system, he must have his battery divided up into two halves, and charge one half while the other half is discharging. That, however, involves that your charging shall never be on at the same time as the discharging, and consequently at the time of the maximum demand. The two halves of your batteries must therefore supply the whole of the demand between them. I think, therefore, that under either alternative, where you are considering a question of distribution on a large scale, such as we may expect to encounter in practice, Mr. Crompton must be prepared to

modify the basis on which his figures rest.   I do not wish to take
up the time of the Society, but I should just like to mention one
point which has been referred to by Mr. Esson to-night.   It is
not relevant to the question of batteries *versus* transformers, but it
is relevant to the question of distribution.   I refer to a system in
which continuous-current transformers combined with batteries
would form the source of supply.   Under such circumstances the
procedure would be this : During the few hours of maximum
demand your continuous-current transformers and batteries would
together supply the current.   Under the conditions of what you
would call medium demand, which is for about one-half of the
twenty-four hours, your whole plant would be in operation ; but
there would be a surplus of current supplied from the transformers,
which would go to charge the batteries.   During the period of
minimum demand, which is shown by the load diagrams before
us to be one-third, or perhaps rather less, of the whole twenty-
four hours, your accumulators alone would supply the necessary
current.   Now such a system as that meets the objections, I
think, to both systems which are the subject of Mr. Crompton's
paper.   You obtain the advantages of storage; you obtain the
advantages of motive power ; and yet you do not require to
have a sufficient bulk of batteries to supply the whole of
your demand.   I think it is extremely probable that a great
many gentlemen will receive with incredulity the suggestion
to employ a system of that kind, because they will say
that " your continuous-current transformer is a revolving piece
" of mechanism, and it is a costly piece of mechanism, and
" how are you going to insulate it ? "   True, it is a revolving
piece of mechanism ; but nobody is going to put it on consumers'
premises.   If you have local centres (and my contention is that
you will have and must have local centres, whatever system of
distribution you use) it really doesn't matter whether your
apparatus is revolving or whether it is stationary.   It is true that
the revolving apparatus will entail a small amount more of
attendance, but the advantages which you obtain from the
system in other respects, I think, will fully compensate for this.
At present the continuous-current transformer is a very crude

piece of apparatus.   No doubt, if you compare it with the present ... Mr. Sellon.
dynamo machine, it is a high-priced piece of apparatus.   But in
practice it would not be built on the lines that we now adopt for
our dynamo machines, and for a given weight and a given bulk
you would obtain a very much larger output from it; and it will
be obvious to everybody why, in a perfectly balanced apparatus of
that kind, that should be so.   As to insulation, I can only say
that if you are really going to employ very high tension currents,
you have in the continuous-current transformer a means for
obtaining immunity from leakage which no other system
possesses.   For example, your secondary coil may be driven
from your primary coil by means of some insulating coupling
between the two; and although I am aware you forfeit certain
advantages in this, nevertheless, so far as insulation goes, if there
is a balance of advantage anywhere in the three systems, it
certainly lies in favour of the continuous-current transformer.
In conclusion, I can only repeat that Mr. Crompton has, in my
opinion, been very wise in addressing himself to a system in
which secondary batteries are to find a place, for I feel convinced
that a system based on these lines will be the system of the
future.

Mr. R. HAMMOND : Will you allow me to say one word in   Mr. Hammond.
correction of a fact mentioned by the last speaker?   He referred
to the systems at Eastbourne and Brighton as being a matter of
a few hundred lights and a few miles of wire.   I think, Sir, that
the Society ought to know that they should more properly be
referred to as a matter of over three thousand lights and twenty-
two miles of wire, worked for almost seven years.

Mr. BERNARD DRAKE : I quite agree with Mr. Kapp that those of   Mr. Drake.
us who are connected with the manufacture of secondary batteries
are much indebted to Mr. Crompton for the way in which he has
pleaded our case; but I do not think we have much to thank
him for in having alluded to Mr. Hammond's work as "temporary
"lighting."   This has brought upon the unoffending battery a
perfect storm of abuse, which I cannot help thinking it would
have escaped if Mr. Hammond had been left alone; especially as
this gentleman has only lately put down some of this maligned

Mr.
Drake

apparatus for the lighting of his own house. The first question which we have to consider in comparing the systems is, supposing that both are working over the same area, which is to be preferred by the public? My own opinion is that the battery system would carry the day, even though 1s. per unit were charged instead of 7½d. In the first place, they are afraid of the high-tension system, and also are prejudiced in favour of the idea of pumping into a reservoir, from which you draw as required. It appears to them, from a common-sense point of view, that this is more reliable than depending on moving machinery. Battery systems must be divided into two distinct classes—those in which the cells are charged in the daytime, and afterwards separated from the dynamo and discharged on to the lamps, in which case the depreciation will amount to 15 per cent., and the efficiency to 70 per cent.; and the others in which dynamo and cells are run together during the heaviest part of the lighting, when the depreciation will be about 7 per cent., and the efficiency 85 or 90 per cent.

There seems to be a prevailing notion that the very presence of batteries in the installation involves the loss of 40 per cent. of all the current used in the lamps. This is not the case; it is only the current which goes into the batteries and is reconverted back again that suffers any loss.

If a charging current of 500 volts is employed there will be no danger to life; no loss in conversion of the current used by the lamps to the full extent of the dynamo output; there can be no sudden breakdown; the station can be worked with one shift of men, and the engines constantly kept going at the point of greatest economy. Under these circumstances I cannot but think that batteries will compare very favourably with secondary generators.

The main question is the depreciation of the plates, and I quite agree with Mr. Sellon that the past is no criterion, for they have been placed in isolated installations in the hands of people who had no idea how to use them. Batteries properly looked after will last twice or three times as long as those which are not under proper supervision. I was glad to hear from Mr. Crompton

that the cells which Mr. Gorham and I made for the Vienna work, Mr. Drake. in connection with which he made the dynamos, are doing so well, as also those which we made for Mr. Preece. There is another case to which I would refer—probably the oldest battery in London—namely, those at the P. & O. offices, the plates for which were made even before we became connected with the Storage Company. These cells have lasted four years without repairs, and are now in perfect order—a result entirely due to the way in which Mr. Hall, the engineer, has looked after them. That batteries can be found which have worked in this way clearly shows that they can be relied upon to last well under certain conditions, but for the present I do not agree with Mr. Crompton that in first cost and maintenance they can be made to compare with the secondary generator system. Where expense is the main object, the district is spread, and large halls have to be lit more than private houses, the alternating system will undoubtedly be most used; but I do not attach much importance to the fact that in America the transformer system is the only one which has made any way, for the Americans have practically only one company who are devoting themselves to making secondary batteries, and they came over to England only a year or so back to learn how to make them. If the same amount of brains and time had been expended on batteries in America as on transformers, we should no doubt have heard from Professor Forbes of the hundreds of tons of batteries he saw, as well as the large quantities of transformers. Automatic contrivances will no doubt be useful, but I see no reason why the system should not be started without them, provided the cells are put in central distributing depôts, for the man in charge can easily switch the batteries in and out by hand as required. In America one sees five or six large engines and dynamos side by side, but it has never been stated that the station could not succeed because the engines do not start and stop automatically according to the work in the lamp circuit.

Finally, I am of opinion that both systems can be made to work quite satisfactorily, and to pay their way, but I am convinced that the question as to which system is most largely

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adopted for the next few years in England will depend very much more upon the engineers than upon the system itself : whichever gets the best men in the early days will become fashionable, and more time will be devoted to its development. If the battery system only receives the same attention as the secondary generator system, and good practical engineers are interested in its development, I cannot but think that it will hold its own both now and in the future.

Mr.
King.
Mr. FRANK KING: As the first in England to put into practice a portion of the subject which Mr. Crompton has brought before us—the distribution of electricity by means of storage batteries—and which I carried out in Colchester some years ago, I have read his paper and listened to the arguments with a good deal of interest. I think that Mr. Crompton might have gone farther in his charging arrangements, so as to utilise a higher tension for that purpose. I used at Colchester an E.M.F. of 1,250 volts. That was running two and a half years, and we never had the slightest difficulty with our cables or with our dynamo machines ; although the material of which the boxes or cells were made had given way, so that they were running like riddles, and there was every incentive for the insulation of the cables and the dynamos to break down, all I can say is that they remained sound and perfect. The cables have been taken out from the Colchester trenches, and I can guarantee that they are as perfect now as when laid down. I believe that is simply due to their being lead-covered. I should propose in almost every instance of secondary battery charging for central stations to use up to 2,750 volts, and I am quite sure that the insulation of the battery can be made so perfect as to allow of that being safely used—not only safely as far as the consumer is concerned, but safely so far as regards risk to the attendants in charge. Another installation, which was laid down four years ago, was at Halton House—Baron Alfred Rothschild's mansion. There we charge a set of batteries with an E.M.F. of 535 volts through something like a mile of charging cable ; and in that instance the cable, which is like that at Colchester, but is laid in plain wooden troughing and buried under the ground, has never

given the slightest trouble from insulation. I am rather _{Mr. King.} referring to these not as matters of history, but to draw attention to the fact that Mr. Crompton in his arrangements might have gone a considerable deal further with his potential for charging, and consequently have saved that very large amount which he puts down for charging cable. The next point, I think, in Mr. Crompton's paper that requires notice from storage battery advocates is the fact that he only uses four subsidiary stations. I am confident that, if he calculates out the fall of E.M.F. in his cable from these four distributing stations, he will find that the E.M.F. of his lamps will vary rather considerably during the fluctuations of demand which are shown on the diagrams. I should use for the same purpose ten battery stations, or, if the area was one somewhat like Kensington, I might go as low as six, but certainly not four; and even using ten stations the distributing cable would have to be three times the sectional area which Mr. Crompton gives, although when charging at high E.M.F. the total expense for cable would be rather less. I think it will be pretty well known that I am an advocate of what I call the complete storage battery system. In my opinion (and my experience has been considerable), no reliance is to be placed on the continual running of any dynamo machine. You must have such arrangements as will enable a consumer always to have his maximum amount of light; and if you have a dynamo charging your storage battery while the maximum demand exists, the chances are that at some unlucky moment when your load is at the maximum away will go your dynamo machine, and your batteries will be overstrained by the extra discharge thus entailed. It may be said we can make batteries that will give twice their normal discharging currents. All I can say, as a manufacturer of storage batteries, is, that I can do the same, but I do not wish to do so at present. In what was known as the B.T.K. system we charged our battery in two halves during the daytime—as a matter of fact, nine hours' charging per day, which is sufficient for the whole three and a half or four hours at which the consumption is at its maximum. With regard to the durability of storage batteries, all statements and all previous experience have

Mr.
King.

been based upon storage batteries which have inherent constructional defects in themselves, which, I think, until lately have been overlooked. But there is not the smallest reason why a battery should not be made in which it is absolutely impossible for any internal action to take place which will run that battery down, and I think it will be agreed that unless such action does take place there is no reason to doubt that the storage battery will remain in order. Professor Forbes has told us that 1 lb. of peroxide was found at the bottom of a cell—in which there was 16 lbs. of active material—after twelve months' use. That is quite beside the mark altogether. We do not put just enough active material into our cells to give us the capacity we want. In all probability there is four or five times as much. If only 1 lb. of peroxide powder falls out of the 16 lbs. in one year, then, if powdering continues at the same rate, my estimate of the life of a peroxide plate at full capacity, which I take to be five years, is very nearly correct. Now, if it lasts only three years, the maintenance rate at which that storage battery can be contracted for is 10 per cent. In fact, I advise my company that wherever batteries are used in such a manner as that which I myself should advocate, we should be making a handsome profit if we guaranteed to keep them in order for 12 per cent. per annum.

I should like to say, in conclusion, to all engineers who propose using storage batteries, if you attempt to charge a number of cells or batteries in parallel your arrangement will most certainly come to grief. Storage batteries *must* be charged in series, or so that every cell receives an identical charge with its neighbour; they may be discharged in whatever reasonable manner you please. The result will be that your battery will be easily kept in order. But when you attempt to charge them in parallel the smallest difference of counter E.M.F. between the parallels will tend to send into one parallel the largest proportion of the current; and the result of that, of course, will necessarily be that the battery will break down at a very early date.

I have nothing more to say on the matter, except that I cordially agree with the trenches proposed by Mr. Crompton, because the brick trenches at Colchester are as sound and dry now as when they were laid down in 1883.

Mr. STUART RUSSELL: At this late hour I feel that I must Mr. Russell. confine myself to the one point in which I really take the most interest, and that is the question of conductors. Mr. Crompton has based a very great part of his argument in favour of the battery system, it seems to me, on the great difficulties with which we shall be met when we try to use underground conductors for high-potential currents. Now I think he has rather based his arguments on the failures which have been met with in America when an underground system has been tried; and he twits the advocates of the A.T. system with basing their confident tone on very slender grounds, on account of the success of this system in America having been attained by the use of overhead wires. I am not going to follow Professor Forbes in saying that in small stations of this kind overhead wires should be used; because, perhaps, the authorities may interfere with us, and we shall not get a chance of doing so. I have therefore looked up a few instances in which underground cables have been used; and just as several other speakers have pointed out with regard to batteries that we must not judge what is going to be done by them by the heavy cost of maintenance which there has been in earlier installations, so I say we must not judge of what is going to be done in the matter of cables by any of the earlier failures. Mr. Sellon referred, I think, to the very disastrous experiences which their company had had in the matter of underground cables. I would like to ask him how far back that dates, and with what class of cable, and how laid—in fact, to give a few particulars about it, instead of such a very general statement; because, as far as I have been able to see in looking up this question, the failures which have occurred with underground cables have generally been with a very poor class of cable compared to what I believe is now being used. We know that in some of the early efforts cables were insulated lightly with gutta-percha, and practically had no mechanical protection at all; or insulated with rubber tape, or with some resinous or bituminous compound; and that, when drawn into pipes, they were—even before the current was put on—in many cases so much damaged that failure was only to be expected in the shape

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Russell.

of continual short-circuits or earths. I believe that really the same result would have occurred if these cables had been used with such a moderate difference of potential as 100 volts. If we now turn to the more pleasing side of the question, namely, the successes, although the number of cases which I actually know of myself is not very large, yet I think they will give us some encouragement. We have heard (although Professor Forbes did not mention it, and I was hoping we should have had some details about it from him) of underground cable being successfully used on the A.T. system in Philadelphia. I gather from what Mr. Smith said at the recent meeting at Pittsburgh that he has some fifteen miles of underground conductors in Philadelphia, which are working with very satisfactory results on the alternating-current system. He also refers to two installations for arc lighting in that same town, one of which, of some four or five miles, he said is giving very little trouble, and the other has given none at all. And at that same meeting at Pittsburgh many of the speakers—I may say the majority of the speakers—seemed to be of opinion that the strain on the dielectric in arc light circuits was very much greater than could ever be the case with the A.T. system. Reference was made to the tremendous strain on any weak point in the insulation—which was put on by a machine like a 60-light Brush dynamo, or one of the large Thomson-Houston machines—when, by the circuit becoming broken, the magnets discharged through the line. In Europe, reference has been made to the successful use of cables at Milan, some for 1,500-volt continuous circuit, and some for 2,000-volt alternating; and I know of cables in Brussels which have been laid in a sewer there (I think, if I remember rightly, there are about five miles of them), and these are working satisfactorily with 1,400 volts continuous current. In England we have the Colchester experience, to which Mr. King has already referred, and which dates back some four or five years—to a time when, I think, we must not credit contractors or manufacturers with anything like the amount of knowledge that they have now on the subject. There those cables were laid, and, as Mr. King has told us, they worked with 1,250 volts with every temptation to break down, owing to the

faulty insulation of the batteries; and yet they worked satis-
factorily for two years, and when taken up were found to be in
perfectly good condition. At Eastbourne, the installation at
which place has been referred to as being a very trifling affair,
they have been working underground for some seven years.
I believe that, in the earlier time, they did not meet with as
much success with their cables as they might have liked; but,
profiting by their experience, they used more suitable cables for
their more recent work, and I believe I am perfectly correct in
saying that at no time since they introduced the A.T. system
have they ever had any trouble with the cables. The main cables
are about seven miles long, though really the total underground
work—so Mr. Hammond has said—is something over twenty
miles.

Mr. HAMMOND: That is at the two places.

Mr. STUART RUSSELL: I think I am about right, then, in the
length I gave of seven miles. At Silvertown we have had a satis-
factory experience of five years, which I believe is about the
longest recorded period during which underground circuits have
been worked for arc lighting without any repair ever being made
in one of the cables. Altogether there is a considerable number
of miles of cable there, and the positive and negative conductors
of arc light circuits, for fourteen or fifteen lamps each, are lying
together in the same trench, and have been so for the time
named, and yet no repair has ever had to be executed on any of
those cables. These few instances, though they do not refer to
very large or elaborate installations—nothing like what we are
looking forward to, with 100,000 lights—are, I think, very satis-
factory. If a high-tension system is used—whether it be with
alternating-current transformers, or the battery system, using
much higher potentials than Mr. Crompton has advocated, or
the continuous-current transformer system, or the combination
(to which Mr. Sellon has referred) of continuous current trans-
former and battery—I do not think the cables will be such an
insuperable difficulty, especially when we come to consider that
the successful installations are of later date than the failures to
which reference has been made—from which we may conclude now

Mr.
Russell.
that people are beginning to know something more about this subject, and to know really what to put down, and how to put it down, when they want to get satisfactory results with high-tension currents.

With regard to cost, I had intended to make some comparison between the cost of the two systems; but the time is now getting on, so I will content myself with saying that, without reducing to the £6,000 which Professor Forbes mentioned as being the figure for his overhead system, it is quite possible, I think, to reduce the cost by something like £6,000, even with underground wires—*i.e.*, to make it practically about one-half the cost of the mains of the B.T. system—simply on account of the fact that Mr. Crompton has been most liberal in his allowance of copper for these mains. He has taken about 360 ampères per square inch, I think, as his current-density; and that, as far as I know, nobody laying down a system of this kind would do.

Mr.
Fleetwood.
Mr. C. T. FLEETWOOD: I should like to make a few remarks, Sir, that will not take more than two or three minutes. In the early part of the paper Mr. Crompton said that he would call attention to the mains as being one of the chief things connected with any kind of distribution. From the time that he made this remark I have been listening attentively to hear whether there was anything like a permanent system of underground mains brought before the meeting. The last speaker took us to the United States, to Brussels, to Milan, to Colchester, to Silver-town, and then————

Mr. HAMMOND: To Eastbourne.

Mr. C. T. FLEETWOOD: To Eastbourne and Brighton. But nothing, Sir, has been brought before the meeting in reference to a permanent system for London. Turning to Table No. 1, we are told what the cost of laying 100 yards of double conductor underneath the footway of London streets would be. In reference to the systems that have been mentioned in previous papers by Mr. Kapp and Mr. Mackenzie, the former gentleman recommended a lead-covered cable placed underneath a plank; but Mr. Mackenzie condemned a lead-covered cable on account of its liability to be fused, and recommended that a trough 15 inches

square should be placed under the footway. I visited Mr. Mr. Fleetwood.
Crompton's system at Kensington the other night, and there he
has a culvert something like 18 inches square. These two last
systems are altogether out of the question for being placed under
the footway of London streets. The experience I have had during
the last fourteen years is such that I know it cannot be done.
What with coal-shoots, water and gas services, fire hydrants,
and pipes that are already there, it is almost impossible even
to get a 3-inch pipe through many of the principal thorough-
fares in London. Yet the system that the Brush Company laid
down from Belvedere Road to the City—a 3-inch pipe into
which they drew cables, which I believe are still there—has
been put aside altogether, neither has the system that was laid
by Siemens Bros. been referred to. Why iron pipes are
abandoned I do not know, unless it is on account of the cost of
insulation. I feel, Sir, that if money were spent on insulating
the wires, iron pipes are the things to use. Wires can be drawn
in and drawn out; the maintenance can be kept up without any
interference with the system. But until that is done I feel
certain there will be no permanent system of underground
electric light wires in this vast city.

Mr. W. H. PREECE: Just one word, Sir. I would make Mr. Preece.
one request. We have had some extremely interesting remarks
to-night from Professor Forbes — really about one-tenth of
what he could tell us—and he threatened that he would give
the result of his experience in America to some other Society.
Now we cannot give him another night this session; and rather
than that he should take his remarks to any other Society, I have
to ask him, in the name of this Society, to be good enough to
elaborate the remarks he has made to-night, so that when the
report of this most important discussion is printed in our
Proceedings we may have the benefit of all that Professor Forbes
may be able to give us as the result of his experience in
America.

The PRESIDENT: I will put Mr. Preece's remarks as a motion The President.
to the Society, and ask Professor Forbes to accept it as a
resolution.

The motion was put, and carried unanimously.

The PRESIDENT: I think we can hardly give a longer time to this discussion; but if there are gentlemen who wish still to express themselves, perhaps they will do so in writing. Mr. Crompton will then be able to read their remarks before he makes his reply, and they will be printed, of course, in the *Journal*. The Secretary reminds me that if any written remarks are sent in they ought to be sent in, say, before the 2nd of May.

A ballot for new members then took place, at which the following candidates were elected:—

Foreign Members:

Joseph Hollôs. | S. D. Niwa.

Member:

Professor W. H. Heaton, M.A.

Associates:

James Melville Chadwick.   James Alexander Phillips.
P. J. Charles.   John S. Sayer.
Frank Christy.   Thomas Webb Watts.
Charles Edward Grove.

Students:

Cecil C. F. Monckton. | Charles Stevens Ward.

The meeting then adjourned.

THE USE OF ELECTRICITY FOR THEATRE-LIGHTING.

By ERNEST GEORGE TIDD, Student.

The author in his introductory remarks drew attention to the numerous fires which had lately taken place in theatres, all originating behind the scenes, and attributable to the use of gas in the "flies;" and he remarked on the great advantages that accrue to the public, both in point of safety and comfort, from the application of electric lighting.

He showed that the illusionary effects produced by gas and lime light were all obtainable with the electric light, and contended that the electric light was capable of producing far greater effects, as it can be completely extinguished at will, and can instantly be re-lit by the mere turning of a switch.

There was also another consideration to take into account, viz., the expense incurred by the deterioration of the decorations of a theatre—a very heavy item in a manager's outgoings ; and he calculated that the decorations would last three times as long when the electric light was employed as under the old system of gas.

After entering into the special details and requirements of the different parts of a theatre—viz., the stage, the auditorium, with its passages, cloak-rooms, &c.—noting the precautions that should be taken in case of a breakdown, so as to avoid any risk of panic, he proceeded as follows :—

There are now in London ten theatres and music halls where the electric light is used, viz., " Criterion," " Terry's," " Savoy," " Alhambra," " Prince of Wales's," " Gaiety," " Haymarket," " Empire," " Oxford," and " Pavilion ; " and one other, viz., " Adelphi," where the light is now being installed. There are three new theatres now building in London, all of which are to be entirely lighted by the electric light; and it is a fact worthy of note that since 1881 no theatre has been built (with

the exception of the "Avenue") in which the electric light has not been used.

I now propose to give a short outline of some of the installations which I have been permitted to inspect.

First, I shall take the "Criterion" Theatre, which is, I think, one of the most satisfactory I have seen, it being an installation complete in itself, having the electrical power under its own control, and not being dependent upon leads from any other source. The installation was put up by the Edison Company at the beginning of 1884, and has continued running from that time without a single hitch of any kind. No gas is used in the theatre at all. The plant consists of three old-type Edison machines, driven by 100-H.P. engine; the engine-house, which is a little distance from the theatre, also contains the Edison-Hopkinson machines, driven by a similar engine, and used for lighting the restaurant. The dynamos are connected in arc, being attached to a special plug switchboard, as shown in this diagram. Arrangements are made so that, in the event of the engine breaking down, by plugging in at A A all the load would be thrown on to the restaurant machinery: this would of course bring all the lamps down, but there are enough that could be cut out in the restaurant by switches in the dynamo-room to enable the performance at the theatre to proceed without interruption. The engineer has to keep the difference of potential up to 150 volts. The regulating switch-board is placed in one of the passages leading off the stage. As you see from the diagram, there are ten circuits, which include resistances, which are adjusted by the sliding contact. The first circuit supplies the footlights, of which there are two rows—a white and a green—of 28 16-c.p. lamps per row. Each circuit has its own switch and cut-out, but both are regulated by the same resistance. The second circuit supplies the dome, where there are 59 10-c.p. lamps. The next supplies the lamps round the circles in the auditorium. The next supplies the wing ladders, of which there are four sets having a total of 80 lamps. The other six circuits supply the battens, each batten having a separate

resistance, so that it can be regulated apart from the others. There are 40 16-c.p. lamps in each.   There are, besides these, two more circuits regulated by the two switches at the end of the diagram, one of which supplies the dressing-rooms, passages, &c., and the other supplies some lamps which are placed inside reflectors let into the ceiling of the galleries, so as to give a downward light on to the programmes; these lamps are left at full candle-power all through the performance.   Each circuit is supplied with a cut-out.   There are about 700 lamps in the whole installation.   The resistances are under the stage in a large slate case, which is well ventilated.   All the leads, &c., are attached to the back of the switch-board, there being a narrow passage behind it to effect this.   The wing ladders have flexible wires attached to them, which are passed through holes in the stage and fastened to terminals underneath.

I will now take " Terry's " Theatre, which is one of the most recently built.   The fittings and wiring were the work of Messrs. Verity, of Regent Street.   The current is supplied from the Grosvenor Gallery.   At first the supply was anything but regular, but I was told that it has lately been better.   I shall not renew the question of direct v. alternating currents controversy, but shall let facts speak for themselves.   The theatre is supplied with gas, which is always kept at a blue flame.   There are two Ferranti converters, which are placed in a fire-proof compartment near the roof.   The switch-board is placed on one side of the stage, and consists of 10 switches and fuses arranged as shown in diagram.   The switches marked " Front," " Dressing-Rooms," " Auditorium," and " Wings " work straight through the lamps; the other six work through adjustable resistances.   The resist-ances are contained in a box measuring only about 18 in. × 12 in. × 8 in., and it is placed at the side of the switch-board; each set consists of 12 carbon rods, the tops and bottoms being alternately connected by brass strips.   Along the top row of strips a brass slide works so as to include 2, 4, 6, 8, 10, or 12 of the carbon rods in the circuit; with all twelve of them in circuit there is a barely visible glow in the lamps.   The total number of lamps is about 400, and they are distributed as follows:—

30 8-c.p. lamps in a circle round the dome.   There are no
lamps in front of the circles, all being placed at the back.

9 16-c.p. lamps in the pit.

13   „   „   „   dress circle.

8   „   „   „   upper boxes.

16   „   .   „   „   gallery.

The lamps on the stage are as follows :—

Float ...   .   21 16-c.p. lamps ;

2 rows of battens   ...   30   „   „   each row ;

1 row of battens   ...   22   „   „

2 proscenium lights...   8   „   „   each ;

and 96   „   „   distributed over the
wing ladders.

In all the passages a lamp-holder and gas bracket are combined.
The connection for the wing ladders consists of a plug fitting
between two contacts which are fixed to a slate base, the latter
being fastened to one of the beams under the stage.

I will next take the "Oxford" Music Hall, which is fitted up
very similarly to Terry's, having been done by Messrs. Verity, and
the current being supplied from the Grosvenor Gallery.   The
installation has only just had its trial run, so is hardly in proper
working order yet.   The wiring has all been done in a very
perfect manner, and I should think is as near the fire offices'
regulations as possible.   Every bracket has a cut-out on each
side of it.   The switch-board is close to the stage, and consists
of 12 switches, and in the centre a circular slide for the resistance.
All the stage lights take up seven of the switches, and they all
work through the same resistance, so that they can only be raised
and lowered simultaneously.   One of the remaining switches is
for the dressing-rooms, two for the side bars, and the other two
for the auditorium.   One controls 24 pendants of three lamps
each, which are switched off during the performance, and the
other controls six pendants of three lamps each, which are always
left full on.   There are four battens of 30 8-c.p. lamps each,
two proscenium lights of 10 lamps each, six wing lights of 10
lamps each; these are connected by means of plugs, in a similar
manner to those at Terry's, only the slate base to which the

contacts are fixed are placed in an iron box filled with cement. The footlights contain 50 lamps, which are connected to bare wires laid in grooved slate and covered up with Berry's fire-proof cement. The slate is laid on an ironwork frame, and insulated from it by means of sheet india-rubber. The whole installation contains 530 lamps. The transformer-house is in the basement; the current enters at 2,400 volts, and leaves at 100 volts.

Now to take the "Gaiety" Theatre, to which I have before referred. There is a complete installation consisting of a 16-h.p. compound engine, driving two Goulden-Trotter dynamos; but as this plant is placed under the auditorium, where the vibration is felt, it is only used in case of a breakdown at the Grosvenor, from whence the current is obtained to light the theatre. Steam is always kept up during the performance, and for the late burlesque of "Frankenstein" the current was used in the last act to run 16 500-c.p. incandescent lamps. The transformer-room contains three transformers—two 250-light 100-volt, and one 150-light 50-volt—connected to the distributing switch-board: this consists of four double-pole switches and cut-outs, which supply—

| Stage No. 1. | Auditorium. |
| Stage No. 2. | 50-volt circuit. |

All the switches are arranged with a double set of contacts, so that the lamps can be instantly switched from the transformers on to the dynamos. At the prompt side of the stage there is another switch-board with five sets of sliding contacts, by which resistance is inserted separately into the four batten circuits of 52 lamps each and the float of 54 lamps. The total number of lamps in the theatre is 450. The installation was fitted up by, and is under the charge of, Messrs. E. L. Berry & Co.

Another installation the stage of which was fitted up by the same firm is on very similar lines to the foregoing. This is the "Empire" Theatre. The current is obtained from the Grosvenor Gallery, although there is a complete set of plant there, which had to be stopped on account of some complaints made about the noise. There are 12 sets of wing ladders, each containing 15 lamps; one set of floats of 66 lamps; two proscenium lights, each

containing 25 lamps; and six battens of 40 lamps each. All the lamps are governed from a switch-board of the same type as the "Gaiety." There are 536 lamps on the stage, and about 500 in the auditorium, making a total of about 1,036 lamps. The noticeable thing about this installation is the artistic arrangement of the lamps in the auditorium. In front of the gallery there are 18 brackets of 2 lamps each; hanging over second circle there are 18 brackets of 3 lamps each; in front of this circle there are six brackets of 4 lamps each; and between them are placed five brackets, each containing 3 gas-burners, which are kept alight. In front of the first circle there is a close row of single lamps. The dome contains 18 lamps, besides gas jets.

I next turn to the "Savoy," which, if taken in order of perfection, ought to rank first, as it was not only the first to be lit by electricity, but it contains more lamps than any other theatre. I owe the particulars of this installation to our Chairman's kindness. The installation was first used on 10th October, 1881, and was put up by, and is still under the charge of, Messrs. Siemens Brothers, having worked since the beginning thoroughly satisfactorily. The stage is lighted by 715 lamps, and there are 601 blue ones which were used in the night scene of "Pinafore." The lamps are nearly all on a shunt-wound dynamo, and they are regulated by inserting resistance in the field magnets—a very much more efficient arrangement than that generally employed, the disadvantage being that all the lamps on the dynamo are simultaneously lowered. The battens are made entirely of incombustible material. The wing ladders terminate in triple plugs, which are inserted into contact-pieces on the stage. The auditorium is lighted by three-branch brackets arranged round the tiers, containing in all 150 lamps; the corridor, &c., having 165, and the dressing-rooms 148. The total number of lamps is 1,178, and the 601 blue ones, making 1,779. The lamps are arranged in parallel on three circuits, one of which is in parallel with a circuit containing two arc lamps which are used outside the theatre: these take 40 ampères. The plant consists of three Siemens dynamos of the S.B. type, each giving 400 ampères at 93 volts. The dynamos are driven at 700 revolutions by two

Marshall portable and one Robey semi-portable, each of 20-H.P. The switch-board is placed in a corner of the stage. The lamps in the auditorium are lowered by inserting resistance in the external circuit, there being other lamps on the same circuit. Some other groups of lamps which have to be raised and lowered separately are done in the same way. The raising and lowering is effected by means of four eight-way switches. The wiring is done in such a manner as to minimise the risk of fire, the leads nowhere crossing, and being kept a considerable distance apart. There are safety fuses in each branch main; no double fuses are used, but each main lead from the engine-room is supplied with one. There are pilot lamps in the engine-room to each circuit. No gas is used on the stage at all. The other theatres are only partially lit on the stage.

The " Prince of Wales's " has a set of plant consisting of a 12-H.P. Clark gas engine, but having one of their new back covers giving 16-H.P., a Siemens D1 dynamo, and 108 E.P.S. cells of the 31-L. type, of which three 59-plate cells have been in four years. Lately leads from the Gosvenor Gallery have been brought into the theatre, and the current is now obtained from there, the plant now being used only in case of a Grosvenor break-down. The float contains 54 lamps, and there is one batten of 16 lamps. The front of the house, offices, &c., contain 346 lamps; they are all 100 volts and 16-c.p. There are two 150-light converters. The total number of lamps is 416.

The " Alhambra " has just had the electric light installed in the auditorium, and partially on the stage. The plant consists of two Crossley 9-H.P. gas engines, driving two Edison 150-light dynamos. There are 108 Elwell-Parker accumulators, 56 of which are used to light the balcony, and the other 52 are used in parallel with the two dynamos. The only lights on the stage are the floats, containing 80 lamps, and one batten containing 60, this being only used in the last scene of the ballet " Enchant-" ment." The whole installation contains 424 lamps; the only gas in the auditorium being in the dome.

The " Haymarket " is supplied from the Grosvenor, but no lights are used on the stage with the exception of a temporary

festoon used in one act of " The Pompadour." The installation contains about 120 lamps in the auditorium and two arc lamps used outside the theatre.

The last place, viz., the " Pavilion " Music Hall, has no electric light at all on the stage, but there are about 300 lamps distributed over the auditorium. The plant consists of two 16-H.P. Otto gas engines, and two Edison-Hopkinson machines, a few accumulators being used for the sake of regulation.

Taking a summary of the theatres and music halls, we find that six work from the Grosvenor, and four have their own plant. Of the six Grosvenor installations, four of them keep gas always burning, while the other two have electrical plant of their own always in readiness.

In the foregoing remarks I have endeavoured, as far as my ability would allow, to bring before you the present state of electric lighting in our London theatres; and, in conclusion, I would remark that the successful lighting of our theatres and public places by electricity may be looked upon as the forerunner of the universal application and use of this light, wherever practicable, in our houses, and more particularly in the mansions of our nobility, &c., where there is a large wealth of paintings and historical portraits, documents, &c.; for, so surely as its efficiency and comfort is seen and felt in our places of public amusement, so surely will it follow and be enjoyed in the homes of our people. In fact, as it has been said, " Nowadays nothing can be done " without advertising," we may look upon the theatre-lighting as a huge advertisement of the electrical profession generally.

ABSTRACTS.

T. GRAY—APPLICATION OF THE ELECTROLYSIS OF COPPER TO THE MEASUREMENT OF ELECTRIC CURRENTS.

(Phil. Mag., Vol. 25, No. 154, March, 1888, pp. 179–84.)

As a result of the experience gained in a vast number of measurements made for standardising Sir Wm. Thomson's new standard electric balances, there is no doubt that the constant of an electric-current instrument can be obtained with certainty within a twentieth per cent. of absolute accuracy by the electrolysis of a copper salt. In the method used the density of the solution of copper sulphate varied between 1·15 and 1·18 ; the solution was generally prepared of a density of 1·18, and then 1 per cent. of sulphuric acid was added. The baths used were of such a size as to allow of 8 cubic centimètres of solution for each square centimètre of electrode surface. Before placing the electrodes in the baths they were cleaned by being held against a revolving cylinder covered with fine glass paper, any dust being brushed off by a piece of clean silk. After the electrolysis, the plates were removed from the bath, rinsed in very slightly acidulated water, and placed for a few moments in a shallow dish of pure water; they were then dried by pressing between sheets of blotting paper, and gently warmed before a fire.

An inspection of the tables and curves accompanying the paper show that the effect of temperature changes about 35° C. is very marked, while about 12° C. it is not important. The apparent electro-chemical equivalent of copper varies directly with the current-density at a given temperature; or for a constant size of cathode per ampère it varies inversely with the temperature. Thus at 12° C. the electro-chemical equivalent appears to be 0·0003287 for a current-density of 1 ampère per 50 square centimètres of cathode surface, and 0·0003278 for 1 ampère per 300 square centimètres ; with a cathode surface of 50 square centimètres per ampère it is 0·0003288 at 2° C., and 0·0003282 at 35° C.

E. van AUBEL—INFLUENCE OF MAGNETISM AND TEMPERATURE ON THE ELECTRICAL RESISTANCE OF BISMUTH, AND ITS ALLOYS WITH LEAD AND TIN.

(Phil. Mag., Vol. 25, No. 154, March, 1888, pp. 191–201.)

The wires experimented upon were obtained partly by casting the metal and alloys in capillary tubes, with all precautions, and partly by drawing the metal. A very powerful electro-magnet was used to produce the field, a current of 28 ampères being employed to excite it. The resistance was measured by means of a Thomson bridge, with the aid of a dead-beat Siemens & Halske galvanometer.

From the tables of results given it would seem that different specimens of bismuth behave very differently; some show the extraordinary result of a decrease of resistance with an increase of temperature. The author examines various hypotheses to account for this anomaly; but none of them explain it satisfactorily.

On the whole, the influence of magnetism on the resistance of bismuth was more feeble in the author's experiments than in those of Mr. Righi. Magnetism always produces an increase of resistance; its influence diminishes when the temperature rises, and is more feeble in the case of the lead and tin alloys than in the case of the pure bismuth.

The resistance of compressed bismuth hardly varies with the temperature. From 16·8° to 42·4° there was a slight diminution; then an increase up to 76°, also very small.

GOUY—ELECTROSTATIC ATTRACTION OF ELECTRODES IN WATER AND IN WEAK SOLUTIONS.

According to Maxwell, when a current passes through conductors there is free electricity not only on the outside surface of the conductors, but also on the surface boundary between two conductors of different specific resistance, since the electric force must have a different value on each side.

The experiments were made in order to ascertain if this hypothetical layer of free electricity would act in accordance with the ordinary electrostatic laws. A Thomson electrometer is placed in the liquid so that the needle and quadrants are immersed. On connecting all four quadrants to the needle, the latter takes up a position which is considered as the arbitrary zero. One pair of quadrants and the needle are then connected to one pole of eight bichromate cells, and the other pair of quadrants to the other pole. There is immediately a considerable permanent deflection, which disappears on interrupting the circuit. The nature of the liquid has no immediate effect, though the production of gas by electrolysis interferes with the phenomenon, and therefore it is best observed in the case of distilled water, which only evolves gas after some time.

The liquids used having a very high specific resistance, the opposite quadrants are almost at the same difference of potential as the poles of the battery; and it is this constant P.D. which causes the phenomenon, and not the variable intensity of the current. The deflection does not alter on reversing the battery connections; it is inversely proportional to the distance of the needle from the quadrants; it is proportional to the square of the number of cells used; it changes its direction when the connections to the quadrants are reversed. In short, the results are quite similar to what would be obtained in the ordinary way with the electrometer in the air instead of in a liquid. The deflections are, however, very much greater than would be obtained in air; the deflection in distilled water is about eighty times greater

than in air. The author considers that this ratio of the attraction in a liquid compared with that in air is comparable with the specific inductive capacity of dielectrics.

G. BERSON—EFFECT OF BLOWS ON THE MAGNETISATION OF A STEEL BAR.

In the case of a freshly annealed bar, placed at right angles to the magnetic meridian, the effect of blows is to diminish the magnetic moment of the bar, the moment tending towards a positive limit, which, for a given bar, depends on its initial value, on the temper, and on the force of the blows. The effect of successive blows gradually diminishes, this decrease being the more abrupt the greater the original magnetism, the softer the temper, and the stronger the blows.

A neutral bar placed in the same position has a progressive magnetic moment produced in it by blows. When a bar in such circumstances receives a number of blows of increasing force, the mean magnetisation is sensibly the same as if it had received an equal number of blows all of the same force as the last and strongest one. If y is the magnetic moment, h the height from which the monkey falls, a and b constants, then $y = \dfrac{a\,h}{h+b}$. When a bar has been magnetised by a succession of blows, on reversing it end for end, it may be demagnetised by one single blow.

Generally speaking, if a magnetised bar is placed in a uniform field so that the component of the field parallel to its axis and the demagnetising force are in the same direction, the bar is always more or less demagnetised by blows. If the two forces are opposed, a strongly magnetised bar will be partly demagnetised, while a weak magnet will be further magnetised, accordingly as the one or other force is predominant.

The results of the experiments lead the author to the conclusion that steel is a heterogeneous body consisting of several kinds of molecules with varying degrees of coercive force. The effect of a series of blows of fixed value will only affect those molecules for which the algebraical sum of the moments of the couples acting upon them is below a corresponding fixed value.

E. BOUTY—CONDUCTIVITY OF CONCENTRATED NITRIC ACID.

The addition of water to fuming nitric acid increases the conductivity almost in proportion to the quantity of water added. The addition of small quantities of strong sulphuric acid or of anhydrous phosphoric acid produces the same result, probably by the formation of electrolytic compounds of the two acids. It would appear probable that the solution of nitric acid does not contain the same electrolyte at all stages; the polarisation varying for the several stages, as well as the nature of the gases given off at the negative electrode.

'E. DUTER—PASSAGE OF AN ELECTRIC CURRENT THROUGH SULPHUR.

Experiments carried out with a glass tube containing crystallised sulphur which could be heated in a sand bath, and into which two platinum electrodes passed, proved that the platinum was attacked by the sulphur and became polarised. To avoid this action the platinum electrodes were replaced by others of pure gold. The tube containing the sulphur was then connected up in series with nine large Leyden jars and with a copper voltameter provided with platinum electrodes; the jars were charged by a powerful induction coil. So long as the sulphur did not boil, nothing was observed; but as soon as the boiling point was reached, bubbles of oxygen were observed on the electrode in the voltameter, proving conclusively that the current was passing through the circuit, and therefore through the sulphur. After eight hours, a little more than one milligramme of copper had been deposited on the platinum electrode —a quantity which corresponds with a current of about one eight-thousandth of an ampère.

ADER—THE PHONO-SIGNAL AS ADAPTED TO SUBMARINE TELEGRAPHY.

A telephone placed directly in connection with a cable gives out no audible sound, because the number of vibrations of the diaphragm falls below 20 per second, which is the lowest limit for the human ear.

Suppose now that the current from the cable, instead of passing directly into the receiver, passes first through a vibrating or rotating commutator which will interrupt the current a great number of times per second. On sending through the cable Morse signals, consisting of dots and dashes, without reversing the current, sounds can be heard in the telephone; they will be more or less prolonged and more or less powerful, and will enable the dots and dashes to be readily recognised.

A cable can, however, be more readily worked by means of currents alternately positive and negative. Such currents applied to the above-mentioned apparatus would not allow the positive to be distinguished from the negative. The arrangement has to be modified to meet this case of alternate currents. The commutator sends the current from the cable alternately into each fork of a branched circuit, comprising two telephones, one of which is held to each ear. At a point of the branched circuit near to the earth connection are two batteries, one in each branch, so joined up as to send their currents in the same direction. When the commutator is in motion it sends the current from the cable alternately through one or other branch. If it is a positive current which is sent into the left-hand branch, it meets a local current in the same direction, and the two are added; if into the right-hand branch, it meets an opposed local current, and is diminished or reduced to zero. Each positive

current from the cable will therefore be audible in the telephone held to the
left ear, inaudible in the right ear; the converse will, of course, take place with
the negative currents from the cable, and the signals can be read in this way.
In order to render the difference between the two currents more marked, the
author proposes to use two commutators in place of one, each commutator
giving out a distinct note: the high note will then be heard in one ear and the
low note in the other. The effects of earth currents may be eliminated by
using an opposed battery. Ordinary keys can be used for transmitting, but it
is preferable to use keys which send a discharging current into the cable after
each signal. The phono-signal may be used for duplex working. On account
of its great sensitiveness, cables of smaller section may be used, thus greatly
reducing the first cost.

E. WIEDEMANN and H. EBERT—EFFECT OF LIGHT ON ELECTRIC
DISCHARGES.

The first observation of the phenomenon that rays of violet light favour
the discharge of electricity in vacuum is due to Hertz. The authors have
reinvestigated the phenomenon, making use of modified apparatus. The
electrodes of a Holtz machine were connected to the terminals of a micrometer
spark-discharger, to which were also connected, as a shunt, the terminals of the
experimental tube. This tube was provided with taps for exhaustion, by means
of a mercurial pump, and for admission of any gas with which it was desired
to experiment. The leading-in wires ended in platinum knobs. Opposite to
these knobs was a plate of quartz let into the glass tube; and through this
window the light from an arc lamp—which is rich in violet rays—could be
projected.

In making an observation, the knobs of the micrometer spark-discharger
were set at such a distance apart, generally about 3 mm., that a discharge
would just fail to pass in the vacuum tube. On throwing the light on to the
"passive" space a discharge took place, and continued, even though the
knobs of the micrometer discharger were brought still nearer together.

It was also observed that this action of the violet rays is not transient in
merely starting the discharge, but affects it during the whole time it takes
place, and entirely modifies its character. The discharge, which is irregular
and broken when the path is unilluminated, becomes quiet and regular as soon
as the light is turned on.

A telephone placed in the circuit proved to be a very delicate means of
testing the phenomenon; when the path of the discharge was illuminated the
telephone gave out a high clear note, which became simply a noise on cutting
off the light.

The experiments further showed that the positive and negative discharges
behaved in a totally different way. By separating the knobs in the experi-
mental tube to a distance of 20 mm., and by taking suitable precautions, it

was possible to throw the light from the arc lamp on the lower electrode only. When this was the negative one, as already mentioned, the discharge became much more regular under the effect of the light; but when positive, little, if any, difference was observed. The action of the violet rays on the negative discharge depends on the degree of vacuum, on the kind of illumination, and on the nature of the residual gas. On steadily decreasing the pressure the force of the phenomenon at first increases, and then diminishes.

By projecting the light from various parts of the arc on to the experimental tube, it was noticed that it was rather the intense light from the crater of the positive carbon which produced the most effect, and not the rays from the arc itself. It was also observed that the light had to fall on at least one electrode, and that it was not sufficient for it to pass through the intervening space. The experiments with hydrogen are very striking, the discharge being a deep red when unilluminated, and changing to a whiter hue when the light was turned on. It is noteworthy that with carbonic acid gas it is not only the violet rays of the spectrum which are active, but that white light can produce the same phenomenon.

The authors consider that the explanation of the phenomenon is to be looked for in the absorption of the gases by the electrodes, this absorption rendering the gaseous films more dense.

LIST OF OTHER ARTICLES

ELECTRICITY AND MAGNETISM,

Appearing in some of the principal Technical Journals during the month of APRIL.

(Philosophical Magazine, Vol. 25, No. 155, April, 1888.)

H. W. WATSON—E.M.F. in Moving Conductors. W. W. H. GEE and H. HOLDEN—Change of Density of an Electrolyte at the Electrodes. Professor S. P. THOMPSON—A Modified Water-dropping Influence Machine. H. TOMLINSON—Effect of Magnetisation on Thermo-electric and other Physical Properties of Bismuth. T. H. BLAKESLEY—Method of Determining the Difference between the Phase of Two Harmonic Currents of Electricity having the same Period.

PROCEEDINGS OF THE ROYAL SOCIETY.
(Nature, 12th April, 1888.)

Dr. C. R. ALDER WRIGHT and C. THOMPSON—Voltaic Circles producible by the Mutual Neutralisation of Acids and Alkalies. F. GOTCH—Electro-motive Properties of the Electrical Organ of Torpedo Marmorata.

(Journal Télégraphique, Vol. 12, No. 4, April, 1888.)

Dr. ROTHEN—Telephony *(continued)*. Dr. ZETSCHE—Duplex Telegraphy by the Method of Dividing the Two Bobbins of the Receiving Instrument *(continued)*.

(Journal de Physique, Vol. 7, April, 1888.)

RIGHI—Electrical Phenomena produced by Radiations. W. OSTWALD—Electro-chemical Researches. P. WALDEN—Molecular Value of Salts deduced from the Conductivity of their Aqueous Solutions.

(Bulletin de la Société Internationale des Electriciens, Vol. 5, No. 46, March, 1888.)

SÉLIGMANN-LUI—Mr. Ader's Plan of Transmission on Submarine Cables. MASCART—Measurement of Illumination. J. W. HILL—Electric Lighting of the Grand Central Railway Station of Cincinnati.

(*Comptes Rendus, Vol.* 106, *No.* 15, *9th April,* 1888.)

F. LUCAS—Solution of Equations by Electricity.

(*Vol.* 106, *No.* 16, *16th April,* 1888.)

A. STOLETOW—A Kind of Electric Currents produced by the Ultra Violet Rays of the Spectrum. A. BERGET—Variation of the Heat Conductivity of Mercury with Temperature. C. POLLAK—New Arc Lamp based on the Expansion of the Conducting Wires.

(*Vol.* 106, *No.* 17, *23rd April,* 1888.)

E. BECQUEREL—Note on Mr. Stoletow's Paper (see above). P. GERMAIN —A New System of Communication by means of Telephones with Trains in Motion.

(*Vol.* 106, *No.* 18, *30th April,* 1888.)

J. and P. CURIE—New Electrometer with a Quartz Bi-Prism.

(*La Lumière Electrique, Vol.* 28, *No.* 14, *7th April,* 1888.)

C. E. GUILLAUME—Thermo-electric Couples. G. RICHARD—Submarine Boats. K———E—Krause's Lightning-Discharger for Telephonic Circuits. C. REIGNIER and P. BARY—Theory of Transformers. E. DIEU-DONNÉ—Electric Light at the Hotel de Ville, Paris. W. FRITSCHE— Central Stations for Electric Lighting.

(*No.* 15, *14th April,* 1888.)

W. C. RECHNIEWSKI—Central Stations for Electric Lighting. A. PALAZ—Electric Light in Railway Stations. C. DECHARME—Relation between Magnetism and Mechanical Actions. C. E. GUILLAUME — Thermo-electric Couples or Thermopiles. P. H. LEDEBOER— Physical Society's Exhibition.

(*No.* 16, *21st April,* 1888.)

W. C. RECHNIEWSKI—Central Stations (*continued*). E. MEYLAN—New Types of Dynamos. P. H. LEDEBOER—Standards of E.M.F. E. ZETSCHE — Bohmeyer's Electric Clock. E. DIEUDONNÉ — New Photometric Arrangement. J. LUVINI—Theories of Atmospheric Electricity. E. SEMMOLA — Electricity produced by Condensation of Steam.

(*No.* 17, *28th April,* 1888.)

C. REIGNIER—A Disc Dynamo. A. PALAZ—The Conditions of Equilibrium of Telephonic Wires and of Phosphor-bronze Wires. J. LUVINI— Theories of Atmospheric Electricity (*continued*). E. DIEUDONNÉ— Electric Traction on Railways. W. C. RECHNIEWSKI—A New Arc Lamp. E. MEYLAN—Electric Lighting at the Palais de l'Industrie.

(*Annalen der Physik und Chemie, Vol.* 34, *No.* 5, 1888.)

S. SHELDON—Alternate Currents and Electrolytes. H. HERTZ—Effect of a Plane Electrical Vibration on a Neighbouring Circuit. F.

AUERBACH — The Production of the Dynamo - electric Current. **S. HENRICHSEN**—Magnetism of Organic Compounds. **A. POEPPL** —Mathematical Theory of Gaseous Discharge.

(*Beiblätter, Vol.* 12, *No.* 4, 1888.)

G. ADLER—A New Method of Calculating the Attraction experienced by a Conductor in an Electrical Field. **J. POPPER**—An Alternate Current Apparatus to Replace an Induction Coil for Measuring Purposes. **W. HAMPE**—Electrolytic Conductivity of the Halogen Compounds. **J. D. OTTEN**—Conductivity of Fatty Acids, and the Action of Temperature thereon. **J. POPPER**—A Standard Daniell Cell. **L. von ORTH** —A New Method of Testing Batteries in Work. **L. HERMANN**— Polarisation of Muscles and Nerves. **M. BELLATI and S. LUSSANA** —Electrical Properties of Selenides of Copper and Silver. **A. BATTELLI** —Thermo-electric Properties of Alloys. **M. TRAUBE**—Electrolytic Production of Peroxide of Hydrogen at the Anode. **HARTMANN and BRAUN**—A New Galvanometer. **A. WASSMUTH and G. A. SCHILLING**—A New Method of Determining the Constant of a Galvanometer. **A. von ETTINGSHAUSEN**—Absolute Diamagnetic Determinations. **L. PFAUNDLER**—New Apparatus illustrative of Magnetic Induction. **G. FERRARIS**—Experiments with Transformers. **A. HEYDWEITER**—Galvanometric Measurements of Ruhmkorff Induction Coils. **A. RIGHI**—Some New Electric Phenomena Produced by Radiation.

(*Elektrotechnische Zeitschrift, Vol.* 9, *Pt.* 7, *April,* 1888.)

Dr. H. WEDDING—Relation between Conductivity and Texture of a Wire. **Dr. R. BÖRNSTEIN**—A New Electric Meter. **Dr. A. DENZLER**— Dynamos of the Zurich Telephone Co. **Dr. W. A. NIPPOLDT**—Calculation of Lightning Rods. **T. SCHWARTZE**—Use of Dynamos for Telegraphy.

(*Part* 8, *April,* 1888.)

Dr. L. WEBER—Atmospheric Electricity. **Dr. F. AUERBACH**—Dynamo-electric Researches. **Dr. F. DEHMS**—The Shape of the Conductor for Electric Cables. **O. CAUTER**—Santana's Duplex Method.

JOURNAL

The One Hundred and Seventy-ninth Ordinary General Meeting
of the Society was held at the Institution of Civil Engineers,
25, Great George Street, Westminster, on May 10th, 1888
—Mr. EDWARD GRAVES, President, in the Chair.

The minutes of the previous meeting were read and approved

The names of new candidates were announced and ordered to
be suspended.

The following transfer was announced as having been approved
by the Council:—

From the class of Associates to that of Members—

George Simpson.

The PRESIDENT: Before we commence the business announced
for this evening, there is a painful duty devolving upon me to
perform. The hand of death has been busy amongst us of late.
At the last meeting I had to announce, to the regret of all, the
demise of an old member of the Society and a sometime member
of our Council. Now it is my melancholy duty to say that the
designer of the first successful submarine cable has been early
followed to the grave by the designer of the first Atlantic Cable.
Sir Charles Bright, our immediate Past-President, known to every-
one by the reputation he early acquired in connection with the
enterprise of spanning the Atlantic with a submarine cable, and of

proving that there was no limit of distance to the success of sub-marine communication, has passed away. His cable had but a short life—faults of construction and other circumstances led to its early failure—but it showed what could be done, and it taught those who persevered in the same path afterwards both what was neces-sary and what should be avoided. Associated with the Magnetic Telegraph Company at an early period in its career, he was an active agent in securing its development, and for it he invented the bell telegraph and other apparatus of great use. In India, in Persia, in South America, in the Mediterranean, in the West Indies, and in other parts of the globe he tended to spread sub-marine telegraphy throughout the world. On the morning of this day week he died suddenly; on Monday last he was buried. In him we lose not only a member of much eminence, whose name will be for ever associated with one of the greatest achievements of electrical engineering, but those who were personally acquainted with him, as I was for more than thirty years, lose also a genial and kind-hearted friend. I hope, therefore, that I am justified in asking you to vote the following resolution:—"That an expression " of our deep regret for his loss, and our sincere sympathy with " Lady Bright and the members of Sir Charles Bright's family in " their bereavement, be agreed to; and that the Secretary be " instructed to convey an expression of the same to Lady Bright."

The motion was carried unanimously.

The following paper was then read:—

ON FIRE RISKS AND FIRE OFFICE RULES.

By W. H. PREECE, F.R.S., Past-President.

We as electrical engineers cannot shut our eyes to the fact that the various applications of electricity are surrounded with dangers which become of serious moment if neglected; while, on the other hand, we are perfectly aware that they can be made harmless if we take care to direct the agency we employ in proper channels. We cannot endorse the oft-repeated statement that the electric light is an absolutely safe light. All systems of artificial illumination are dangerous, but of all systems of artificial illumination there is no question whatever about this,

that electricity can be made the least harmful. But we must remember this—that in all systems, wherever there is the greatest danger there is always the greatest safety, for the simple reason that where there is great danger there you always have proper precautions taken.

.I was one of the first to sound the alarm of the danger of electricity in a letter to the *Times* in the year 1880, and the result of that letter was to bring about all those busy promoters of electric lighting that were then so prevalent in this country.

Now the elements of safety are, first, a knowledge of the dangers; secondly, the character of the dangers; and, thirdly, the antidotes to those dangers. The first question that I propose to ask and to answer is this, Where we have systems of this kind full of danger, are rules and regulations necessary? There is danger connected with the burning of a candle, but no insurance office has ever issued a rule that you are not to burn a candle at both ends at the same time; nor has any insurance office told us that cats must not be allowed to knock over petroleum lamps, and that it is foolish to place them under the bed. As regards gas, we are not told that pipes must be large enough to convey the necessary quantity of gas, and that gas, if allowed to leak, is alike offensive and dangerous. All these things are self-evident, and nobody would for one moment dream of issuing rules to meet self-evident dangers. But even gas is accompanied with certain dangers. There is a great personal friend of mine living not very far from me in Wimbledon. He is a very wealthy man, who does not hesitate to spend £300 or £500 upon a picture, and if his wife were to take a fancy to a new South African diamond he would not hesitate to spend £1,000 upon it; but whenever I say to him, "Why do you not light up your house by "electricity? see how lovely it is," he shakes his head, and says it is too expensive—"Look at depreciation; look at interest." He does not take the cost of depreciation into account when he buys the wife a piano, nor does he consider interest when he gives her a birthday present of £1,000. Now this gentleman has his house lighted with gas; he has a hall beautifully decorated, upon which

he spent some hundreds of pounds. Only last Saturday there was a terrific explosion. In that lovely hall there was a gasalier with one of those horrible telescopic joints, which had been pulled down: the gas escaped; the servants smelt the gas, and, as servants generally do, came down with a lighted candle. The result was an explosion, and my friend will have to spend nearly twice as much money to repair the damage done as he would have spent if he had lighted his house by electricity.

Now the reasons why rules are necessary for electricity are these: The agency we use is novel, it is invisible, it is mysterious, it is inodorous; there are few people—there is not a man in this room who knows what it is. We know only how to handle it; but all the world at large feels that there is something weird in it they know nothing at all about, and they fear to tread upon it.

Electricity is singularly open to fraud. There is nothing so easy as to deceive a person ignorant of electricity, of the character of the apparatus and of the materials used to employ it. It is a field for deceit, and it is above all things that locality where what our American friends call "cheap-jackism" is rampant. The result is that rules and regulations are absolutely essential to protect the users of electricity from the rapacity of some of those who supply electricity.

In 1881 our American friends, the Edison Company, of New York, issued a series of rules; and the Fire Insurance Board, I think it was, of New York followed their example. In 1882 the rules that were introduced in New York formed the basis of rules that were issued by the Phœnix Fire Office here, under the guidance of Mr. Heaphy; and it is a very curious thing that these rules of Mr. Heaphy's, with which we are all so thoroughly acquainted, contain even to the present day an American brand in one particular rule, and that is the use of the word "ground" as applied to an earth circuit. The Society of Telegraph-Engineers at the same time—in 1882—took up this subject, and we brought out rules that are familiar, I have no doubt, to many of you. At the same time the Franklin Institute in America did the same thing. From 1882 or 1883 things in England have been going very pleasantly indeed. The rules that were issued by

the Society of Telegraph-Engineers were virtually an assertion of certain principles.   The rules that were drawn up by Mr. Heaphy were drawn up in the interests of the fire office, and they are virtually detailed rules that have been the guidance of, I think, nearly every telegraph-engineer amongst us.   Personally I can say that I have never used any others ; every installation with which I have been connected has been specified to those rules. But last year an alteration was suggested.   Someone started a hare, and that hare has been running rather fast since then.   Not only has the Society of Telegraph-Engineers been carrying out a work that it is my duty to detail to you to-night, but others have stepped into the breach.   We have at the present moment rules issued by three independent offices, and the cry is, " Still they " come."   Nevertheless, with these numerous rules before us, and the prospect of other rules to come, we are not so badly off as they are in the United States.   In the United States there are rules drawn up, to which I referred just now, by the New York Board of Fire Underwriters.   Those rules, originally drawn up by Mr. Woodbury, have been from time to time revised, and they have kept pace with the advance in electric lighting ; they are short and concise, and, as far as the practice in New York is concerned, they meet their requirements.   At Boston the Fire Underwriters' Union have done the same thing ; there is a capital series of instructions drawn up there.   Some six States form New England, and there happens to be another insurance body with headquarters at Boston who have drawn up other rules and regulations, and these rules number as many as seventy-two. At Philadelphia there are rules and regulations.   The Franklin Institute has its rules and regulations ; and there is scarcely a town of any size or consequence in the States, with the exception, perhaps, of Chicago, that has not got its own rules and regulations. So that these poor Americans are perhaps a thousand times worse off than we are.

In the spring of last year it was suggested to the Council of this Society that the time had arrived when their rules and regulations should be revised.   The old committee was reappointed to deal with the subject.   I might tell you that the rules and

regulations drawn up by this Society were, prior to that, in considerable demand; some thousands of copies were issued, and at the very time when we were asked to reconstitute the committee a demand had arrived from one of the large provincial offices for 200 more copies. From inquiries I have made, I have not the least doubt whatever that if we take the number of installations in this country there are more installations in number—I do not say a greater number of lamps—put up under the rules and regulations of this Society than under those of the Phœnix Fire Office. Of course in London the Phœnix Fire Office has been paramount, and I think no other rules have been used; it is not so in the country. In May last this committee was formed, and did me the honour to elect me as their chairman. The very first decision come to was that our Secretary should call upon the Fire Offices' Committee and see how far any movement on our part to reconsider these rules would meet with their support and their approval. Mr. Webb went there, and informed them that " the Society had " no desire to move in the matter unless they could be assured " that their doing so would be of service to the Insurance Com- " panies generally, and would be in accordance with their wishes; " that if the offices were satisfied with any rules then in existence, " the Society were content to leave matters as they were." We were told in reply that it was desirable to make some change. It having then been decided that some steps should be taken, it was perfectly evident that it was most desirable that there should be only one set of rules in existence; and there being, as it were, two powers—the Society and the Phœnix Fire Office—I came to the conclusion that the only satisfactory method of bringing matters to a crisis and settling them satisfactorily was to get Mr. Heaphy to come and work in conjunction with our committee. Mr. Heaphy joined the Society. We formed a sub-committee of, I think, Mr. Crompton, Mr. Siemens, General Webber, and myself. We had a great number of meetings, extending through May, June, and July. We met Mr. Heaphy, and matters were going on as swimmingly as one could wish,— everything went like a marriage bell,—but unfortunately a little

cloud arose in the horizon which we did not expect.   We
found that when we brought these gentlemen connected with the
fire offices together it was something like attempting to mix oil
and water; it was a signal for a storm, and the result was a
general electrical disturbance.   The arrangements that we had so
nicely made were that the original rules drawn up by the
Society of Telegraph-Engineers should be revised to date, and be
simply generalities, and that they should be followed up by
detailed rules to be called the "Fire Office Rules," which were, as
a matter of fact, the rules drawn up by Mr. Heaphy.   But there
were certain points connected with these rules of Mr. Heaphy's
that did not meet the approval either of myself as chairman
or of members of the sub-committee.   There was not one
word to be said against the matter of these rules; the objec-
tions were to the phraseology, to the mode in which they
were drawn up, to the effect that a rule was defined clearly and
decisively, and then could be departed from with the approval
of the inspecting officer; in fact, it made the inspecting officer
an arbiter in the matter, and we rather desired to see that
the rules should be hard and fast rules that should only be
departed from under very exceptional circumstances.   The crux
that we broke upon was this: It was proposed by Mr. Heaphy
to the whole committee that the rules should be preceded by
words to the effect that "The following detail rules having been
"drawn up by the Phœnix Fire Office for electric light and electri-
"cal power installations, and having been laid before the Society
"of Telegraph-Engineers for approval, are recommended for
"general use."   Now the whole committee had not been through
those rules.   Mr. Heaphy insisted that we should come to a
decision that these rules were approved by us before we had
examined them, and as chairman of the committee I decisively
and decidedly declined to put any such proposal before
the committee.   Mr. Heaphy, I am sorry to say, left the
committee.   Now, when you are in trouble, there is always a
very good way, and generally a successful way, of getting out
of it, and that is by doing nothing at all: it is the principle
of "masterly inactivity;" and as at this time the holidays

arrived, and the British Association took up the attention of some of us, the work of the committee was left until later on in the year. In December, when we commenced again, we found that "masterly inactivity" did no good, and it was necessary in this case to carry out our determination to draw up rules of our own; so we set to work, with the result that the rules were issued in March. Certain offices object to these rules even as they are, and we have not yet succeeded in convincing all the fire offices either that they would be wise to accept our rules, or that they would be wiser to accept the Phœnix Fire Office rules. Mark you, this question of rules and regulations is entirely a question between the insurers and the insured. We can only act in the position of guides, philosophers, and friends; we cannot attempt, and never have attempted, to force rules upon anyone, and there is no such thing in drawing up rules of this kind as is sometimes complained of here of professional dictation or the interference of busybodies. It cannot be said for one moment that the action of this Society in this business has been anything but one dictated by their sense of duty. The work of a committee is an extremely difficult one. Suppose you have a committee of six men, and you have to consider six rules: you will find that every man of those six will have an amendment and an alteration to every one of those six rules, and the result is that thirty-six amendments will have to be considered. When you have to deal with twenty-three members and a committee of such members— I ought to have told you who constituted the committee. It was formed of the strongest men we could find representing science— the consulting element, the contracting element, the users. We had Professor Adams, Professor Ayrton, Mr. Crookes, Professor Foster, Professor Hughes, Sir William Thomson, for science; as representing the consulting interest we had Sir C. T. Bright, Dr. Hopkinson, Mr. E. B. Bright, Professor Forbes, Mr. Gordon, Mr. Massey, and others; as contractors, Mr. Crompton, Dr. Fleming, Mr. A. Siemens, and Major-General Webber. You could not have had a stronger committee than that; and when you come with such a committee to deal with twenty or thirty rules, where are you? Why, Sir, you know—I need not appeal to you, because

you have handled a good many committees; but those who have
not handled committees have no idea of the worry, trouble, and
bother it is to get rules and regulations of the moment of ours
through such a committee. I will just show you what kind of
work we had to do. I will take, for instance, the first rule, drawn
up by the Guardian Insurance Office or by the Westminster Office—
I am not sure which it was; but let us suppose that we are sur-
rounded by five or six members of the committee, and, as chair-
man, I read the rule: " *The dynamo machine must be placed in*
" *the engine-room* "—well, of course one member would instantly
spring up and say, " Why, where else would he put it ? "—" *or in*
" *an apartment free from and not communicating* "—another one
would say, " Well, how does he intend to connect his dynamo to
"his engine?"—"*and not communicating with any room in which*
" *inflammable material is manufactured or stored* "—another one
would at once say, " Where is the person who would think of such
" a thing ? " while three or four others would say, " Why, this rule
" is drawn up to avoid plagiarism; what does Mr. Heaphy say ? "
We should refer to his rules, and find that he says: "No dynamo,
" motor, or any apparatus for generating electricity to be placed
" in any working room of any cotton, flax, jute, or corn mill,
" or in any mill or factory of a like description, or in any
" working room or place where any hazardous process is carried
" on, or in which any hazardous goods are stored, or where there
" is special risk, unless permission to the contrary be given." Now
let us take our twenty-three members of committee. What do
they say? Rule 29: " The armatures and field magnet coils should
" be thoroughly insulated. Dynamos should always be fixed in dry
" places, and they must not be exposed to dust flyings or industrial
" waste products carried in suspension in the air; they should not
" be permitted in working rooms of mills where liability to such
" danger exists, or where any inflammable manufactures are carried
" on or any inflammable materials are stored." That appeals to
everybody, and everybody knows exactly what it is, and after
spending perhaps one day over a rule like that we are all perfectly
satisfied. That is the sort of thing that we had to go through.

Let me take first of all a few of the cases that we want to

legislate against.   A short time ago there was a paragraph
in all the newspapers that Lord Brassey's house was on fire,
and that it was due to a secondary generator.   The alarm of
fire was due to the secondary generator; but that secondary
generator was fixed in a fireproof box; it was new; it had warmed
up a little bit; the insulation smoked a little; smoke was seen to
issue out of the fissures of the box, and instantly the servants
screamed; and no doubt, like Raleigh's servants when they saw
smoke issuing from his mouth and poured water over him, the
servants of Lord Brassey poured water on the secondary gene-
rator.   That little fact has done more mischief than anything
that has happened for a long time in the minds of those who
were contemplating the use of electricity.   At the Temple
Theatre in Boston a short-circuit was formed between two
cotton-covered wires coated with paraffin.   It was seen; the
people in connection with the theatre at once took up the
boards; they saw the flame, but before they could do anything
the flame rushed through the flooring owing to the extremely
inflammable character of the paraffin, and the place was burnt
down.   Not very long ago, nearer home, at Derby, in the stores of
the Midland Railway, a short-circuit took place through using a
tin shade to a lamp.   It was one of those wretched old wooden loop
holders that I hope are now entirely out of date.   This burnt;
the wire caught fire, and it took some trouble to put it out.
Then there was a case at the Parliamentary House in Brussels—
fortunately trifling.   There was another case at the Star Theatre
at New York, another at the Porte St. Martin Theatre in Paris;
but while the number of accidents due to fire can be counted on
our fingers as regards electricity, why, the list of fires in the year
1887 in theatres and places of amusement which were entirely
destroyed is enormous, and a fearful amount of life was lost.   It
fills half a page; I cannot remember the number.   The average
was two or three theatres in different parts of the world burnt in
a month.

   Wherever we have electric lighting there is a series of risks
that we have attempted very much to protect ourselves from—
risks little anticipated by the ignorant.   In stables, who would

imagine that the ammonia that issues from the *excreta* of the animals will attack the wire and the metal staples that are used, and produce accident ?   Who would think that the mere washing of floors would lead to a fire, unless attention were strictly called to it ? and who would think that rats and mice are perhaps the greatest enemies that we have to contend with ? Then, again, there is another convenient and useful mode of fixing wires in houses while the houses are being built—that is, to bury wires in the cement.   There is no more foolish and no more disastrous practice that anybody can adopt than to bury an insulated wire in cement; and the worse the insulation the worse the mischief. Again, moisture in any shape or form must in houses sooner or later become a danger.   In underground work, in cable work, in crossing streams, in crossing oceans, water is our safeguard ; there is not an instance on record that I know of where gutta-percha has deteriorated in water.   But where you come to place india-rubber or gutta-percha indoors, subject to changes of temperature and changes of moisture, cracks are sure sooner or later to occur in the insulating medium, moisture enters, leakage takes place ; the water is decomposed by the current ; we have electrolysis ; we have the conductor attacked, and sooner or later a breakage occurs in the conductor, when an arc is formed ; the insulating medium is an inflammable hydro-carbon: it takes alight, and fire results.   It is very doubtful now whether we are adopting a wise precaution in encasing our wires so much in wood ; it is dangerous to use wood casing anywhere where the wood is subject to moisture.   I have on the table a most interesting example of this, taken from a ship.   There have been three or four cases where the wood has taken fire on board ship.   The specimen on the table is well worth your very serious examination.   The wire is undoubtedly very fair wire ; nevertheless moisture fell upon it, a leakage took place, a short-circuit of a kind was formed, the fuse did not act, and a fire occurred.   Where you have a steady leakage through moist wood of that kind, the current being of low intensity, the fuse does not act, the fire takes place, and the mischief is done.

Another cause of danger that I cannot too strongly put my foot upon as far as I possibly can, not so much on account of

its danger, but on account of the mischief that sooner or later it is sure to inflict on the lighting, is the use of mercury. I can speak now of experience of thirty-five years in the use of mercury, and I have never known a single instance where a mercury contact did not sooner or later create mischief. Unless mercury can be renewed every day or every week, or frequently, that mercury, in whatever capacity or wherever it is put, must sooner or later lead to trouble, and I have even seen the result of a fire due to a mercury contact.

Another very serious cause of accident in houses fitted up with electric light installations is the temptation which the charm and comfort of the light gives to those using it to extend their wires to different rooms, to add on lamps here and there, and eventually to call upon their conductors to carry more current than they were originally designed to carry, and, as a consequence, to produce heat and danger; but of all sources of accident there is nothing so serious or so dangerous as bad joints and bad connections. On this point of joints practice is very curious. All old stagers would say that no joint can be perfect unless it is soldered. Yet I find in one of the Boston rules these words : " Joints in wires to be securely made and wrapped with " tape ; soldered joints are preferable, but not essential." I take that to be fatal to these rules.

As regards soldering, there is a very curious difference of practice as regards the flux to be used. In one of the American rules—the New England rules—it says, " Rosin must not be used " as a flux ; " in Mr. Heaphy's rule, " Rosin should be used when " soldering ; " in our rules we say, " When soldering fluids are used " in making joints, the latter shall be carefully washed and dried " before insulation is applied." As a matter of fact, in these points we must be guided a good deal by experience, and we have found by experience that fluids make better joints than rosin. In the Post Office, where until the unfortunate rise in the price of copper we were using copper to a very large extent, we make it a rule that all joints should be made with what is known as " Baker's Fluid," which is, I believe, zinc chloride or hydro-chloric acid saturated with zinc or neutralised with something else; but,

whatever it is (I have never had it analysed), it is an admirable
flux, and makes better joints than rosin or anything else I know.
It is not wise to use a fluid to make joints in submarine cables.
There it is certainly advisable to use rosin, but the use of rosin
need not be compulsory.   All we want to do is to make a perfect
joint; and whether borax, sal ammoniac, hydro-chloric acid,
" spirits of salts," or whatever it may be, is used, as long as care
is taken, it does not matter what the flux is as long as the result
is a perfect joint.

The next great source of trouble is loose connections.   I
mentioned in this room once before that in an installation I had
a little something to do with there were no less than 6,000
places where, sooner or later, owing to the vibration of the
building and changes of temperature, the screws must shake
out and the joints become loose, and every one of those 6,000
connections sooner or later is bound to generate heat and to do
harm.   These are of course in cut-outs, and I think it is most
essential and desirable that we should avoid these loose joints
as much as possible, and that all connections should be soldered
where practicable.   Cut-outs formed the subject of a paper here
so recently that I need scarcely refer to them ; but we know very
well from experience, and from what has been said in this room,
that cut-outs are a source of danger rather than a source of safety
unless they are properly looked after.

Switches, if not properly designed and constructed, are a
source of danger from their connections becoming loose.   I have
here an extremely interesting switch : it is one of the Edison
pattern, and was put up in the House of Commons, I think, in
the year 1882.   It was very nearly the cause of another fire in
our House of Commons.   A connection at the back became loose,
heat was generated, and fortunately it happened during a time
when people were about—they are fortunately generally about
when the electric light is going—and it was seen in time ; other-
wise the switch was fixed in a handsome oak carving, and in
another hour there would have been a fire in the House of
Commons from that switch.   It is a case which merits your
careful examination.

Another great point, and one to which I have devoted recently a great deal of attention, is the question of the character of the insulating medium, or the character of the conductor used. What we want is something that shall act as an insulating medium and at the same time be non-inflammable. It unfortunately happens that gutta-percha, india-rubber, paraffin in all its forms, rosin, rosin oil, bitumen, and nearly everything is of an inflammable character. But I have recently been trying some wire that is used to a very large extent in America by the Westinghouse Company, and I was rather in hope the other night that we might have heard something about it from Professor Forbes—I have no doubt we shall hear something about it when we read the report of the remarks he did not make—that is, the wire known as Waring's. The material that is employed as the insulating medium is one of the distillates of petroleum oil. Paraffin, naphtha, kerosene—I do not know how many things—come from this wonderful petroleum that grows, according to Mendeléeff, the Russian chemist, and is absolutely growing at this very moment, in the earth. The theory that is generally accepted, of Mendeléeff, is that the moisture of the earth percolating through the crust comes in contact with the intense internal heat : the temperature is so high that the water is dissociated into its constituent parts, hydrogen and oxygen. In the earth we have iron as its largest constituent. The density of the earth is about 6, and the density of iron is something a little over 7, but it is well known that iron forms a very large feature indeed of the substance of this earth. Meteorolites when they fall to the surface of the earth are composed very often of iron. Now there is in the centre of the earth iron, carbon, and various carbides of iron ; and when the dissociated hydrogen in the nascent condition comes into contact with the carbon, a hydro-carbon is formed which takes various forms through the various temperatures it passes through, and comes to the surface in the form of petroleum. This is no theory, because this wonderful Russian chemist, Mendeléeff, has succeeded in making a kind of petroleum by this very action. This petroleum is a very remarkable material. When it comes to the surface of the earth you apply heat to it : at a certain tem-

perature naphtha and spirits come off; then illuminating oils;
then lubricating oils; and, as the temperature changes, the
paraffins come off; until at last there is a residue that will stand
an enormous temperature. This residue is, truly, a hydro-carbon;
but you will find after the lecture, from the samples I have on the
table here that I have been experimenting with, that it is scarcely
inflammable. I have tried many experiments with it, from that
useful scientific article called the homely match to a blow-pipe
flame, and I have failed in every instance, except one, in setting
this stuff on fire. It is the least inflammable material that I have
come across, and you will have some very pretty experiments
shown to you after the lecture to illustrate this. There are many
samples of this Waring's wire here.

Now these are the principal dangers that we have to meet.
We cannot say that our rules have met with very great success.
The value of a man's work is very often estimated by the amount
of criticism that it engenders; and where it is the fashion to
criticise, as unfortunately it is the fashion in this country to
criticise, everything that is done by a Government department
and everything that is done by a corporation like ours, we do not
estimate the value of the work that we have done by the number
of criticisms, but by the value of the criticism. Now during the
whole time that this rather heavy and arduous work was going
on, letter after letter appeared in the press. The members of
the press did not take the trouble to send to the Secretary of our
Society to learn what we were doing; no one, until very recently,
called on me. The result was that many silly and senseless
letters, always sent anonymously—when a man wants to say
anything sensible, he always puts his own name; if he wants to
say something he does not like, he always sends it anonymously.
I do think the admission of anonymous letters, especially into the
technical papers, is a very unfortunate practice. All these letters
were written on the assumption that the then existing rules were
perfect. I say the existing rules were very good; I cannot give
too much praise to the work that has been done by the Phœnix
Fire Office. I think the way in which the Phœnix Fire Office
rushed into the breach and accepted transformers before we

electricians did, speaks very highly for the pluck of the Phœnix
Fire Office; and there is nobody who has had such fortunate
opportunities to acquire the results of experience as Mr. Heaphy
has.   He has taken advantage of them: he has inserted the
results in his rules; his rules are revised month after month and
year after year; but, nevertheless, Mr. Heaphy's rules are not
perfect.   I should be very glad indeed, speaking for myself
personally—the committee of the Society is virtually broken
up—to see the work that we have tried to do started afresh from
the point where we broke off.   I think there ought in this
country to be only one set of rules, and those rules should be
general principles enunciated by this Society, and they should
be detailed rules which should be accepted by all the fire offices.
How are we to do that ?   I do not know, in the face of some of
the criticisms that we are subjected to.   But you know some of
the criticisms: I wish I had them here.   I did think at one time
that I would extract them and answer them.   What are you to say
to a criticism which suggests that a rule must have been written
in the Emerald Isle ?   This "Hibernian" rule was one passed and
approved by twenty-three of the most eminent members of this
Society, and yet we have an anonymous critic writing and
suggesting that it was written in the Emerald Isle.   The rule
is this: " *When conductors are carried in very inflammable*
" *structures, precautions should be taken to isolate them there-*
" *from.*"   Well, now, does it not stand to reason that when
you carry conductors through an inflammable building it is
advisable to isolate those wires from that building?   Why, at
this very moment we are carrying conductors through a very dry
wood building, of exceptionally dry wood, where a spark would
kindle the whole place into flame at once.   Is it advisable to lay
the conductors, insulated though they may be, upon this dry
wood ?   No; the rule says you must isolate them in some
non-inflammable material.   It is perfectly clear that our critic
confused the word "insulate" with the word "isolate."   The
Hibernianism was not with the Society of Telegraph-Engineers,
for their rule is plain and comprehensible, even by Irishmen.
Then it is suggested that in rule 32—in which, mark you, we

have attempted a very serious and important task—which says:
" When these [that is, transformers] are used to transform either
" direct or alternating currents of high electro-motive force—
" that is, from or to an electro-motive force of, say, 200 volts—
" they, together with their switches and cut-outs, must be placed
" in a fire- and moisture-proof structure, preferably outside the
" building for which they are required."   Now the critic asks
whether the 200 volts should not be 2,000 volts.   No ; if it were
2,000 volts it would be simply nonsense, but by putting in
200 volts what have we done ?   We have done what nobody did
in incandescent lamps, for nobody defined a filament ; we have
done what was not done in telephones, for nobody defined a
diaphragm.   But we have defined the difference between low
and high tension, and we say that all electro-motive forces of
200 volts or over shall be called high electro-motive force, and all
below 200 volts shall be called low electro-motive force ; and I
think you will find that rule of very great service and one that
ought not to be criticised in a hostile spirit.   I will not go through
any more.   I should have no difficulty, of course, from my own
point of view, in making you believe that these rules are good ;
all I can say is this—that twenty-three gentlemen have devoted
the best part of their time to put these rules on a solid, sub-
stantial basis.   But rules and regulations alone are not needed.
I do sincerely hope that something will be done to have only one
set of rules.   I look upon this set of rules brought out by the
Guardian and Westminster offices as little creditable to those
offices.   They have been drawn up with an amount of, I would
almost say ignorance, that is very regrettable.   I will not take
these rules in order.   I will only point out that there is one under
which the very worst description of wire I have ever seen could be
admitted into our buildings, and there are several of the rules
that certainly ought to be expunged.

Rules in their way may be very useful, but the precautions
that we require to protect our buildings are, first, proper design.
You never can have security unless you have efficient design.
Let your work be properly designed, let it be put into the hands
of respectable and proper contractors, and let it be subject to proper

inspection. If we can only induce thoroughly efficient attention, —if we can succeed in obtaining non-inflammable dielectrics,— if we get rid of the great trouble of jointing;—and let me now tell you that Mr. Gordon has adopted a heroic method of getting rid of joints; he has not one in all his large Whitehall Court installation of some 7,000 lights: the wire is taken from the switch to the lamp and cut to its proper length, so that there is no joint in the wire. There is an admirable rule, that took us a great deal of trouble to determine, which specifies what each system shall give in the way of insulation. I will read it to you; it is Rule 16: " The insulation of a system of distribution should be such " that the greatest leakage from any conductor to earth does " not exceed 1-5000th part of the total current intended for " the supply of such lamps." If you specify that the insula- tion of your conductors shall be 150 megohms per mile, who is to find that out when erected, and who is to test it? With switches, cut-outs, and fittings of all sorts and kinds, what wire is there that will give you 150 megohms per mile? It will give you 150 megohms per mile if tested in water, but when fixed and used I do not think there is an installation in this country that will give 150 megohms per mile; and therefore what is the use of specifying for something in the rules that cannot be carried out? If you say that the leakage from all causes shall be 1-5000th part of the maximum current, you have something that you can work with, and something that any man, however crude his apparatus may be, can test. If he has 5,000 lamps, one lamp is a capital detector for him: if the one lamp is illuminated by leakage the rule is not complied with, and the cause must be sought. But the great precaution I would bring to your notice is that of the proper education of our coming electrical engineers. It is all very well for those who have spent the best part of their life in learning what electricity is, and what electricity can do, to express their opinions and to give their opinions. Who is there who cares for opinions? Who cares twopence for an anonymous opinion in a technical or political paper? We do not want opinions; we want facts. We want facts put before us in a proper light; and we old gentlemen who come here and

dictate to you—I try very hard not to express opinions, but to give you facts : I know my opinions are not worth much to you, but my facts often are, and so I want the education of our young electrical engineers to be carefully watched, so that they shall not grow up with false ideas and false notions. The chief element of danger is ignorance, but even a greater element of danger is the terrible competition that exists at the present day. The curse of the electric light is the "jerry" builder. A man will go in and tender for a light at a price which he knows will not pay him, but he hopes to get his reward by putting in extras. Those are the sort of fellows we want to outflank by issuing rules and regulations ; we want to stop ruinous competition ; we want to stop bad work ; and we want to see that everything is properly carried out. In drawing up these rules, I think the Society of Telegraph-Engineers has only done its duty ; and I do hope and trust that this meeting, while it will not hesitate for one moment to make any just and fair criticism—we are quite prepared to answer any questions that may be put—will not be unfair in its criticism, or imagine that this Society has been actuated by anything else but a desire to put electric lighting on a proper footing, and certainly not, as has been suggested more than once, with the object of securing notoriety. The healthy, charming luxury that we all wish to see in everybody's house has so far been retarded by a struggle for existence—a struggle for existence especially from those who wish to earn a living by erecting structures ; but if they will only follow the rules and regulations that we have laid down, and comply with those issued by the fire offices, then, whatever fear there may be at the present moment, it will disappear entirely, because the sources of all danger will cease. Artemus Ward used to lecture—he had a panorama on which there was a smudge of paint here and a daub of paint there—and he said: "Ladies and gentlemen, for seven "years I did not know what this dab of paint was, but the "artist told me this morning that it is a cow, and the artist "must be right." Mr. Heaphy has told me that there never has been one single instance of fire of any sort or kind in any installation that he has inspected that has been carried out

under his rules, and I believe that to be absolutely true. Mr. Heaphy must be right. I am perfectly certain that if the rules and regulations of this Society be followed, under proper inspection, there will be nothing in this country to lead people to imagine that electric lighting is dangerous.

Mr. MUSGRAVE HEAPHY : Mr. President and gentlemen,—It was not my intention to have spoken this evening. I have to thank Mr. Preece for the very kind and flattering way in which he has spoken of the Phœnix Fire Office and myself. There is one thing Mr. Preece has said, however, that I must take notice of: it is in reference to the division that took place between the Phœnix Fire Office and the committee of the Society of Telegraph-Engineers. I think, gentlemen, that when you have heard my statement and my account of it, you will form a different opinion to that you must have formed to-night. I only hope, gentlemen, that I may have the opportunity of making my remarks at the next meeting. Mr. Preece let fall a hint in the course of his kind and courteous observations relative to the breaking off of the relations, that for his part he should be exceedingly pleased if the work that was then interrupted could be gone on with again. Is not that so, Mr. Preece ?

Mr. W. H. PREECE : Quite so.

Mr. MUSGRAVE HEAPHY : I must tell you, gentlemen, that we had long negotiations with your committee, and during all the long sittings there was not one single point of difference on any matter connected with any of the rules; we were absolutely unanimous; and all the trouble that has been thrown upon you has been, *not on account of the rules,* but on account of certain objections to one word, and that is the word "Phœnix," on those rules. There would never have been a thought of wanting new rules if this word could have been got rid of.

With regard to this question, I will not touch upon the matter of transformers except in very slight terms. You all know when transformers were about to be introduced in this country what people said ; you remember the fear and the timidity that there was everywhere in connection with them :

there was not a single company in the world that would state Mr. Heaphy.
whether they should be admitted into a building or not. The
matter came before the Phœnix Fire Office to decide, and what
did the Phœnix Fire Office do? It did not hesitate, or wait to
see whether any other office had drawn up rules on transformers,
but it decided the matter for itself; it took the risk, and a very
great risk it was. It stated *publicly* that transformers with high-
tension currents might be admitted into buildings under certain
regulations, which it gave, and that nothing extra would be
charged to the consumer. We knew what might take place
with the secondary circuits; but notwithstanding that customers
were allowed to use transformers, they were also allowed to
continue to use every other form of illuminant that they had
used before, with no extra charge of any kind made to them.
The rules have been successful, and transformers are now allowed
all over the world; and the Phœnix, up to the present, has never
had a single loss with a transformer, neither have I yet heard of
a loss when the transformer has been placed up in accordance
with the Phœnix rules. It is all very well for people to come
forward now, and bring out like rules, when the Phœnix Office
has stood all the brunt, and might have lost heavy amounts
of money if its rules had been different. At our next meeting
I will give my own account with regard to the matter Mr.
Preece has mentioned. I cannot conclude, however, without
saying that I feel assured the Phœnix Fire Office will only be
too happy and too willing to meet the Council of the Society of
Telegraph-Engineers in every way, and resume the work that
was then interrupted; provided, of course, that the point on
which the difference occurred is amicably settled.

Mr. KILLINGWORTH HEDGES: I did not intend to speak Mr. Hedges.
to-night, but after Mr. Heaphy's challenge, that I suppose refers
to all members of the Institution, I should like to say a few
words. I do not know what circumstances Mr. Heaphy refers to
about these rules; but they seem very well drawn, and very
much to the point, and ought to have been brought out two
years ago. What I cannot understand is why the Phœnix rules
should have that extraordinary word "Copyright" upon them.

**Mr.
Hedges.**

I think it should be generally known that there cannot be any copyright in the Phœnix rules; and if you will just bear with me for one moment, I will tell you how the regulations for the prevention of fire risks were first introduced. A paper was read at the Society of Arts, on May 3rd, 1882, by Mr. Thomas Bolas, in which the risks incidental to electric lighting were practically demonstrated and discussed. Previous to this, Mr. Preece had written several letters to the *Times* on the subject, and had drawn attention to dangers that might occur with electric lighting. A letter of mine on this subject was also published. Well, at the discussion of Mr. Bolas's paper, Mr. Heaphy presented some rules: I have been looking them up just now, and I see they are called the second edition, and dated February and April, 1882; but, curiously enough, I had already received rules from America, which were drawn up by Mr. C. J. Woodbury, which, though perhaps not word for word, are practically the same as Mr. Heaphy's rules. Mr. C. J. H. Woodbury was then the inspector of the Boston Manufacturers' Mutual Fire Insurance Company; and in 1882 a paper was read by him before the Cotton Manufacturers' Association, giving the results of the application of his fire risks rules, in which he refers to the work done, and the great value of the rules which had been drawn up the year previous. I submit that there can be no copyright in rules which have been imported into this country, and it is hardly generous to an engineer who has done very good service, and one of the chief authorities in the United States. I do not wish to put this matter forward to the detriment of Mr. Heaphy's work in any way, as I am perfectly disinterested in the matter; I would rather say, let credit be given to those to whom it is due, and henceforward that there should be one set of rules for all the insurance offices.

**Mr.
Human.**

Mr. H. HUMAN: I should just like to say one word, but it is rather a bold thing for me to do, in the face of what Mr. Preece has just said. Perhaps I might be allowed to say, in reply to the remarks about the Guardian and Westminster rules, that, whatever merit or demerit may attach to those rules, they were brought out for a specific purpose, and they have to a certain

extent answered that purpose. It so happened, as Mr. Preece has Mr.
Human. pointed out to-night, that things had arrived at a very unsatisfactory state. We had, for many months, been hoping and waiting for rules from this Society, and, failing their appearance, it was felt necessary that a set of rules should be brought out so as to put an end to what we all felt was an intolerable state of things. It so happened that when these rules were launched they met with much criticism, some favourable, and a good deal, no doubt, ungenerous; but the point I wish to urge is this—that in some quarters it seemed to have been thought that no office whatever had the right to issue rules of its own, but that they must adopt rules bearing the name and address of another office. Now that would amount to this—that when an office was applied to by an intending customer they were expected to hand him a set of rules bearing the stamp of another office; and it became very desirable that that state of things should be put an end to. Whatever effect these rules may have had, I believe that they have done one thing, and that is, that they have brought the offices to a sense of duty in this matter; and I must say that the rules of this Society will, I hope, help to pave the way so that we shall before long see one set of rules issued acceptable not only to the offices generally, but also to the members of this Society; and if that result should follow, then the labours of those gentlemen, which have doubtless been considerable, will not have been altogether in vain. I can say that, whatever be the end of the Guardian and Westminster rules—whether they have a long or a short life before them—the gentlemen who brought them out will consider that they have done something towards putting an end to what was simply an intolerable state of things; and I am glad to have had the opportunity of making that statement.

Mr. J. W. SWAN: Mr. President,—I think Mr. Preece has Mr. Swan. rendered us a very useful service in exposing as he has done the fire dangers in connection with electric lighting. I remember, when incandescent lamps were first shown in this room, Mr. Preece being then in the chair, that he sounded the note of alarm with regard to fire risks; and I thought at the time that he pitched the note rather high, but I am bound to say that I

Mr. Swan. do not think so now.   All that Mr. Preece has said with regard
to the fire risks in connection with electric lighting has been
very necessary indeed, not only in the interest of the public, but
also in that of the industry of electric lighting.   All the
precautions that he has spoken of as necessary to be taken are,
in my opinion, most necessary.   Mr. Preece has spoken of one of
the principal dangers as arising from ignorance.   I think so, and
from ignorance of two kinds—from ignorance, in the first place,
arising out of inexperience ; and, after that, from the ignorance
that experience cannot dispel.   When electric lighting was a
perfectly new thing, installations were put up, even by persons
of very good electrical knowledge, in a way that would never be
thought of now.   I can call to mind, for instance, a certain
electric light installation which was superintended by an electri-
cian of great eminence, and in which, as it seemed to him, due
precaution had been taken against every cause of danger from
fire.   But, as the event proved, there was something overlooked.
Wires were used in the interior of a building—naked wires
supported on ordinary earthenware insulators, the two leads not
being very far apart—and, in quite an unlucky way, a short-
circuit was produced by the trickling of water between the two
wires across a beam against which they had been fixed, and a
very serious accident was all but caused in that way.   Now that
is an instance of an accident arising from ignorance, but
ignorance entirely the result of inexperience.   A good many of
the faults that have led to accidents or dangers have been faults
of that kind, arising out of inexperience and want of foresight.
I think it would be an extremely good thing if some of the early
installations could be thoroughly overhauled with the view to the
detection of defects, and especially those which later experience
has proved to be defects, and that were not so regarded at first.

I entirely agree with Mr. Preece as to the necessity of making
all the joints that can possibly be got at soldered joints.   I think
we might with advantage even carry the practice so far as to
insist upon the cut-outs, which are now invariably attached to
the wires by means of screws, being affixed by soldering.

I also agree with what Mr. Preece has said about switches.   I

think abundant proof could be given of the extreme necessity Mr. Swan. there is to see that no cause of danger lurks there.

I feel that we are very greatly indebted to the Fire Risks Committee, who have drawn up these rules so carefully, for minimising the dangers which Mr. Preece has so forcibly reminded us of. I think that the rules should be distributed as widely as possible, and as widely insisted upon ; and that insistence should be followed up by inspection. I hope that the differences that have arisen in connection with the insurance companies will be settled, and that there will be a general agreement upon one set of rules.

Mr. MUSGRAVE HEAPHY : Certainly the insurance companies, Mr. Swan.

Mr. JOHN B. VERITY : I think there is very little left for Mr. Verity. discussion upon Mr. Preece's remarks, as we all agree with him ; but I presume we are really here to see if there is any chance of the general acceptance of the new rules issued by the Society. We are placed in a very awkward position, because it is not the Society that insures ; and the utmost the Society can do is to bring influence to bear on the insurance companies to accept their rules. If they will not agree to them, where are we ? We can all understand that, naturally enough, there must be a large amount of rivalry between the insurancies, more especially as the Phœnix Fire Office has a particularly able man. It is all very easy *now*, when others are getting to know how to do things ; but remember how matters were a few years ago. It was at *that* time Mr. Heaphy came forward and drew hard and fast lines, which had to be complied with, or the insurancies would not pass the installation. Now the surveyors of other offices are getting to know about electric lighting, and are naturally desirous of having something in which the word " Phœnix " does not appear, the result being " new rules." We seem now to have an opportunity of putting an end to this trouble, for from the remarks of the surveyor of the Guardian Office he is willing to accept a properly drawn out set of rules. Will Mr. Heaphy come in again, and let differences drop ? It is really in the interests of electric lighting, which I know he has at heart. (A laugh.) I

Mr.
Verity.

do not see anything particularly to laugh at, because it is a fact.
Let him come forward again, if he will, and meet the committee,
so that some arrangement may be made by which all the insur-
ancies may work together.   It is the contractors putting down
installations continually who best know what difficulties there are
to be overcome; and most of us who have had much experience
know that we have got on very well with Mr. Heaphy, who has
done a large amount of good in preventing demoralising com-
petition.   I think all are anxious to come to an amicable arrange-
ment, and this would be a happy termination to this very
necessary discussion.

Mr.
Manville.

Mr. E. MANVILLE: I should like, Sir, to emphasise what Mr.
John Verity has told us.

I noticed that when Mr. Preece read out the names of the
gentlemen who formed your committee to decide on fire rules
here, he told us that it was a very representative one, and was
composed of contractors and users as well as other gentlemen.
I cannot say that I heard him mention the name of any users,
and only the names of two contractors, who were very eminent
contractors, but still I think the feeling exists among us that
perhaps several more contractors might have been selected. As
Mr. Verity says, it is a question between contractors and fire
insurance offices, and I suppose everyone will agree with me that
Mr. Heaphy, owing to the really large experience he has had, is
in a better position than anyone else to gauge the dangers
arising from electric lighting, which, according to his own
statement, have never produced any bad results in installa-
tions carried out under his rules. Mr. Preece made a remark
about wires always being soldered. I think most of us would be
inclined to agree with him in this respect; but I may astonish
you, perhaps, a little by saying that a consulting engineer of
some eminence told me he did not believe in soldered wires, but
thought a twisted wire joint was the best to make.

Mr. W. H. PREECE: I hope he was not on the committee.

Mr. MANVILLE: I do not think he was.

Mr.
Verity.

Mr. JOHN VERITY: I hope that Professor Forbes will say
something about what has occurred in Boston.

Professor G. FORBES : I would really rather not. I have got nothing prepared from any notes I have taken; and, really, I think Mr. Preece has given a very good account of the manner in which the rules have been prepared in the States. Mr. Woodbury is undoubtedly *the* authority on the whole subject there.

Mr. R. E. CROMPTON : Mr. President,—I can testify to the extreme care that every member of the committee took in trying to forward the cause of electric lighting from the best point of view—that is, by so framing our rules as to ensure extremely good work.

I was one of the first persons who came in contact with Mr. Heaphy on electric lighting matters. I think I undertook almost the first job in lighting a private house, with Mr. Heaphy as consulting engineer. You have all heard of that house—Mr. Coope's, Berechurch Hall. I must say that I think that if I was in Mr. Heaphy's position, and had worked as hard as Mr. Heaphy has done in perfecting his rules, I should feel as if I *did* have a moral copyright in those rules. He has identified himself with the subject for several years past; and I think if you were to poll the electric light contractors, who have really more to say on this matter than anybody else, except the insurance companies themselves, you would find nine out of ten of them in favour of Mr. Heaphy's rules being adopted, and none others. Thus you will observe that my opinions agree very closely with those of Mr. Preece. I think it is a great pity that our Society has been forced into this position of having to stand between the competing fire offices. I addressed letters to all the fire insurance companies asking what rules they employ, and I find from the replies that the feeling is strongly in favour of the Phœnix rules; so that if the matter goes to a poll of the offices the Phœnix rules will carry the day. And now I take this opportunity of appealing to the representatives of the other fire offices who are present this evening. I ask you gentlemen to put yourselves in Mr. Heaphy's place. If you had done what he has done,—if you had identified yourselves with the subject as he has done,—would you like to have the credit taken away from you, and your name merged in among the general

Mr. Crompton.

ruck? Mr. Heaphy, in a certain sense, invented the fire office rules in this country, by identifying himself with the subject; and I do think that he has moral rights in this matter, and that what little advertising advantage is given to the Phœnix Office through Mr. Heaphy being connected with them has been fairly earned, They took the risks at the time when other people would not take them; their inspector occupied much of his time in getting the requisite details together to enable him to issue edition after edition of the rules when other offices did not do so. The Phœnix Office, through Mr. Heaphy, came to us, the contractors, and found out from us what was best to be done; and whenever we have wished for reasonable alterations in the rules we have found Mr. Heaphy always ready to yield in the matter, and hence the large number of corrections and editions that have been issued. I say that it is a matter of common fairness that you gentlemen should give way on this matter. If you can give way, the whole can be settled without troubling the Society of Telegraph-Engineers. Mr. Heaphy has said long ago that if there are still any objectionable points in his rules he is willing to alter them ; but I do not think it is fair to ask him to omit his name, or that of his office, from the rules in the preparation of which he has taken so large a part.

The PRESIDENT : Perhaps Mr. A. Siemens, as the other contractor who was on the committee, may have something to say?

Mr. Siemens.

Mr. ALEXANDER SIEMENS : The great difficulty which exists in Mr. Heaphy's rules, and which Mr. Preece has already alluded to, is the uncertainty contained in the saving clause which has been added in nearly every instance at the end of the rule—" subject " to the permission of the fire office inspector." The firm with which I am connected have never had any complaint about the working of this condition, because, as Mr. Crompton has already said, we have always found Mr. Heaphy open to conviction, as he has been following from the first the policy of consulting the contractors on electrical points, and has made his rules as little stringent as possible for really *bonâ fide* contractors. The great step in advance which I think has been made in the rules lately issued by our Society is that something definite is stated in every instance which independent people can control.

As I shall not be allowed to speak again in this discussion, I Mr. Siemens. may now say that I clearly recollect the circumstance occurring at the committee meeting to which Mr. Preece has referred. Mr. Heaphy wished us to pass a clause, the words of which were read out by Mr. Preece, saying that the Society's committee approved of certain rules, before the rules themselves had been settled, and this I think the committee reasonably declined to do.

The PRESIDENT: As the hour is growing late, and as it is The President. clear that the discussion cannot be finished to-night, it will now be adjourned until the next meeting.

The next General Meeting will be held on May 17th, for the purpose of hearing Mr. Crompton's reply to the discussion on his paper, and for finishing the discussion on Mr. Preece's paper.

An Extraordinary Meeting will be held on May 24th, when Sir William Thomson will read a paper on his new standard and inspectional electrical measuring instruments.

A ballot took place, at which the following candidates were elected :—

Foreign Member :
Captain Victor Candido Barreto, Imp. Braz. N.

Associates :

Robert A. Dawbarn.	James H. Rosenthal.
Arthur C. Devey.	Roger W. Wallace.
Henry Human.	Charles Edgar Wetton.
Philip Francis Morton, B.Sc.	Alex. Mackenzie Williams.

Students :

Leonard Melvill Green.	George Müller.

Alfred Frederick Metzgar.

The meeting then adjourned.

An Extraordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, May 17th, 1888—Dr. J. HOPKIN-SON, F.R.S., Vice-President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates for admission into the Society were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council :—

From the class of Associates to that of Members—

Reginald Belfield.

The
Chairman.

The CHAIRMAN : Gentlemen,—The Council have felt that, owing to the considerable length of time which is taken up by some of the senior members of the Society in discussing the interesting matters which are brought before you, too short a time is left at the end of the discussions for the younger members to bring forward their views.   They have found that some steps were desirable to limit the length of time taken up by individual speakers ; and by doing so they hope to improve the character of the discussions, to make them more lively and brighter, and possibly to a certain extent to limit the length, so as to make the meetings more attractive to the members, and, consequently, larger and better in every way.   It is therefore proposed that the following rule shall be adopted, rather as an experiment, but which we hope will succeed :—" That in the discussions at the general " meetings of the Society the time allowed to each speaker be ten " minutes, at the end of which period the Chairman shall ring or " sound a bell ; and if the speaker do not close his remarks " within the next two minutes, the question shall be put to the " meeting whether or not he shall be allowed to continue them ; " but the Chairman shall have authority on exceptional occasions " to suspend the operation of this rule "—the intention of that

being that the Chairman should have the full authority and the Mr.
Crompton. full opportunity of giving distinguished gentlemen who come a long distance to address the Society the length of time they may desire without any interruption whatever.

I now call upon Mr. Crompton for his reply to the discussion raised upon his paper.

Mr. R. E. CROMPTON: The long discussion which has taken place upon my paper, partly in my absence, is not a very easy one to answer in brief; but I will do my very best to compress my remarks by grouping them into various heads, and by giving one answer for all such remarks that may be so grouped.

I will at once make an explanation with regard to Mr. Hammond, who seems to labour under the idea that in speaking of his installations I referred to them as being carried out purely for advertising purposes. Nothing was farther from my thoughts. I spoke of them as ordinary installations, carried out more for the purpose of popularising the electric light than for being specimens of what an installation of a more permanent nature ought to be; and I characterised all overhead-wire installations in the same way. I must apologise to him at once for the mistake I made about Eastbourne; I do not know how I got it into my head that Eastbourne wired overhead. I had my attention more particularly directed to Brighton, where I knew a very considerable portion of the wiring was overhead, and to one in London—the Grosvenor Gallery installation—the whole of which is overhead. Professor Forbes appears also to have misunderstood me in thinking that I was discussing the question of transformers *versus* accumulators for all purposes and for all kinds of installations; whereas my object was to discuss transformers *versus* accumulators for a system of electrical supply for a representative residential district of London. I therefore commence by saying that I put overhead wiring out of court. Wherever overhead wiring can be used I have no doubt that distribution by means of alternating currents and transformers is a very good system to use, and probably in first cost the plant is cheaper than battery plant. But I wish to confine the discussion to the limits I put upon it—that is, the lighting of London.

Mr.
Crompton.   Mr. Kapp commenced his remarks by referring to my load diagrams, and stated that, although they might be correct for the present, they were hardly representative for London; if I had taken a district in which there was much office lighting, that my valleys would have been filled up, and that in consequence, instead of the 2,100 units calculated upon the area of the diagram, I should get the 3,600 units spoken of in his article in *Industries.* I do not admit this for one moment. London is not like New York. In London our residential districts contain very few offices, which are grouped together in the City; a few are at the West End, but the majority are in the City. But I think the matter of supreme interest at the present moment is the lighting of our houses in London; it is there that we shall feel the full benefit of the electric light, and anything that any of us can do or say which will forward the cause of the distribution in that direction is of value. I fully admit the force of what Sir David Salomons and several other speakers have said on the subject of the sale of motive power, as by such sale we fill up the valley in the diagram which will be of the greatest advantage to the supplying company. As the capital expenditure of the company is somewhat in proportion to the height of the *peak* of the diagram, and the income of the company is in proportion to the *area* of the diagram, it follows that the more the valley is filled the better the company will pay. Thus the sale of electricity for motive power is to be devoutly desired, and I think it will come; but I hardly thought it would be a fair argument for me to use in favour of the B.T. system as against the A.T. system, which cannot sell motive power. Every argument that I have been using in favour of the B.T. system will be considerably strengthened when I have the additional income that I should get in the daytime from using the direct system combined with batteries.

Mr. Kapp appears to have misunderstood my figures in Tables I. and II., otherwise it is difficult for me to follow his calculations, which are as follows:—He states, referring to Table I., that if, instead of using the insulated cable that I use, costing about £116 per 100 yards for the double cable, he proposes to use a cable insulated in the same manner, but covered with lead,

which would increase the cost to £130, he would by that extra Mr.
expenditure save considerably on the cost of laying. In this he
is mistaken, as the sum of 3s. per yard forward, which he seems
to think sufficient to cover all the expense of laying such a pair
of cables under a London footway, is quite inadequate. Is he
aware that the £25, or 5s. per yard, I have put down for
trenching and repaving per 100 yards is absolutely the figure
levied by the vestry? It is a sum that we are forced to pay
whether we like it or not, and on which we can make no
reduction. For every yard of York paving we move we have
to pay 4s.; for every yard of asphalte or wood pavement, 11s.;
but if we suppose that there is only about 10 per cent. of asphalte
footway or wood pavement crossing in our laying, we must bring
up the 4s. to 5s. Hence I have taken 5s. throughout, or £25
per 100 yards. Mr. Kapp seems to think that contractors can
be found who will undertake the risky work of laying cables
in the streets without asking for anything to cover risks and for
profit. Has he seriously considered how considerable are these
expenses of opening up and guarding the trenches, how liable he
is to be heavily mulcted in compensation to foot passengers for
trifling accidents caused by the carelessness of his workmen, and
that he must have some profit for the use of his money? You
will see that under the head of "engineering and super-
"intendence" I have reserved a sum for these matters; and
I think that it will be admitted that the allowance I have made
is as small as is reasonable, as it has, in addition, to cover the
services of the engineer who designs and manages the whole of
the street work. If Mr. Kapp adds together these items he will
find that the total will be considerably higher than the £18 10s.
per 100 yards. It is highly important that I should dwell upon
this specimen of Mr. Kapp's criticism, because on this cost of
laying the mains so much depends. This has been little under-
stood, none of us having had more than two years' practical
experience of it. In bringing it before you I hoped to be of
service to those who, like myself, are engaged on this interesting
subject, and who wish to have some idea of what had been the
cost of such cables as have been already laid.

Mr.
Crompton.

Turning to my second table, although I expressly stated that
I did not consider that my system of laying was perfected, and
I therefore deprecated criticism, yet it was very considerably
criticised. The point, however, of General Webber's criticism is
taken off by the fact that he misunderstood the method of sup-
porting the conductors, as he spoke of them as being suspended
from the roof of the culvert. If that were the case, no doubt
water from the roof would run down the suspending links, and
would remain as a conducting film on the insulators, and thus
reduce the insulation. My insulators, however, stand on short
pillars resting on the bottom of the trench, and I see no reason
why insulation would suffer, even if we had a couple of inches of
running or stagnant water on the bottom of the trench. General
Webber says that the trench will be blocked by the *debris* left by
water, that vermin of every kind will congregate in it, that we
shall have legions of rats, and that in a short time our culverts
will be in such a condition that we shall lose the insulation
entirely. I cannot conceive that such a state of things can be
possible with ordinary care and attention. We have had one
year's experience with these culverts, and none of these things
have occurred, nor is there any sign of them occurring. We
have had a very trying winter. As is natural under the circum-
stances of commencing a new installation, the trenches have been
opened far oftener than they will be in future; they have been
exposed to all kinds of weather. Thus there has been far greater
opportunity for wet to get into them than will hereafter be the
case; but, in spite of this, the insulation of the bare conductor is
practically as high, length for length, as that of the best cable
made by the best makers in the world. I here want to bring to
your notice an interesting fact which may be of use. It is very
easy to keep the insulation of a cable high when it is not cut into
by the distribution branches being jointed on to it—*i.e.*, so long
as its insulating cover remains intact from end to end; it is then
easy to measure its insulation resistance by megohms. But when
a few hundred such cuts have been made through its insulation,
and the branch wires have been jointed on, it matters not how
carefully the insulation be replaced and the joints made good,

every one of them becomes a source of leakage: the megohms resistance soon comes down to thousands of ohms; and, what is the curious part of it, the whole of the exterior surface of the cable becomes a means of communicating the leaks that are started at the minute cracks of the joints to the earth, so that there is an enormous earthed surface wherever the hemp or jute covering comes in contact either with the pipes or troughs containing the cable or with the earth itself. In practice we find it extremely difficult to make these joints tight. It is the point I shall expect trouble with in underground cables carrying the high potentials proposed by the advocates of the A.T. system. A writer in one of the electrical papers has already called attention to this fact, and I am sure that whoever plans and carries out a district having a large number of attachments to the high-tension mains will find that his difficulties at these points are very great—infinitely more than any difficulties we are likely to encounter in the B.T. system, in which the only conductors so cut into are those carrying 110 volts.

I am perfectly aware, as has been stated by Mr. Fleetwood, that there are many places where we cannot use the culvert system; but he has made a mistake in supposing that we require a culvert 18 inches square: we have actually used culverts only 5 inches deep, on account of the cellar arches approaching so near the surface of the pavement. We do want a considerable width, because in my system we put in mid-feathers of brickwork between every one of my bare conductors, so as to make it impossible for any swing or side movement of the conductors to make a short-circuit; but 18 inches is found to be ample even for the three-wire system, and I have no doubt that if I wanted to get a charging main in addition, 24 inches would be sufficient; and there is no difficulty in getting this width under the footway, the difficulty being in getting depth. This in several cases has been cut down to 2 inches, and then the only plan possible is to draw each individual insulated cable into a separate lead pipe from $1\frac{1}{2}$ to 2 inches diameter.

I must turn now to the cost of the generating plant. Most of the actual makers of alternating-current dynamo machines appear

Mr.
Crompton.
to think that I have under-estimated their cost in my paper. Professor Forbes is alone in thinking the contrary, and this is no doubt due to the fact that Mr. Westinghouse has already settled on a standard system. Professor Forbes says he makes only three kinds of machines, and is able to turn them out by special machinery. I see no reason why we should not be able to do the same here if our demand were equal to that of Messrs. Westinghouse; but if my arguments are correct I do not think there will be the same demand for alternating machinery in this country as in America.

As to the cost of transformers. Mr. Kapp says that I over-estimate them; not because he considers that my price per transformer is too high, but that I want too many of them. I have estimated the cost of fixing a transformer common to two houses at £15: this price includes the chamber which contains it, and the cost of labour incidental to fixing. He thinks that the number of transformers could be reduced, and at once goes to the extreme of collecting his transformers into ten distributing stations in fact, to use Professor Forbes's American expression, he "banks" his transformers in these distributing stations. Now I wish to point out that, if we take Mr. Kapp's figures as correct, the network to distribute from these ten stations will take just as much copper as I use when distributing from ten stations. He told me that my calculations were wrong, and that the copper I had allowed was only sufficient to give the allowable fall of potential if I distributed from ten stations, therefore he would have to adopt the same figures—*i.e.*, the 20,000 yards of cable, at the cost in my table for the B.T. system. By that means he would at once add £6,000 on to the cost of his distributing mains, and on the top of that he will have to add the cost of such sub-feeding mains as he must employ to carry the current from the end of his large charging mains to these sub-stations. I have not run this figure out, because I do not know where he is going to place his ten stations, but it will probably reach £3,000 or £4,000; thus £10,000 is at once added to the cost of his system by this method of distributing. On the other hand, what does he gain? I have given the total cost of transformers as £7,500, and if he saved

the half of this he would spend £10,000 to save £3,500. This is **Mr. Crompton.**
how Mr. Kapp proposes to reduce the cost of the A.T. system. I
think, however, from the conversation I have had with other gentle-
men, that Mr. Kapp's proposals are not generally accepted; and from
what I gathered from Professor Forbes and other gentlemen's
remarks, it appears certain that they would wish to put down
transformers to one or two houses, as I have done, so that I do not
see how their cost can be reduced. It is not the cost of the trans-
formers themselves, but of starting a fresh job each time that a
fresh transformer is required to be fixed, as they will not be put
down simultaneously. All contractors know that such small jobs
cost more than continuous work that can be given out on contract.
I think those of you who have had experience of such work will
agree with me that £15 for fixing a transformer for two houses, in-
cluding the cost of the transformer itself, with profit and risks, is
not an excessive sum, and I do not see how it can be materially
lessened.

I now come to the vexed question of the efficiency of these
transformers. My statement that they are efficient at low rates
of output has been traversed by several speakers. Mr. Mordey
wrote to me on the subject, as he was unable to speak during the
discussion. For purposes of argument I will adopt the figures
given by the advocates of the A.T. system, and will suppose that
the efficiency is as high as they make it where the transformers
are worked at 30 per cent. of their maximum output; but what
I repeat is that 40 per cent. of the average daily output of your
stations is taken at an extremely low rate of output of the trans-
formers. By looking at the diagram you will see that this 40
per cent. goes only up to the lower horizontal line: this really
means that there are only two or three lamps running in each
house during about nine hours of the twenty-four. Two houses
having one transformer common to both would require its capacity
to be 4,800 watts to supply the maximum demand, whereas the
180 to 190 watts required for the few lamps is rather less than 4
per cent. of this total output for nine hours, and it is during this
time that the efficiency is so low; and I shall hereafter point out
to you that it is during the corresponding part of the output that
the efficiency of the accumulators is at its highest.

Mr. MORDEY: Rather a small one.

Mr. R. E. CROMPTON: I have now got to the point at which I wish to cordially endorse all that Mr. Preece said when describing the pluck and energy shown by Messrs. Gaulard & Gibbs. Nobody admires more than I do the result of their labours in pushing that system to its present success. Nothing in my paper was intended to detract from the credit and honour that they have fairly won in the teeth of just such uncompromising opposition as I am now receiving from their friends.

Mr. Swinburne made a remark about transformers which does not appear to have been noticed by the friends of the A.T. system—*i.e.*, that the transformers do not regulate ; *i.e.*, to use an electric slang expression, they compound down—the E.M.F. falls as the load is increased—and no one has succeeded in winding them so as to overcome this difficulty. If this is so—and no one has denied it—it is another serious difficulty in the way of distribution by the A.T. system, and a difficulty which I did not take advantage of when I was comparing the merits of the two in the body of my paper.

Now I turn to the accumulators. First, I must point out that my paper speaks solely of the use of accumulators as transformers. Mr. Drake appears to be the only gentleman who has brought this strongly before your notice. There is a very wide difference between accumulators worked in the manner I have advocated so as to act as transformers, and so that during the hours of large demand the current comes direct from the dynamos, the accumulators only supplying the extra current during the peak of the diagram, and afterwards supplying current only during the long periods of very small output when the engines are stopped, and, second, the plan of using accumulators by filling them in the daytime and discharging them at night. Mr. Frank King, Mr. Parker, and other gentlemen asked me why I did not refer to the Colchester installation. I admit that the Colchester installation was worthy to be referred to ; but it was so totally different from the system that I have been advocating, and so costly, compared with the A.T. system, that I considered it out of court. For it is evident you require

six times the amount of batteries if you use them in the way _{Mr.} _{Crompton.}
they were used at Colchester, so that the cost of these batteries
alone rules them out of court. At present I am keeping down
the cost of batteries as much as possible by using them only as
described; but no doubt as our experience extends I shall use
them more largely than I now propose, and then I should be able
to show you a sheet of costs on which both the charging and
distributing mains would be very much reduced, on account of
the larger number of distributing stations. But for the present
I have guarded against stating what Professor Forbes has wrong-
fully called a hypothetical case; the whole of my figures are
based upon experience. Every figure that I have put before you
I am prepared to substantiate by carrying out the work at the
price, as I have done it before, and I know that I can do it again;
and I have actually done more with the battery transformer system
than has yet been done with the alternating-current transformer
system when underground mains have been used, although
Mr. Hammond informs us that it has been done at Eastbourne.
I am very glad to hear of the success in this one case. But I
hear that at Rome, where the mains have been laid underground
with the greatest care, the best Siemens concentric cables being
used so as to avoid induction, and with every effort made to get
high insulation, they have had great trouble, and the cost of their
distribution there has been extremely high, simply on account of
the repairs and perpetually replacing of the mains.

I have been asked over and over again why I choose to use
400 volts or 450 volts for distribution. The reason I do so is that,
although I should very much like to use 600 volts or 800 volts, I
do not know whether difficulties will then arise which have not
hitherto been met with. The distribution at Vienna which has
been so successful is at 450 volts; it is for 13,000 lights, so that it
is rather larger than the specimen installation we are discussing.
The mains are nearly 1,500 yards long, consequently it resembles
it very closely. So that, both as regards capital and working cost,
the figures that I have placed before you represent as closely as
is possible without breach of confidence the results obtained at
Vienna, Le Mans, and Kensington. At Vienna the 440 volts we are

Mr.
Crompton.

using has never given any trouble. Mr. Kapp says that we shall not be allowed to use such a high E.M.F. here. I see Col. Armstrong in the room, and I feel satisfied that I could convince him by the Vienna experience that 440 volts is perfectly safe to use. In London, however, we can do a great deal of distribution without any necessity for the extreme E.M.F. of the four batteries system, as we can use two groups of batteries only in series, and the system is then an ordinary three-wire affair—in fact, the same as the majority of the Edison work in America, only with the advantage of using the batteries to cut down the cost of both charging and feeding mains, as these will no longer require to be large enough to work the maximum output as shown by the peaks in the diagram ; and this means halving the copper in the charging mains. The Le Mans station, about which I have said very little, is an extremely perfect little station, and has worked without any hitch whatever. It is a three-wire system. At the gasworks there is a generating plant, the dynamos giving about 250 volts ; a pair of cables are carried up to an accumulator station close to the market-place, and from thence the three-wire system is taken all over the town. With a modified three-wire system for batteries you do not require quite the same complication as you do to work a three-wire system from dynamos. Some gentlemen hardly appear to appreciate the fact that by the use of accumulators you may reduce the cost of your feeders very largely on either the direct, three-wire, or five-wire system if you put your batteries at the distant point ; and I have shown that the gain is so great that when you are using 440 volts, effective, the cost of the 2,000-yard charging main is no higher than that for the 2,000 volts, A.T. system. As my figures show, the A.T. system costs £6,160 for 2,000 yards ; in the other case it costs £6,137. I do not deny for a moment that when the distance exceeds 2,000 yards there will be a balance in favour of the A.T. system.

In regard to Professor Forbes when he spoke in such slighting terms of a little installation of 10,000 lights, I tell him that an installation of 10,000 lights, with a 2,000-yard charging main, forms one-tenth of a 100,000-light plant. You may easily distribute from a waterside site in London 100,000 lights exactly on

the lines given in my paper.   Therefore there is nothing in ^{Mr.}
Professor Forbes's point when he talks about 10,000 lights being ^{Crompton.}
a small unit.   As a matter of fact, it is quite a sufficient unit to
enable us to calculate all the charges of cost, interest, and so on ;
although I admit that there are other charges, such as company
management, which can be reduced when they are distributed
over the larger plant.

My attention was called by Mr. Kapp to an omission from my
paper, which is very easily explained.   He asked me why I did
not include the rent of the four accumulator stations.   The
reason is that the rent of an accumulator station is really in-
cluded in those items which I have excluded from the paper.   If
you take the rent of the larger station required for the dynamo
machinery for the A.T. system, and set it against the smaller rent
required for the other, the extra £150 which is required for rent
of four accumulator stations is covered, and it does not affect the
result.   This sum of £50 happens to be the exact figure that we
have to pay for this kind of station : it represents the rent of
a coach-house and stable ; and I have had such stations for
accumulator stations offered to me in at least a dozen cases at
this rent.   I find, however, that I cannot get a single room for a
less sum, as it is difficult to get a long lease of a part only of
mews property ; so that if I only want to put in the transformer
Mr. Kapp speaks of, I shall still have to pay £50 a year for it.

Mr. Parker found great fault with me for not giving a fuller
description of the Howell accumulator.   I have already said that
my paper is intended to apply to all well-known accumulator cells,
and not to my own in particular.   I did not, therefore, give a
description of my own cells, although there was, no doubt, some
temptation to do so.   But I am sure that Mr. Parker can turn out
very good accumulators, and, judging from his remarks, I should
say that his accumulators have a very good advocate in himself.

As to Mr. Kapp's criticisms on the cost of accumulators.   You
will notice that his question has been answered by the practical
makers of accumulators : Mr. Parker, Mr. Sellon, and Mr. Drake
have practically agreed that my figures are correct.

As to the efficiency of accumulators, about which so much has

Mr.
Crompton.

been said. Mr. Preece, Professor Forbes, and a great many other gentlemen seemed to be surprised that I claim 80 per cent. efficiency between the lamps and the terminals of the dynamo. I am going to call your attention to this fact strongly later on, but I must say that I have obtained this efficiency for many months at Vienna, using E.P.S. accumulators. I have obtained a higher efficiency at Kensington, and at a private installation of my own; and it is very easy to understand how this is, when we are using accumulators as transformers. I should here state I am speaking of total efficiencies—*i.e.*, the watts at the lamps divided by the watts at the terminals of the dynamo. There is another kind of efficiency—current efficiency—*i.e.*, current taken out of the dynamo divided by the current put in : that is current efficiency; but I do not refer to it here. I am speaking of the watt efficiency when I say that at such times as the dynamos and batteries are working parallel, and the current is neither passing into or out of the accumulators, there is little or no loss due to the fact that the batteries are connected parallel to the circuit, which would otherwise be a direct-supply system. Of course, if any short-circuit or leakage occurred inside the batteries from plate to plate, then there would be loss ; but it is easy to guard against such a thing happening, and with proper precaution it need not happen. It has not happened either at Vienna, Le Mans, or Kensington. I know that in times past there have been losses of this class from bridges of oxide being formed between the plates, and a large portion of the current has passed off in that way, and thus accumulators have got a bad name ; but they are better understood now, and this trouble only exists in private installations where there is no opportunity of watching the forma-tion of these short-circuits, so that the matter goes on from bad to worse, until eventually the current efficiency of these accumu-lators falls very low indeed. But I take it for granted that there is a sufficient staff on my system to prevent this occurring, and it pays to prevent it occurring, for directly it begins it does great damage to the accumulators. Now I have shown you that during about 60 per cent. of the total output the efficiency of the accumulators does not come into question ; it is merely a

question of direct supply ; and through the remainder, or about 40 Mr. Crompton. per cent. of the output, I have shown you when I was speaking of the transformer efficiency that during nine hours the batteries would discharge at a very low rate. By actual experiment I know that at this low rate we can get 82 per cent., but I do not claim it ; I really calculate on 70 per cent. only. Now, if you add 60 per cent. at no loss, and 40 per cent. at 70 per cent., and make due allowance for losses in mains, you get a total efficiency considerably exceeding the 80 per cent. I have claimed. Therefore the severe criticism that Professor Forbes has passed on that portion of the paper entirely falls to the ground.

I turn now to the question of the depreciation of batteries. Mr. Gordon has made public certain figures which show, without any further comment from me, the rate at which we believe that we can afford to maintain accumulators, and I think we have made ourselves safe in the contract that he read out to you. It was not my intention that he should read it, but as he has done so it is before you to judge by, and all the experience I have had with the accumulators that I have been using points out that we are well within the mark when we say that we can maintain them for 12½ per cent. I think I am at liberty to say that during my recent visit to Vienna, since the paper was read, I took the opportunity of inspecting the large battery of accumulators. I there ascertained this fact—that the company were not willing to accept a tender for the maintenance of the accumulators at 10 per cent. on their cost, as they thought they could do it at a lower figure.

I believe that at Kensington Court the cost of the alterations and little repairs that have been made to the battery during the first fifteen months' working amounts to about 2 per cent. of its cost. My experience of our own accumulators does not extend beyond two and a half years ; but as Mr. Drake has pointed out that there is a battery which has been carefully dealt with at the P. and O. Office, which has lasted four years, I have a battery in my possession which, as far as I can judge, has not deteriorated at all during the last two years ; it has cost nothing for repairs, and I do not see why it should cost anything during the next two years.

This battery has been discharged at high rates, and used as hardly as I propose to use accumulators in my paper.

Mr. Kapp says that I have erred in allowing such a large variation as 4 per cent. in the E.M.F. in the distributing network, and that I have not allowed sufficient copper even for this large variation. I think, if he works it out carefully, as I did before writing my paper, he would find that he is wrong, and that the extreme variation is under 4 per cent.; and it does not follow that there need be this variation in the lamps, because by switching on additional cells at the time of maximum demand we can halve the difference, and thus reduce the variation to 2 per cent. But even 4 per cent. is not so terribly noticeable as Mr. Kapp appears to think. Mr. Kapp laid stress on certain calculations made for the Bradford mains, but Mr. Shoolbred tells me there were no calculations———

Mr. GISBERT KAPP: The calculations were made quite privately; I saw them at Bradford.

Mr. J. N. SHOOLBRED: I never made such statements. I cannot understand what you mean.

Mr. R. E. CROMPTON: I understood you to say that you did not know how many lamps were going to be used at Bradford, and that the cables were to be laid down " as required."

Mr. J. N. SHOOLBRED: You said there were no calculations, which is a very wide statement.

Mr. R. E. CROMPTON: I now come to the remarks of Mr. Hammond, who tells us that there is no necessity for providing accumulators as a reserve, because every practical engineer knows when a steam engine is likely to stop. Mr. Hammond's experience has been excessively favourable. I find that steam engines usually do stop just at the time when they ought not to stop—i.e., at the time of heaviest load—and then they very often pull up very suddenly. Everyone who has had experience of this kind knows that when the end of a connecting-rod gets warm the engines sometimes stop very suddenly. Then there are the cases of belts coming off. Has nobody ever had a belt come off a dynamo when three or four machines were running parallel? What happens then? Suppose there are four or five machines running parallel.

Mr. MORDEY: The cut-out acts.

Mr. R. E. CROMPTON: One gentleman says, "The cut-out acts." But I know what happens in addition. A report comes in from the customers the next day that the light has been excessively bad that evening. At all events, such things cannot happen when we have accumulators as a reserve. We can then stop the engine right in the middle of the fullest load, as was the case when we had not boiler power enough at Kensington: we were able to stop the engine and get up steam without the customers knowing anything about it. We cannot do this with direct working.

I have already mentioned a few points raised by Professor Forbes. I was not here when Professor Forbes made his remarks. I think that if I had been here he would have shortened them, as a large portion of them are based entirely on false premises. For instance, he bases many of his arguments on overhead wires, which I have never had under consideration at all. I am going to select a few of his remarks, and you can judge from them as to the accuracy of the remainder. He objects to the use of the words "I am told" as leading you to suppose that my paper was based on hearsay evidence, and as such should not have been brought before this Society. The boot is on the other leg. *I* have been talking from experience, *Professor Forbes* from what he has "been "told." I used the words "I am told" once, when referring to statements made by the makers of accumulators other than my own, simply because I can answer for my own, but not for those of other people. I believe that my paper contains more commercial and contractors' experience than is usual in papers read before this Society. I think you will find that as a rule those who read papers on scientific subjects will deal with everything but the commercial results, which may be copied and taken advantage of by rival firms. I feel a little sore on the subject, as in my paper I have tried to put myself above that sort of thing. The figures I have put before you may be of use to contractors, and to gentlemen like Professor Forbes, who has hitherto had to deal chiefly with theoretical subjects, and has had small opportunity of dealing with contractors' work. I say this because I know that up to date

Mr.
Crompton.

there has been no work in the nature of main-laying carried out
in this country under a consulting engineer.   Mr. Forbes goes on
to state that my allowance for the reserve plant for the A.T.
system is far too great; he proposes to cut away this item.   I
took Mr. Kapp's figures as given in *Industries* because I thought
that Mr. Kapp knew his subject, and I still think that these
figures are correct.   Mr. Forbes now states that they are wrong,
because Mr. Westinghouse can get 600 kilowatts at the lamps
out of 1,000 I.H.P. in the steam engine cylinders; that is to say,
he obtains 81 per cent. total combined efficiency.   Never was
such a monstrous statement made.*   We in this country rather
fancy ourselves as engine builders and dynamo makers; at any
rate, Mr. Willans has probably made the finest and most
economical high-speed engines ever produced, and we have
several makers of dynamos unsurpassed by any makers in other
countries.   Mr. Westinghouse has copied Mr. Willans' engine, but
it is not a very good copy; at any rate, we have never heard that
he has obtained such results as Mr. Willans has obtained here.
But yet Mr. Westinghouse is supposed to have obtained with his
engines and dynamos an efficiency about 12 per cent. higher than
that which has been obtained by the Willans engine combined
with the Edison-Hopkinson dynamo, with the Siemens dynamo,
or with any of the best known dynamos in this country.   If
Professor Forbes during his long stay in America has listened
seriously to such statements as these, I must say that I shall find
it difficult to follow him; but I do not think, when he comes to
consider the matter, that he will continue to believe that 600 kilo-
watts at the lamps can be got out of 1,000 I.H.P.   He will have
to admit that it requires about 1,200 I.H.P., and that the 250 H.P.
I have allowed is not an extravagant figure, but is the very smallest
that is necessary.   When he has admitted this, he has, in addition,

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* I have since heard from Professor Forbes that in his remarks (see p. 428,
line 23) the *1,000 I.H.P.* should read *1,000 Brake H.P.* This correction, of
course, reduces what would otherwise have been Mr. Westinghouse's very
extraordinary performance to a very ordinary one, and is a further corrobora-
tion of my statement that 1,450 I.H.P. is not too large an amount to allow
(including reserves) for generating 600 kilowatts at the lamps.—R. E. C.

to admit the extra allowance for the increased cost of site and Mr.
Crompton. buildings; and with these two items he has admitted every cost in my sheet except that of mains, which I decline to discuss with him, as he bases everything on the overhead wiring, whereas mine is underground wiring.

I think that I have now answered most of the gentlemen who have discussed my paper. The opinions of those who are practically acquainted with the working of central station lighting appear to be fairly balanced. But figures based on facts are worth more than opinions, and I have in my possession facts which prove most conclusively that in all the most important points that are worthy of discussion—*i.e.*, labour, fuel, wear and tear of conductors—the B.T. system is overwhelmingly superior to its rival. There are no doubt several persons in this room who know that I am speaking accurately when I state that the experience of the Grosvenor Gallery installation has been such that their estimates of future coal consumption are based on the supposition that each electrical horse-power in the lamps will take about 18 lbs. of coal. It is needless to say that I should not have the hardihood to stand before you if I thought the B.T. system would take as much, or a quarter as much.

I hope to have an opportunity at no distant date to clear up this question beyond dispute. As soon as our extension of plant is ready at Kensington, I shall have great pleasure in allowing gentlemen to make such arrangements as they choose to see for themselves that the statements I have made of the economy of fuel on the B.T. system is correct.

I now wish to sum up. I have shown you that in the A.T. system the dynamos are certainly more expensive to manufacture; they are certainly more difficult to put parallel. In the A.T. system the machinery must always be working. Put against these facts that in the B.T. system you have cheaper plant; you have machinery that need not be always moving, consequently the labour costs very much less than the labour in the A.T. system. I have shown that the saving in conductors in the A.T. system is small, while the liability to breakdown in the underground conductors is much larger than with the other. I have

Mr. Crompton. shown you that there is no income from sale of motive power possible with the A.T. system, whereas there is every probability of a large income from this source for the other. I have shown you that the A.T. is extravagant in fuel, and that the B.T. system is not so. I think that when I put all these together I have prepared a very formidable array of charges against the A.T. system.

The Chairman. The CHAIRMAN : I am sure you will all agree in according your hearty thanks to Mr. Crompton for his very interesting paper, which has brought forth such an important discussion, and I am sure you will extend your thanks to him also for the very full reply which he has favoured us with to-night.

Carried unanimously.

We will now resume the discussion on Mr. Preece's paper on "Fire Risks and Fire Office Rules." Mr. Heaphy is here, and I have no doubt he will have something further to say on the subject.

Mr. Heaphy. Mr. MUSGRAVE HEAPHY : Mr. President and gentlemen,— At the last meeting I asked, and obtained, permission to make a statement this evening as to the reason the sittings of your committee on the Phœnix fire rules were interrupted, for I felt that the account given by Mr. Preece unintentionally conveyed a wrong impression. Had it not been for that remark I should not have spoken at all, for obvious reasons. In May last a committee of your Council was formed to revise their rules of 1883. Communications were opened with the Phœnix Office on the matter, the result being that a tentative agreement was arrived at, that in order to settle once for all any difficulties as to rules for the future the Telegraph-Engineers' rules and the Phœnix rules should come out under the same cover, the cover to contain no mention of the Phœnix. The first portion of the pamphlet was to contain general rules and principles for installations, drawn up by your committee ; the second portion of the pamphlet to contain the Phœnix Office detailed rules for installations ; there being an acknowledgment clause that these detailed rules were the Phœnix rules. In order to make certain that there was nothing in the Phœnix rules that

would be objectionable, four gentlemen of your committee were Mr. Heaphy
to form a sub-committee to go through them, clause by clause
and word for word, and in the event of our not agreeing the
arrangement was to come to an end.   These gentlemen were to
be men practically acquainted with the difficulties of electric
light installations : they were Mr. Preece, Mr. A. Siemens, Mr.
Crompton, and General Webber.   We had many sittings at the
offices of your Society without the slightest hitch or difficulty
of any kind, and the Phœnix rules were unanimously approved
of after certain verbal alterations had been made and a few very
slight variations, all of which were agreed to.   The acknowledg-
ment clause was then framed and agreed to.   The sub-committee
then revised their general rules : with those I had nothing to
do.   Here the pamphlet is.   The rules came out in the cover
of " The Society of Telegraph-Engineers."   That was to meet any
objection from any fire office that did not like the word " Phœnix "
seen.   The pamphlet is headed " Rules and Regulations recom-
" mended for the Prevention of Fire Risks from Electric Light-
" ing ; " then come the names of the committee, Mr. Webb's name
being attached as the Secretary.   Then a little acknowledgment
clause only : " The following detail rules have been drawn up by the
" Phœnix Fire Office for electric light installations and electrical
" power installations, and having been laid before the Society for
" approval, are recommended for general use."   I thought, and so
would you if you had attended the number of sittings I had, that
the matter was now at an end so far as I was concerned, when I
received a letter asking me to attend your committee to explain
certain rules.   I thought this strange, as the matter had been
so thoroughly threshed out by the sub-committee, and that they
were so thoroughly conversant with every point.   However, I
accordingly went, when, to my surprise, the first thing I heard
spoken at the table was that the acknowledgment clause was
called into question, with a kind of half intimation that instead of
the detailed rules being acknowledged as the Phœnix rules they
should be termed rules *based* on the Phœnix rules.   I also found
that the whole of the Phœnix rules were to be gone over again—
not the original rules, mind you, but the slightly altered Phœnix

rules that were in the cover of the Society of Telegraph-Engineers. What, then, was the position of the Phœnix Fire Office? These rules might have been slightly altered again verbally, and the acknowledgment clause altered objectionably, and all copyright lost. Perhaps the very name of "Phœnix" might have been expunged in the next edition, and what remedy would the Phœnix have had? None. If it objected, the answer might have been, "Oh, the detail rules have been drawn up with the consent "of your representative sitting as one of the committee; you "cannot now refuse to recognise them, nor object to the "acknowledgment clause, whatever it may be." Gentlemen, the Phœnix Fire Office, after a time, might have been entirely deprived of its rules. I only say "might." What was the only course open to the Phœnix? What would any of you have done under the circumstances? Why, you would have done what the Phœnix did—say that, unless the acknowledgment clause was to stand as agreed to in this pamphlet (in the event, mind you, of the whole committee being satisfied with the rules), it must retire from all further negotiations. I then wrote a letter to the Secretary of the Society. It is a very long one. I will not take up your time by reading it; but it goes very fully into the matter, and I only hope it will be allowed to be printed.* But I should like to refer to the high terms of praise in which the Phœnix Office spoke regarding Mr. Preece, General Webber, Mr. Crompton, and Mr. Siemens. I cannot sufficiently tell you the amount of time those gentlemen gave, nor the courtesy and labour on Mr. Preece's part. I am quite certain that if they had been allowed to have had the matter in their own hands I should not have had to trouble you with any statement to-night. But they were out-voted.

Mr. W. H. Preece: I object to that.

Mr. Musgrave Heaphy: Well, they would have been out-voted. Well, gentlemen, that letter was written, and I attended the next meeting of the whole committee, when they declined to give me any information as to the acknowledgment clause—whether it was to be altered, or what it was to be like—until the sittings on

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* As the substance of the letter is given in Mr. Heaphy's remarks, the Editing Committee have not considered it necessary to publish it.—Ed.

the Phœnix rules—in *this* pamphlet, mind you (*i.e.*, that to be Mr.
Heaphy.
issued by the Telegraph-Engineers), not *this* (*i.e.*, that issued by
the Phœnix)—had been completed. Gentlemen, I had nothing
to do but retire, and I did retire. Several gentlemen of the
committee asked me to stop, saying that I could reckon on
their support. That I knew: I could reckon on their support,
and it would have been favourable; but when I said that
they might possibly be out-voted by others of a different
opinion, the reply from some of the members was, "Yes, that
"might be so." I do not wish for one moment to impute that
your committee would have done anything that it did not consider
right and honourable,—I do not wish to cast the slightest imputa-
tion upon anyone,—but remember that there are often two ways
of regarding the same thing, and what would seem perfectly fair
and right from one view would be felt as a bitter wrong and
injustice from another. Let me give you an instance. A speaker
last Thursday in this room actually stated in effect that because
some of the clauses in the Phœnix rules that were brought out
in February, 1882, were then similar to certain rules in America,
therefore there is no right to copyright in the Phœnix rules, not-
withstanding all the original thought and labour bestowed upon
them to make them perfect; that an installation placed up in
accordance with them is different from other installations; and
that they treat upon systems of lighting now that were undreamt
of six years ago. All this is to be monopolised by anyone who
chooses to appropriate it. I am only instancing how we may have
differences of opinion on the subject. I do not know what the law
of the land is on the matter; that is not for me to say. I can
assure you that the Phœnix Fire Office has always been, and is,
most willing and most anxious to do everything that is just
and right, and to prevent any trouble to the trade; but it does
consider that fairness should be done to it in return. Let me tell
you what took place after the interruption of this committee. A
gentleman connected with one of the most powerful offices—one
of the leading fire offices—spoke to me and said, "Let us see
"whether we cannot come to some arrangement;" and we did come
to an arrangement. Here it is. The Phœnix cover is cut out again,

Mr.
Heaphy.

of course, and the title is " The Fire Offices' Rules for Electric
" Light Installations." The Phœnix gave up everything except
the acknowledgment clause to save the copyright: " The follow-
" ing rules, which have been drawn up by the Phœnix Fire Office
" for electric light installations and electrical power installa-
" tions, have been adopted by the leading fire offices throughout
" the kingdom, and are recommended for general use." The
demand for these rules in this form was getting so great from
offices that it was impossible to supply them in time. I believe
almost every office in the kingdom would have been using them
at this time; but one office spoke to the secretary of the Phœnix
Office, and said, " You must not say ' The Fire Office Rules;' you
" have no right to use the expression ' Fire Office Rules,' " or words
to that effect, for I was not present at the time ; and then something
to this effect: " You say here, ' have been adopted by the leading
" ' fire offices throughout the kingdom ; ' are not we a leading fire
" office ? and we do not adopt them ; " and in consequence of
that, gentlemen, the Phœnix Office withdrew their rules under
that cover.

It would seem as if it did not matter what trouble the trade is
put to—not a bit of it—so long as the word " Phœnix " can be got
away. Gentlemen, you know what injustice is : you have had to
feel the tyranny of Mr. Chamberlain's Act; but that Act is mercy
itself compared to what is now sought to be imposed upon the
Phœnix. You have had twenty-one years allowed before your
work is confiscated ; the Phœnix rules are not to have twenty-
one weeks. I will never believe that you will be party to such
a wrong.

Mr.
Human.

Mr. H. HUMAN: With regard to the remarks of Mr. Preece
on the last occasion, in the brief criticism he bestowed upon
the Guardian rules, he condemned a clause which, in my
opinion, has no application to the purpose whatever. He read
out a clause which specifies the minimum insulation resistance
allowed, and he compared that to a rule of the Society which deals
with the total leakage of an installation. The two things are not
altogether the same. We do not deal, as a matter of fact, with
the question of the total leakage allowed ; but in the previous

rule we first of all specify what materials should be used for Mr. Human. covering conductors, and we follow that up by giving the minimum electrical resistance of such covering in megohms; and we adopted what is, I believe, the common practice of the trade. It is addressed to the makers of conductors, and we simply specify the minimum insulation resistance for conductors, and I therefore do not understand why Mr. Preece should have condemned that rule by comparing it with a rule of the Society which only deals with the leakage of an installation.

With regard to that rule of the Society. We ought, perhaps, to have dealt with the question of total leakage; but at the same time the question of what leakage may be allowed in an installation is one more concerning the engineer than the fire office. It is so important a question to the engineers that we can well-afford to leave it to them. But I daresay in the future we shall be only too glad to adopt the suggestions of the Society. They now very kindly inform us what should be the minimum leakage of an installation, but I find there is a difficulty in the minds of some practical men as to getting at the 1-5000th part of the current. Mr. Preece the other night spoke of it as being rather a simple matter, and illustrated it by saying, when you have 5,000 lamps, if you get a leakage that will light one lamp you have it at once. We are not, however, in the habit of dealing with 5,000 lamps, and it must be remembered that the fire offices are more concerned as a rule with 10, 20, or 30 ampères; and to get at that in a practical manner, from what I can understand from those who are constantly dealing with this question of leakage, an instrument to be of any use would be necessarily of so delicate a nature that it would break down in practice. I should be glad to hear the opinion of experienced persons on that point, such as a current of 15 ampères. Mr. Preece severely criticised the Guardian rules, and it seems to me that he rather misapplied the rule to a very important matter with which it has really no connection. I am very much obliged to you for allowing me to make that statement.

The Earl of CRAWFORD: Mr. President,—Before this subject The Earl o Crawford. is dropped I wish to say a word as to the question of rules that are put up for the guidance of electrical contractors in wiring

The Earl of
Crawford.
houses. A short time ago I had the honour of being admitted to the committee appointed by your Council to consider these rules. I then spoke, urging them not to go into the question of multiplication of rules. I took the liberty then of expressing my views to them, and of saying that where rules are multiplied you are multiplying weaknesses. There are certain points which are strong and common to all. Every set of rules has weak points. The more rules you have, therefore, the more weak points exist. The result really comes to this—that where you begin to set up an installation, and have it wired by contract, any contractor who is not of a high moral tone would take advantage of them, and take all the weak points together : he is still strictly within the letter of the law, but still has set up the installation really in a non-satisfactory state.

I think that what Mr. Heaphy says with regard to the Phœnix Office is very true. Some considerable time ago—I believe in 1882—the rules since known as the Phœnix rules were first published in the *Times* newspaper. Since that time the Phœnix Office has devoted its attention very largely to the subject of electric lighting—one which is vital to us all. Other offices at that time were almost unanimous in saying, " Electricity is an " unknown factor to us—one with which we may burn our fingers, " and one with which we will have nothing to do." To the Phœnix we owe a debt of gratitude for having set up these rules; and not merely have they set up these rules, but they have advanced with the science that we all have at heart; and with every new discovery the Phœnix have invariably gone into the question, investigated it, pointed out how it can be dealt with from the point of fire risks, and have modified or added to their rules so as to bring the new inventions into full play.

Well, we have found, gentlemen, at the Grosvenor Gallery that installations which have been set up in accordance with the Phœnix rules, and which have been passed by the inspectors of the Phœnix Office, are the most satisfactory of any that are done, or that can be come across. We have certainly as large an experience as anybody in this kingdom. We have at the present time a very considerable number of lights running. We have an

output of something like 20,000 lights daily; we are putting on installations at the rate of something like ten houses a week. We find in certain cases—I will not give any particulars, but leave you to guess to what I allude—that work is not done to the satisfaction that it should be done to the fire offices: the result of that is that under certain rules work ·may be passed by inspectors " to " their satisfaction," but it is absolutely and entirely prejudicial to our interests as producers of electricity and as distributors of current, because the work, in our opinion, is absolutely unsafe. I may inform you now, gentlemen, that, having this in view, we have come to the determination at the Grosvenor Gallery that in future any installation that comes to us which is not wired under the Phœnix rules, which has not been inspected for the Phœnix Office, will have to be inspected for our satisfaction under the Phœnix rules before we turn the current into the house. In saying this I am aware that it is a strong statement to make; but we have a very great stake in this business: we feel that the extinction of the light suddenly .in any place of public resort, owing to what we *know* to be the defective work in putting it in—I do not say that necessarily the defective work is due to the contractor, but I do say that in many cases it is the fault of the person who employed him to put it in—is an evil so serious that we are bound to do all we can to guard against it. A man cannot make a racehorse out of a donkey, and a man also cannot get the best article unless he is willing to pay a proper price for it. That is the plain English of the matter. I think the sooner the bogus installations are stamped out in this trade, the sooner we shall have the electric light taking its position as a recognised commercial business in London.

There is only one other thing, Sir, if you will allow me for one moment to hark back upon my story. I think that Mr. Crompton may have made a little mistake. He spoke of our using at the Grosvenor Gallery 18 lbs. of coal per horse-power, and said he could prove it. I am afraid he has misplaced a decimal, and should have said 1·8 lbs. of coal.

Mr. C. J. WHARTON: The question of these rules is of very great importance to two classes of persons—one the electric light

Mr.
Wharton.

contractors, and the other the insurance companies. The question is, Do we require any alteration in the standard up to the present? I will take the Phœnix rules as the standard. The contractors, I take it—those who do honest work—do not require any alteration. Fires have not occurred under the rules, and although Mr. Heaphy is strict, he is not too strict. Do the fire insurance companies require any alteration? I think not. Up to the present moment, I believe, a fire has never occurred where the Phœnix rules have been carried out. Those are the two classes of persons most concerned in the fire rules. It appears to me that neither of them require a change, and if any alteration is made it must be for the worse. Therefore it does not strike me, as a contractor, that any alteration is required. As a matter of sentiment Mr. Heaphy has put it very ably to-night; and you must all acknowledge that he has done very good work, and is entitled to all the honour attaching to these rules.

Mr.
Shoolbred.

Mr. J. N. SHOOLBRED: Before this discussion closes, might I ask Mr. Preece if he would refer in his reply to a subject—the heating of conductors—which is a possible source of danger in electric lighting? It is generally understood, that the same density of current will affect the temperature of different sizes of wire in a very different degree. Mr. Preece is understood to have carried out certain experiments on the subject, which would be very interesting to many members of the Society.

With regard to the discussion generally which has taken place, may I be allowed just to mention, that I think a good deal of feeling has unnecessarily been imported into it, by the non-attendance to one particular paragraph in the rules recently issued by this Society—that is, the second paragraph, in the commencement, which says: " It is to be understood that these general rules are " not intended to supersede any detailed rules which fire offices " may issue for their own protection "? I think I understood it to be so meant by the committee generally—and I attended at many of the meetings—that there was no intention whatever to create, as it were, antagonism between these rules and those certainly of the Phœnix, or of any fire office at all. The re-appointment of the Fire Risks Committee was, I understood, at the request of

several of the fire offices, in order to lay down certain scientific Mr. Shoolbred.
principles————

Mr. HEAPHY: Certainly.

Mr. J. N. SHOOLBRED: Upon which the fire offices themselves, perfectly independently of the Society's rules, might form rules for their own guidance, taking the former into consideration. So that, while the Society's rules are to a certain extent abstract scientific principles, the fire insurance companies' rules have to deal with certain risks and concrete matters which the Society's rules do not pretend to, and it would be out of place that they should do so. But it appears to me that this point has been lost sight of very much in the discussion, and the Society's rules have been held up as being directly antagonistic, especially to the Phœnix rules, as though one or the other must exist, but not both. I think I am correct in saying that it was not the intention of the committee (perhaps I shall be kindly corrected if I am wrong) to attempt to suppress the rules of the Phœnix Office or any other office at all, but simply to give certain scientific advice which might, or might not, be of use in framing the regulations which those fire offices thought it their duty in their interests, and in that of their clients, to apply to their own particular business.

Mr. MUSGRAVE HEAPHY: I have not said one word against Mr. Heaphy. the Telegraph-Engineers' rules, and I think those remarks of Mr. Shoolbred do not apply to me. I have not criticised the rules at all.

Mr. M. R. PRYOR: I should prefer not to speak in any way on Mr. Pryor. the part of the fire offices generally, as I might perhaps, but only to say that I cannot quite admit Mr. Heaphy's position, or understand why he should feel surprised that the other offices do not accept the statement on that edition of the Phœnix rules which he produced, that the rules were accepted "by the leading fire "offices," because they were not so accepted, although they were accepted by some important offices. Therefore there was some little feeling aroused.

With regard to these rules and the reason why my own office did not accept them. The reason is this: The fire offices are not in a position to protect the contractors for the supply of electric

Mr.
Pryor.

light from leakage taking place in installations. They quite admit that, on the whole, good installations are safe installations, and, therefore, satisfactory to them, but they have no concern with any possible waste at the expense of the consumers or the contractors; they wish to protect themselves against undue fire risk, and by so doing they will prevent increase of premium against the public when electric light installations are established. They are therefore anxious to lay down rules; but I think I may say most of them are not anxious to make it necessary that inspectors should be employed in all cases. It appears to us to be no more our business to inspect electric lighting installations than to inspect gas lighting installations; we have nothing whatever to do with anything but estimating the risk produced by any given cause, and there we intend to draw the line. In the present experimental and tentative state inspection is necessary, and we make it; but we cannot look upon that as a permanent state of things. We therefore cannot accept rules which are drawn so as to involve the necessity for satisfying certain inspectors; they are not, from our point of view, workable rules. We wish to see rules laid down which shall cover principles and be fairly easily applied; and, where tests are necessary, that these tests should be easily made. Beyond that we do not wish to go. It is perfectly clear that if electric lighting takes the place which I trust it will take, the inspection of every installation in every house would be perfectly and absolutely impossible, and would be a great nuisance to the householder, and no benefit whatever to the insurance office. Therefore the Phœnix rules are, from my point of view, inadmissible.

Mr.
Rawlins.

Mr. W. RAWLINS: As a member of the Society and surveyor of the Kent Insurance Company I have to claim your indulgence for a few moments. I regret that I was not here on the last occasion to have heard the complimentary remarks which Mr. Preece made on our rules, but probably he only meant them to be taken in a Pickwickian sense. I daresay it has struck you gentlemen as being rather singular that two members of the Council who spoke on the last occasion should, after their arduous labours, extending over nearly twelve months, have been

Mr.
Rawlins.

anxious to throw over the new rules of this Society, which they
themselves assisted in framing, for those of the Phœnix Office,
which are not, I should consider, favourable to electricians
generally, for they seem to point to the necessity of the surveyor
of the fire office superintending an installation on behalf of the
insurers, and then on behalf of the fire office passing judgment on
himself.

With regard to the Phœnix rules, there is just one observation
I should like to make. One rule states that the conductor shall
be of such an area as to allow at least 100 per cent. more electri-
city to pass through it than shall ever be required for the lights;
then it goes on to say that in certain instances the inspector may
allow more current to pass through the wires, and under some
circumstances he may require less. Problem: Required to find
the area of conductor necessary. The rules that have been
brought out by several offices, and which have been arranged
mainly by Mr. Human and myself, have these advantages: they
are not copyright, no charge is made for them, and they do
not require the services either of an honorary or a paid inter-
preter. These rules were drafted the latter part of last year,
because it was doubtful whether the new rules of the Society
would ever see the light, and the rules of 1882 and 1883, under
which we had previously worked, were in abeyance. We hoped
that our requirements being, as we considered, clearly stated,
contractors would be able to carry out their work without the
slightest friction with the fire offices.

Mr.
Hammond.

Mr. R. HAMMOND: I would say just one word to suggest to Mr.
Preece that in his reply he might take advantage of the leading
fire insurance companies being represented here this evening to
impress upon them the great advisability of not only admitting
electric-lighted premises at the same rate as gas-lighted premises,
but at a considerable reduction. I think the greatest good done by
these rules is that they have put the industry on a proper basis.
We are now beginning to be more and more recognised. Some of
us have struggled for years in trying to prove that electric-lighted
houses were even much safer than those lighted in any other form,
and have sought to get this fact acknowledged; and now that we

Mr.
Hammond. have got all the insurance people together here, let them under-
stand what we are really striving for, which is about 15 per cent.
reduction in premium whenever electricity is used for lighting.

Mr.
Preece.   Mr. W. H. PREECE, in reply, said: The discussion on this
paper has taken a very different course and a very different line
to that which I contemplated when I undertook to bring before
the Society the subject of "Fire Risks incidental to Electric
"Lighting." My hope was that by bringing before you several
examples of serious causes of accident we might have elicited
from members of the Society other cases of other accidents that
to the majority of us were novel and interesting. Unfortunately,
owing to the necessity of my having to refer to the action of the
committee on the rules that have just been brought out by the
Society, matters quite foreign to the risks incidental to electric
lighting have been imported into this discussion. I do not
propose to refer to those burning questions. I simply wish to
refer to one or two points that are more of a scientific interest
than a personal one. I should like, first of all, to point out that
some speakers seem to think that the risks connected with
electric lighting, and the rules drawn up to reduce those risks to
a minimum, are questions that simply affect the insurance offices
and contractors. I protest against that with all my power, and I
say that the people who are most interested are the users of the
electric light.   .

I said that the chief object and chief advantage of rules, and
the chief reason why the Society of Telegraph-Engineers and
Electricians took up the subject at all, was to protect the users of
electric lighting from what I called the rapacity of certain con-
tractors. Of course there are contractors and contractors, as we
all know; the struggle for existence and the competition in the
trade is so severe that there are contractors who give orders to
manufacturers, and there are manufacturers who would actually
not execute the orders given.

Again, this discussion is, to my mind, a proof of a very old
proverb, and the discussion just ended by Mr. Crompton was also
a proof of the same proverb—that "a man convinced against his
"will, is of the same opinion still." I do not know that really

either the result of the great contest we had on the question of Mr. Preece. alternating-current and battery distribution, as well as the discussion on fire rules, has changed the opinion of a single man on the question. Those discussions have afforded us a good deal of amusement, and we have passed three or four evenings this session, I think, far more pleasantly than we should have passed them if we had visited either a theatre or a music hall. I am not quite sure that we have added very much to the information or to those scientific facts that this Society was originally constituted to distribute to its members.

Just one point. I do not like to say one word—after what I have said I scarcely like to refer to my friend Mr. Heaphy again— but I do think it is only right to mention that this green-covered pamphlet that has been flourished before you was not printed by the Society of Telegraph-Engineers ; it was printed and produced in that form by the Phœnix Fire Office.

Mr. MUSGRAVE HEAPHY: At the desire of the Society of Telegraph-Engineers.

Mr. W. H. PREECE: I merely mention that, because I do not wish to allude to that part of the subject beyond saying that I repeat every word I uttered the other night as to the good that Mr. Heaphy has done towards the improvement of electric lighting. I do not want to raise any further discussion, but I do want to say this—that if the Society, or if I, as the chairman of the committee, or as one who takes a great personal interest in this question of the distribution of the electric light, can in any shape or form further the interest of our science by bringing together the fire offices and the Telegraph-Engineers, or by doing any mortal thing, I am always ready to do it.

One point that is of a little scientific interest. Mr. Human referred to the rule by which we could measure the leakage on a system by taking 1-5000th part, and wanted to know how we could easily detect the 1-5000th part of 15 ampères. Well, I do not suppose there is a single telegraphist in this country,—I do not think there is a budding electrician in this room,—who could not with the simplest galvanometer in his possession measure at once 3 milliampères, and 3 milliampères is the 1-5000th part of

15 ampères.   So that I think, therefore, the system of measuring leakage by taking the figures we name is a matter of very great simplicity.

Another interesting question was asked by Mr. Shoolbred with reference to the heating of wires.   This relation between heat and currents is one that has occupied me more than any other question for the last five or six years.   Scarcely a year has passed that I have not brought in some shape or other a paper before the Royal Society, or before this Society, or before the British Association, developing the relations that exist between the heating effects of currents and currents; and the last paper that I read was not very long ago, where I gave the constants that will enable you to determine with great accuracy the current that will fuse any wire of any size of any material.   I have been endeavouring in the last few weeks to apply this same formula and these same constants to the case of wires heated to a given temperature above the normal temperature of the atmosphere.   Professor George Forbes gave us some four or five years ago a table in which the currents required to raise wires to 9° and to 26° above the normal temperature were calculated; and Mr. Robert Sabine not long before his death did something of the same kind; but both these gentlemen based their calculations upon a constant determined by Mr. Macfarlane in Sir William Thomson's laboratory at Glasgow: he showed the emissivity—that is, the rate of loss of heat per unit area—on the surface of a copper sphere when polished and when blackened. There is every reason to believe that this constant of Macfarlane's is not applicable to cylinders.   Mr. Box wrote a book in the year 1868 where several tables were produced showing the influence of heat in contact with air, and he showed that the law determining the emissivity of spheres and cylinders was different.   I am quite convinced from the experiments I have made that we are a little bit in the dark on this question, and I hope before this session is over that I shall be able to put the results of these experiments in such a form that anybody may calculate with exactitude what temperature a wire will acquire with any given current.   We can get at it approximately, but I am afraid we are now 30 or 40 per cent. out.

I do not know that there is any other question that I have to *Mr. Preece.* answer. I can only, in concluding, express a certain amount of regret on my part that my original notion has not been carried out. But it has this advantage—that while there is no question that is of more interest or of more consequence to us than the risks of electric lighting, that question remains to be solved, and therefore there will be a reason why on some future occasion some one more competent than myself will be able to bring this question before the Society and let it have a full and free discussion.

The CHAIRMAN : I am sure you will all join in according your *The Chairman.* hearty vote of thanks to Mr. Preece for his interesting paper. It has certainly given rise to a lively discussion, though perhaps not so instructive as some of our discussions have been.

The motion was carried by acclamation.

The CHAIRMAN : Mr. Crompton wishes to make a short explanation.

Mr. R. E. CROMPTON : I think before we separate it is only *Mr. Crompton.* justice to Professor George Forbes that I should say a few words on a point in which we are fully in accord. He reminds me of a fact that I had forgotten, otherwise I should have stated it in the body of my paper : that he was one of the first to propose the very important improvement in main-laying which has been mentioned in my paper—*i.e.*, the great advantage of bare copper underground conductors. He worked at the subject at the same time that I did, so that the credit is as much due to him as to myself ; and I am very happy, after the severe criticism I have had to make on his remarks, to testify to this point.

The meeting then adjourned until May 24th.

An Extraordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, May 24th, 1888—Mr. E. GRAVES, President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to be suspended.

The following transfer was approved by the Council :—

From the class of Students to that of Associates—
H. E. Mitchell.

The following paper was then given :—

ON HIS NEW STANDARD AND INSPECTIONAL ELECTRIC MEASURING INSTRUMENTS.

By Sir WILLIAM THOMSON, D.C.L., LL.D., F.R.SS. (L. & E.), Past-President.

The general principles and many of the details of the instruments placed on the table for the inspection of the meeting are already well known, having been described in the electrical journals. It has been considered, however, that there are members of this Society who might like to see some of the instruments themselves, and to have a few explanations, which I may be able to give personally, regarding details that have not hitherto been published ; and that is my excuse for bringing before the Society a subject of which so large a part has already been published and is known to members of the Society.

All of the instruments which you see on this table fall under the designation of standard electric measuring instruments, and all except one depend upon Ampère's discovery of the mutual action between fixed and movable parts of an electric circuit. The one instrument which I except is that which I hold in my hand—a portable marine voltmeter or milli-ampere meter. This

instrument, for instance, is a milli-ampere meter as it is now before you, but with the addition of resistances wound on the tubular guard surrounding the stretched wire (which I shall describe presently) it becomes a voltmeter. Resistances amounting to about 930 ohms of platinoid wire wound upon these cylinders render the instrument a voltmeter, capable of measuring through a range of from 90 to 120 volts.

In enumerating the standard instruments I said that with one exception those to be explained this evening are founded upon Ampère's discovery, and all come under the designation of standard electric measuring instruments. But there is another instrument on the table, which I shall at this time merely mention—a magneto-static current meter. This type of instrument I use as an adjunct to the series of standard instruments because it is exceedingly useful in extending the scale above and below the range of any one of that series. Two of the magneto-static species with one of the standard instruments give the means of measuring currents through the whole range from one milli-ampere up to several hundred amperes.

I shall briefly describe the marine voltmeter before passing on to the other standard instruments. It consists essentially of an oblate of soft iron suspended in the centre of a solenoid. I am sorry that I am not able easily to open up this instrument in order that you may see the interior, but I may tell you that I have a more recent instrument fulfilling exactly

Fig. 1.—The Portable Marine Voltmeter.

the general description I shall give of this type, but with somewhat more convenient mechanical details, in which, by simply

taking out a screw, I can remove the cap and show you the working parts. The resultant action between the electromagnetic force due to the solenoid, and the oblate of soft iron with its equator oblique to the lines of force, is a couple; and that couple is balanced by the torsion of a stretched platinoid wire on which the oblate is suspended, the platinoid wire being kept stretched to about one-third its breaking weight by two springy arrangements at its two ends. The sketch on the board represents the type of an instrument which is being made in Glasgow. In *that* type the bobbin is divided for convenience, the ground plan showing two halves of the bobbin, which can be put together with great ease; between them there is a hollow cut out, sufficiently wide to let a fine platinoid wire pass down through the centre, bearing in the axis of the solenoids an oblate of soft iron. The instrument before you is constructed with a single bobbin, and has on that account complicated details which I need not now speak of, because I believe that I shall abandon them altogether. I feel confident that I shall not adhere to the type of single bobbin, considering the much less simplicity that it involves in the more delicate and vital part of the instrument, viz., the suspension of the oblate and the index-needle, which in the new form will be lighter, and therefore better adapted for working at sea, and less liable to damage from any very rough usage that it might experience in carriage on shore. To explain the principle of the instrument, I may remind you that, as Faraday long ago showed, a long bar of soft iron tends to place its length along the lines of force in a uniform field of magnetic force; an oval or a flat disc tends to place its equatorial plane along the lines of force. I had to choose between an oval and an oblate, and I found that the oblate

FIG. 2.—Section of Marine Voltmeter.

was better—that a larger magnetic moment was available in the circumstances by choosing an oblate than by choosing an oval. In the oblate used the equatorial diameter is rather less than a centimetre, the polar diameter rather less than half of that amount.   The tendency of the oblate to place its equatorial plane in the direction of the lines of force of the solenoid, is the force due to the electro-magnetic action, which is balanced mechanically by torsion in the use of the instrument.   The principles upon which I was led to the choice of this configuration of iron depended upon the circumstance that when we do not use excessively high force in the magnetic field, so that the magnetisation of the iron shall be very far short of saturation, a figure of a short character, in which there is no one diameter very much more than twice as long as any other diameter, is much less disturbed by magnetic retentiveness than an elongated figure. In the instruments of this class the electro-magnetic force is very nearly in exact proportion to the *square* of the strength of the current, which is rigorously the law of the Ampère-force used in, and in that respect they bear very much resemblance to, the gravity-balance instruments, by which the highest possible accuracy is to be attained.   On the other hand, in instruments of the class to which Ayrton and Perry's ammeter belongs, the force is more nearly in simple proportion to the strength of the current, owing to iron being used nearly in a state of saturation.   These details will no doubt be interesting to some members of the Society, for all are scientific enough to know that consideration of details is of the very essence of success in practical work.   The elements of electrical and mechanical science—rudimentary drawings of levers, springs, solenoids, and magnets—are valuable, but without the application of physical science in the construction of electrical instruments —without consideration of the qualities, both electrical and mechanical, of the matter involved in the design of the details— the instrument or piece of apparatus will not answer its purpose, and it is really the working out of such details that is the life-work of anyone who is engaged in practical science.

I wish to point out one difference between this instrument

and the type now being made. In this species of instrument the torque* due to the electro-magnetic action is balanced by the elastic torque, or the torsional couple, as it is more commonly called, in this twisted wire. In the instrument before you we have a latent zero, and to verify the zero I must undo the torsion by turning the torsion-head accordingly. The range of the instrument is essentially limited. As it stands just now it is made to read from 90 volts to 120 volts. Indeed, it is impossible to make this a very satisfactory *long-range* instrument, except by graduated resistance-coils. The needle is not allowed to come to the zero at all, and my reason for that is that it was only so that I was able to eliminate practical error from change of zero in the copper and platinum wires which I used until lately, when I adopted platinoid. The elasticity of fine platinoid wire has proved so good that, after a year's use of the instrument continuously in the electric light circuit of my house, I found, on testing it by an absolute standard, that the latent zero had not changed more than one-fifth per cent. in its effect on the indication of strength of current. I found, in fact, no sensible variation whatever when I took the twist out and verified the zero, afterwards replacing the twist. We all know that the permanence of the elasticity of this piece of wire may be considered as quite perfect; so that now or a hundred years hence, or even as many million years hence as would give the earth a very different gravitational force, this wire will remain constant. So that in one respect this portable marine voltmeter is a more constant instrument for all time and for every place than any gravitational instrument can be. At the present time gravity differs notably in different parts of the earth, being one-half per cent. less at the equator than at the poles. But for all practical purposes you must feel that gravity is the most constant force we have to deal with; although, looking at the elasticity of metals, and

* The word "torque," introduced by my brother, Professor Jas. Thomson, often saves a great deal of awkwardness, and is exceedingly convenient when we have to speak, not of a couple of forces, but of a system of elastic or other forces balanceable by a Poinset couple.

looking forward a sufficient number of millions of years, it is something interesting to know that the elasticity of a piece of metal is a more perennial standard than gravity itself. Either elasticity or gravity may therefore be considered as practically quite perfect, provided the material of the spring does not alter in any way by becoming rusted or by evaporation going on, but retains its qualities unchanged from age to age. The marine voltmeter is really an accurate and standard instrument, while it is perfectly portable and bears the roughest usage. The platinoid-wire spring has been kept stretched for a considerable time, and heated to the temperature of boiling water, and after such treatment—or *ageing*, as we may call it—the instrument preserves its constancy ; and we find that turning it upside down, knocking it roughly down on its side, or carrying it in a cab through a rough street, does not alter the indications at all. But if even still more rough usage, or long-continued rough usage, or accident, should alter the indication, we have a ready means of adjusting the zero, even in this form now before you (and still more easily in the improved form), and so can absolutely reinstate it as a standard instrument at any moment. The two things that require constancy to render this a perennial instrument are the oblate and the platinoid wire. These are constants that we regard as practically absolute, so that from generation to generation that little instrument will really be a standard current meter.

This instrument has not yet been much used at sea. I have not pushed it forward, because I have been anxious to satisfy myself as to some of the mechanical details ; but an instrument exactly like the one before you has been in use on board one of the State Line steamships (the " State of Nevada "), and has now made five or six voyages across the Atlantic, between Glasgow and New York. In the first four voyages it saved breakdowns, from over-incandescence, estimated at about 30 lamps, and from that time forward the consumption of lamps has been very much less than formerly.

The other standard instruments now before you are all

founded, as I said, on the mutual forces between the movable and
fixed parts of an electric circuit. I will take these instruments
in order, beginning with the one adapted to the measurement of
the smallest current. The useful range of the first instrument of
the series—the centi-ampere* balance (Fig. 3)—is from 1 to 50
centi-amperes. The next instrument is the deci-ampere balance,
a specimen of which is now before you. The centi-ampere and the
deci-ampere balances are quite similar in appearance, the differ-
ence being entirely in the number of turns and the thickness of
the wire in the coils. The instrument now before you may be
taken as representing the deci-ampere or the centi-ampere
balance, but the type which is made now differs somewhat from
the type before you. The type before you would not be improved
by the difference if it were to be used solely for currents in one
direction ; but if it were to be used for alternate currents, the
copper bobbins on which, as you see, the wire is wound, and
the thick double-sheet copper constituting the sole-plate and
frame of the instrument, would, by the currents induced in them,
interfere very seriously with the indications. Accordingly, for
the instruments to be used for the measurement of alternate
currents, the type, while it is exactly the same in principle as
that instrument, is like another of the instruments on the table
before you—the deka-ampere balance—in which we have a slate
base. Passing over the ampere meter for a moment, the range
of the deka-ampere meter is from 2 to 100 amperes. This is a
type of which a considerable number were made for Sir Coutts
Lindsay and Company for the Grosvenor Gallery installation, at
the request of Mr. Ferranti. The deci-ampere meters and the
deka-ampere meters of this type have been made for the purpose
of being available for alternate as well as for direct currents, and
therefore they have a slate base, and slate cores and slate ends for
the coils, instead of copper ; but in other respects the arrange-
ments are the same as in the instruments shown.

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* The French nomenclature is, whatever classical critics may say of it,
exceedingly convenient and valuable—the Latin numerals for the sub-
multiples, the Greek numerals for the multiples—milli-ampere (the thousandth
of an ampere), the centi-ampere, deka-ampere, hekto-ampere, and kilo-ampere.
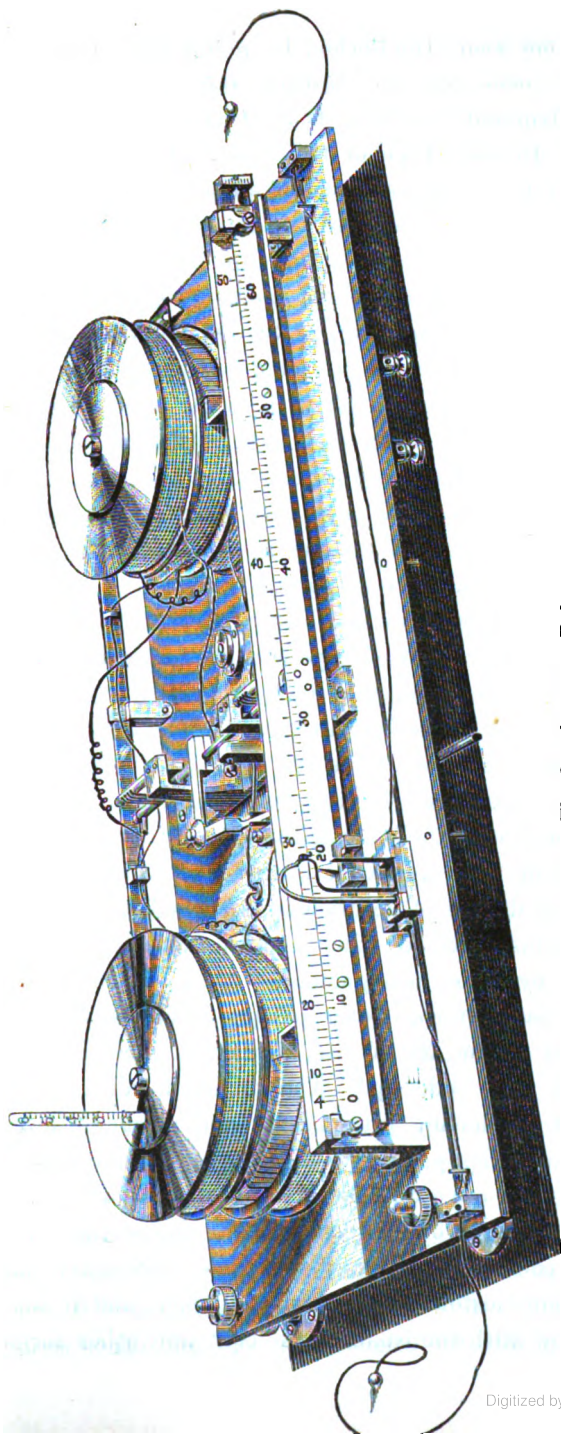
Fig. 3.—The Centi-ampere Balance.

I will not weary the Society by going too much into details, and pass over without further reference the principle of these instruments, because that, I think, you all understand already. In each type of this series of balance instruments, except the hekto-ampere and kilo-ampere balances, the movable part of the circuit consists of two coils, one carried at each end of a horizontal balance-beam, and the fixed part consists of four fixed coils placed one above and one below each movable coil.

Look at this instrument (Fig. 3). The movable coil here at one end of the balance-beam is attracted down by the coil below it and repelled down by the coil above it, while at the same time this movable coil at the other end of the balance-beam is attracted up by the coil above it and repelled up by the coil below it. We have thus two *movable* and four *fixed* coils in this type of instrument, and the advantages so obtained cannot be had with anything simpler. We might take away the two upper fixed coils, and leave only the two lower coils, but a slight change in the height of the axis of suspension, as might be due to a slight stretching of the suspension ligament, would introduce a sensible error in the use of the instrument. On the other hand, we have a place of *minimum* force for the middle position of each movable coil between its two fixed coils, and by choosing the sighted position of the index to correspond very nearly to the place of minimum force we have a condition of things suited to give the highest degree of accuracy in the results. I may just give you an illustration of the practical accuracy and hardiness attained by this arrangement. As a practical matter, my ambition has been that boxes passing from the workshop of my instrument maker, Mr. White, should be labelled " Glass—with-" out care ; any side up." That is the ideal, but we are very far yet from having attained it ; and although any of these instruments *when packed* may be turned upside down, somehow or other there are a number of mischances that do happen, and which cannot well be guarded against. One of these instruments, carefully packed in a box marked " Glass—with care," sent through the Belgian Custom House, arrived broken, and it was returned to Glasgow with the balance-arm bent and other serious injury,

which showed that the damage was not due to the breakdown of
the suspension ligament or anything of that kind, but that the
box containing the instrument had probably simply been let fall
upon a stone floor in the Custom House, and been subjected to
carelessly rough usage. The arms were simply re-straightened,
the balance re-suspended, and everything replaced by the instru-
ment maker. It was then sent up to my laboratory and tested,
with the result that it was found to agree to within one-twentieth
per cent. with its indications before it met with the accident
giving that very disintegrating rough usage. Now that kind of
accuracy can only be attained by working in the neighbourhood
of the minimum or maximum.

Here are a few particulars as to the resistances, number of
turns, and so on, in the coils.

In the centi-ampere balance, of which the range is from 1 to
50 centi-amperes, each movable coil contains 440 turns of No. 30
copper wire, resistance 19·6 ohms, say 20 ohms (it depends on the
temperature, of course); each of the four fixed coils contains 1,295
turns of No. 26 copper wire, resistance 30 ohms; total resistance
of the instrument, 161 ohms. That is a large resistance, but to
have a standard instrument for measuring such small currents I
do not see any way of doing it without a large resistance; and,
after all, it does not take a great number of cells to give the cur-
rent, and in most places where the instrument is used there is
sufficient power to give the electric potential for the useful
current.

The deci-ampere balance, with a range of from 1 to 50 deci-
amperes, has a total resistance of only 2 ohms. The two movable
coils have each 64 turns of No. 17 copper wire, resistance ·18
ohm, and the four fixed coils each 166 turns of No. 17 copper
wire, resistance ·428 ohm in each; total resistance of the instru-
ment, 2·072, or say 2·0 ohms. The deci-ampere balance has a
great advantage over the centi-ampere balance; for, although
founded on exactly the same principles, there is in the former a
far smaller proportion of space occupied by insulation, and a much
larger proportion of space occupied by copper, in the vital part of
the instrument; and thus we have a much more than proportion-

ately small insulation, if we calculate the proportion of the dimensions on the idea of the different instruments being exactly similar. The different instruments are essentially not similar, because of the insulation being necessarily so much thicker in proportion with fine wire than with thick wire.

The ampere balance here before you is of the newest type, and is not adapted for the measurement of alternating currents; but a like instrument, slightly less advantageous, can be made for that purpose. Its range is from ·5 ampere to 25 amperes. The two movable coils each contain 10 turns of copper ribbon 1·2 centimetres broad and ·11 centimetre thick, and four fixed coils each contain 37 turns of copper ribbon 1·8 centimetres broad and ·07 centimetre thick; the total resistance of the instrument being about ·18 ohm. The insulation of these coils is very fine paraffined paper, which takes up exceedingly little space.

In the hekto-ampere balance (Fig. 4) and in the kilo-ampere balance I have dispensed with one feature of importance with reference to the very highest accuracy, and that is the complete and thorough realisation of the minimum principle. In these we have a single coil—a single rectangular ring rather, not a coil—of copper, movable, and a fixed rectangular coil or ring of copper below it. In the hekto-ampere balance the fixed ring consists of 10 turns of thick copper ribbon insulated from one another, and the range of measurement is from 10 to 500 amperes. In the kilo-ampere balance, whose range is from 50 to (50 × 50 =) 2,500 amperes, the fixed coil is made up exactly as in the hekto-ampere balance, with the difference that instead of insulating paper between the turns they are soldered together and the whole forms a single ring. This method of making the fixed ring was adopted because we found great difficulty in getting forgings or castings of copper of these dimensions. The very first instrument of this kind, which was made to the order of Messrs. Paterson & Cooper, had for its fixed rectangle a forging of copper; but we had to wait a long time for it, and, when it came, the forging was not very satisfactory. I have therefore come to the conclusion that, for the present at all events, it can be made more easily, and with more certainty of a thoroughly satisfactory
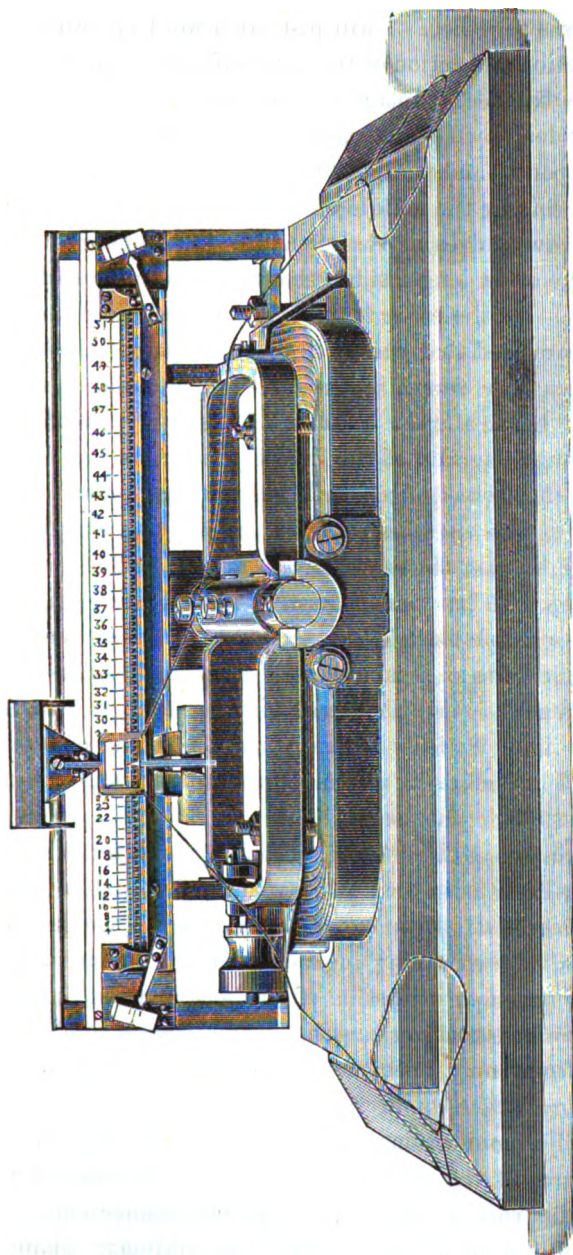
Fig. 4.—The Hekto-ampere Balance.

result, by making it of a copper ribbon and soldering the different turns together. I will just say a word or two regarding the construction and action of the hekto-ampere balance and of the kilo-ampere balance. Imagine a rectangle of copper about $2\frac{1}{2}$ centimetres deep by $1\frac{1}{2}$ centimetres thick, movable about an axis through its centre and parallel to its shorter sides, and imagine a current entering by the middle of one of the longer sides, dividing between the two halves of the rectangle and coming out at the middle of the other longer side: that is the movable part of these instruments. On the other hand, the fixed coil of either instrument is so arranged that the whole current passes through it in one direction, once round in the kilo-ampere balance, ten times round in the hekto-ampere balance. Thus it is evident that the current through one side of the fixed coil attracts the current in that side of the movable coil, while the current through the other side, going in the opposite direction, repels the current in the movable coil, so that the movable coil is tilted in the manner of a balance-beam. It has been advisable in this case to give up one part of the principle for highest accuracy, because it would make it so very cumbrous an instrument to carry, and because the suspending ligament is very short and very powerful, so that there is exceedingly little liability to error due to possible change in its length; and therefore I ventured upon the simple form and arrangement which you see.

There are two or three details which I am afraid I must pass over, as the hour is so very far advanced, but just a few words with reference to the ligament, which is, after all, the key of these instruments, as everything depends upon it. There is nothing new in the principle of action of the instruments, but some of the details of construction have involved a good deal of consideration, and have only been arrived at after a vast number of unsatisfactory attempts in various directions. Instead of knife edges, flexible connections for introducing the current to the movable parts of the circuit are used, and the whole weight of these movable parts is borne by the flexible connections. Weber, I think I am right in saying, made an ordinary balance with flexible springs instead of knife edges; and we all know the

admirable use of the elastic suspension in the pendulum of the common clock, and of the astronomical clock—an instrument of the very greatest accuracy that exists in the whole range of practical science : in the astronomical clock the elastic suspension is found practically superior to supporting it by knife edges.   I will not say whether ordinary chemical balances may or may not be made more advantageously with suspension ligaments than, as now, with knife edges; but for electric current-measuring instruments of this kind, in which, besides bearing the weight, we have the affair of introducing the current from a fixed coil to a movable coil, the elastic suspension has great advantages.   Well, after trying copper ribbons, and finding exceedingly unsatisfactory results through buckling, owing to the impossibility of getting the force uniformly distributed through each broad ribbon, I saw the desirability of trying narrow segments of ribbon, and thus developed the latest ligaments, which consist of very fine copper wires about $\frac{3}{4}$ centimetre long, laid close side by side, and numbering from 10 up to 1,000, according to the different types of the instrument.   The number in each ligament of this kilo-ampere balance before you is 800.   The ligament is not seen on account of the strengthening guard which is fixed across the middle of the instrument covering the ligament itself altogether and providing against rough usage and chance of injury.   To make this instrument portable a piece of brass is put in beside the ligament, and two screws are screwed down so that the whole weight of the movable coil is taken off the ligament, and the fixed coil is screwed firmly by the divided trunnion against this brass plate.

I have only now to say, in conclusion, that there is on the table before you a set of instruments capable of measuring continuously, and each adjustable by comparison with the one below it, through a range of from one milli-ampere up to 2,500 amperes—that is, from the thousandth part of an ampère to 2,500 amperes—or the ratio of 1 : 2,500,000.   It is exceedingly convenient to have absolute standard instruments through so wide a range, but most people will be perfectly satisfied with a much less range of *standard*; and accordingly I should just call attention to the range that we get by a deci-ampere balance ranging from 1 to 50

deci-amperes, a milli-ampere magneto-static instrument, and a deka-ampere magneto-static instrument, to extend the range of accurate measurement down to one milli-ampere and up to 50 or 100 or 200 amperes.

I have here a magneto-static instrument (Fig. 5) which is very
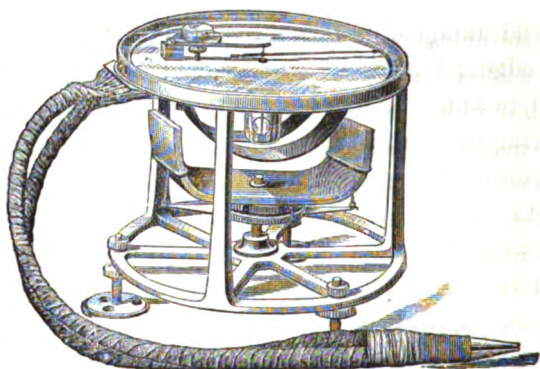
Fig. 5.—Magneto-static Current Meter.

conveniently set at one ampere to the division, but it may perfectly readily be set at either two amperes or half an ampere to the division. The index needle is carried by a silk-fibre suspension, and those who know the constancy of silk-fibre suspension in the laboratory would not willingly give it up for anything else. I showed this instrument at the British Association meeting at Manchester, and proved to the meeting the very rough usage it might get, and the tumbling about it might suffer, without breaking the fibre. The glass will break, any part will break, before the suspension gives way ; but the fibre will wear. I have learned from experience that a railway journey from Glasgow to London is very apt to *wear* out the fibre, though I believe that fifty years' usage *in situ* would scarcely wear it out. Accordingly there is in this instrument a little trouble in putting on a proper guard for the suspension, to make it perfectly portable and hardy ; but that trouble being taken, the fibre shows no tendency to break down, and is perhaps as perennial as any part of the instrument. We can adjust the zero in an instant, and the sensibility can be readily altered by raising or lowering the magnet, which may be securely fixed in any position by its clamping

screws. An inspectional instrument readable from one to 100 times one is, I think, practically only obtainable by the magneto-static method, and therefore, though this is not a standard instrument in itself, it is yet an instrument that is a very useful adjunct to absolute standard instruments. For a large variety of laboratory and practical purposes an equipment of useful instru-ments to give the means of measuring, within a quarter per cent., any current whatever, from a milli-ampere up to 100 amperes, might be as follows:—The deci-ampere standard-balance; for stronger currents, a magneto-static instrument with a coil consisting of a single turn of thick copper; and for smaller currents another magneto-static instrument on exactly the same plan, but with about 300 turns of very fine copper wire. With these three instruments the range from one milli-ampere to 200 ampères is very readily obtained; the magneto-static for stronger currents being standardised by the deci-ampere balance at the top of the deci-ampere meter's range, the magneto-static for smaller currents being also standardised by the deci-ampere meter at the bottom of the range of the deci-ampere meter.

With reference to the vertical scale voltmeter, it is designed for use in an electric light station, or at any place where it is desired to have an inspectional potential meter of the highest accuracy. The instrument before you is of the earlier type. I am sorry that I am not able to show you the most modern type, but one is at present in the Glasgow Exhibition as part of the exhibit of my instrument maker, Mr. White, of Glasgow. In the instrument now before you one movable coil is repelled by one fixed coil; but this construction I have modified to one movable coil at one end of the balance repelled down by a fixed coil, and another movable coil repelled upwards by another fixed coil at the other end of the balance. I departed from the simple construction because the instrument threatened to become, not a pure electrical measuring instrument, but the measurer of a com-pound quality partly depending upon very complex considera-tions of temperature and of moisture of the air, and only partly upon the things that we wish to measure; it was, in fact, a mixed hygrometer and voltmeter, and the hygrometric part of its

indications sometimes disturbed the electrical measurements
as much as a half per cent. The best way of eliminating that
was to take away the counterpoise weight and substitute a
coil, with, of course, a corresponding fixed coil; and that has
got over the hygroscopic error. One point remains—the tem-
perature correction. I wish to have no error amounting to as
much as one-fifth per cent. in this instrument. We have a
platinoid resistance here below, amounting to about 700 ohms,
and about 50 ohms or 60 ohms in the instrument itself, and the
change of temperature of the platinoid produces just a sensible
effect—a change of resistance with temperature—while the
change of resistance of the part of the circuit that is copper is
of course very considerable; but as this last is only a small
part of the whole resistance, the whole temperature error is
not large. With 20° or 30° temperature it might amount to
more than one per cent., and accordingly a thermometer is
used, and when an absolutely correct reading is required a weight
corresponding to the temperature observed may be placed in the
pan. But in the new type I have simplified that, and if one
wants to make an accurate measurement he looks at the thermo-
meter and sets a balancing flag to the number on the scale corre-
sponding to the reading on the thermometer; so that without
touching the weight at all the temperature correction is at once
applied.

The President. The PRESIDENT: The highly interesting explanation of these
instruments to which we have just listened hardly admits of dis-
cussion in the ordinary sense, but I daresay there are several
gentlemen who would like to make some observations on what we
have heard, and, perhaps, to ask Sir William Thomson some
questions.

Mr. Preece. Mr. W. H. PREECE: Sir, there are few members present to-
night who can boast of the practical experience that I can narrate
to you. It was my good fortune to have acted, a good many
years ago, as assistant to the great and immortal Faraday in
carrying out his classical inquiry into lateral induction on under-
ground wires. More than thirty years ago I learned how to use

Mr. Preece.

a differential galvanometer for the purposes of the exact measurement of resistance by an instrument devised by Dr. Werner von Siemens, and made by the house of Siemens & Halske. Later, all my experience has been acquired by instruments made by Sir William Thomson. Now we are singularly fortunate in having in this room to-night two of our great fathers in the art of exact measurement. We have in front of us Dr. Werner von Siemens, whose presence we greet with so much pleasure.

[Dr. von Siemens bowed his acknowledgments of the great applause with which this allusion to his presence was received.]

And we have heard from Sir William Thomson to-night the vast and wonderful range of his instruments of precision. Gentlemen, these are the two philosophers who have taught us all how to measure with scientific accuracy the electrical phenomena with which we deal practically. Many engineers imagine that when they can measure the millionth part of a millimetre by means of the microscope, and when they can judge of the distance between the earth and the sun with their telescopes, their instruments extend over an enormous range; but their range, combined with accuracy, is not to be compared with the accuracy with which we electricians can measure from the ten-millionth part of a milli-ampere up to currents reaching such figures as 25,000 amperes. And again, not only have we got this enormous range and this great exactitude, but we have the knowledge that, under the guidance of our lecturer to-night, we have instruments which impart in our minds the most implicit confidence. There is no engineer who can say with the same feeling of security, and the same determined firmness of purpose, that his measurements are right as we electricians can who use these instruments. It was, I think, Mr. Samuel Smiles who stated that attention to details was at the root of human progress. We have learned from Sir William Thomson to-night that attention to details has been his guiding spirit throughout all his scientific career, and it is attention to detail that we know is the very root of the accuracy of these instruments that he has brought before us.

We are extremely indebted to Sir William Thomson for the clear explanation of the able discoveries he has given us to-night.

Mr. Preece. We have all of us more or less seen descriptions of these instruments in type; many of us have seen them in various forms; and some of us are rather anxious to know when Sir William Thomson is going to stop. All his instruments are marked by wonderful accuracy, but he will not cease. He will insist on "painting the "lily and gilding refined gold." The instrument that he made in the year 1858 to measure the currents through the Atlantic Cable was, in its form, as accurate as any one on this table.

I propose that we accord our most hearty thanks to Sir William Thomson for his description to-night, and you have already accorded a greeting to our great guest this evening, Dr. Werner von Siemens.

The President. The PRESIDENT: Before calling upon any other speaker, I would ask Dr. Werner von Siemens if he would be kind enough to make a few remarks.

Dr. Siemens. Dr. WERNER VON SIEMENS: Unfortunately my knowledge of the English language is not sufficient to make any appropriate remarks on the subject.

Professor Ayrton. Professor W. E. AYRTON: I have not had any actual experience with the charming instruments that we see before us this evening, therefore it is impossible for me to speak about their use in practice; but what one can certainly say is this: My colleague and myself have been engaged for a long while in the design and manufacture of electrical measuring instruments; and others who, like ourselves, have been similarly engaged, know the extreme difficulty that exists in bringing any one single instrument to anything approaching approximate perfection. Some idea of an instrument is not very difficult to arrive at, but to make that instrument a working apparatus, to make it a measuring instrument, is attended with an enormous amount of difficulty. Well, if there is all that difficulty in getting success with each type, what difficulty must there have been to get success with the vast range of instruments that is before us on the table this evening! That Sir William Thomson should have been able, with all the scientific claims on his time, to devote himself, I should think almost without intermission, to perfecting what he has done, only makes one feel still stronger admiration of his marvellous powers,

about which it is almost impossible to speak satisfactorily and do Professor Ayrton. justice to.   One feels, if he will allow me to say so, as if one were speaking of a being residing in another universe, having powers that one does not quite understand, and which we can only look at with reverence.   That is really what I feel this evening, and what I have sincerely felt for the last twenty-five years.   Criticism is out of the question.   You may ask questions, but you cannot really criticise, because you feel somehow or other that even if the thing seems wrong it really is right; it is only that you cannot understand why it is so.

There is one question, however, which I would like to ask in connection with the first instrument that Sir William showed us, in which he mentioned that a platinoid wire is used.   I ask it for a very simple reason.   We have ourselves been trying to use platinoid wire for an electrical instrument for some time, but we have been met by the tendency of platinoid to oxidise.   I would like to know, if Sir William Thomson can tell us, whether he has been troubled with this difficulty, and, if so, what means he has adopted to get over the alteration in the elasticity of the platinoid by its slow oxidation.   Platinoid seems to oxidise like German silver, and is therefore unlike, for example, platinum-silver, which does not oxidise in the same way.

I have been appalled at the vast range of instruments before us this evening, but we must not forget that this large contribution to electrical measuring instruments which we have before us is but a very small part of the contribution that their inventor has made to electrical apparatus; indeed, almost all our delicate electrical measuring instruments are due to the same author. Nobody has used the quadrant electrometer with more pleasure, and at the same time with more agony, than I have—with pleasure because I have been able to do with it what it was impossible to do in any other way, with agony on account of its peculiar habit of getting out of order.   The advantage, however, much more than counterbalances the disadvantage.   As an example, some years ago, in Japan, Professor Perry and I were able to measure with a quadrant electrometer the chemical action of paraffin oil on metals—an action which our colleague, a very

Professor
Ayrton.

well-known chemist, was totally unable to detect by any known chemical test. The question was, Did paraffin oil, free from sulphuric acid, act on zinc? He tried to analyse some paraffin oil that had come out in a galvanised iron tank—from America I think it was—and was unable to find any trace whatever of any action. The electrometer showed that you could get chemical action at once of paraffin oil on zinc, proving that the electrometer test was infinitely more sensitive than any test that the chemist had any knowledge of at that time, or even now has. I do not know that one can say more. One might talk for a week and still not be able to say all one would like to say about the marvellous inventive power that we have seen this evening, and which is, in fact, a repetition of the inventive power that we have seen during the last fifty years exercised not merely in devising and perfecting electrical measuring instruments, but also the siphon recorder, the deep-sea sounding apparatus, the mariner's compass, &c. In fact, there is no single subject to which Sir William Thomson has given his attention even for a short time on which he has not left his mark, and left that mark very firmly impressed.

Professor
Forbes.

Professor G. FORBES: As the opportunity for asking questions has been suggested, I would like to remark, with regard to the voltmeter in which a water tube is provided for preventing chances of dangerous short-circuit when connecting high-potential mains or circuits, that I have noticed that with a high-potential alternating current the voltmeter does not act when this water tube is in, and I should be very glad if Sir William Thomson would tell us something on that point.

Sir William
Thomson.

Sir WILLIAM THOMSON: I might perhaps answer that now. Professor Forbes's observation was communicated to me———

Professor FORBES: I think I told you myself.

Sir WILLIAM THOMSON: And I have ascertained that there is no doubt whatever that his interpretation is correct—that the water does not afford a sufficiently rapid electrification to allow the electro static voltmeter to give the correct mean potential when applied to an alternating-current circuit, such as the Grosvenor Gallery installation, on which the observation was made. In such a case I would let the instrument take its chance as to what

Sir William
Thomson.

may happen in case of a potential at 2,000, 3,000, or 5,000 volts upwards when there is an internal contact, because the guard will not be a sufficient protection. The water communication in this tube would have been sufficient protection, but, as Professor Forbes has found, it is not sufficiently good to charge these plates in here. I did not describe the instrument because I was afraid of occupying the time of the Society too long. I am indebted to Professor Forbes for the observation.

Professor
Adams.

Professor W. GRYLLS ADAMS : I have really, Sir, nothing to add to what has been so well said already by Mr. Preece and Professor Ayrton. We all look with respect and admiration at the progress of electrical science in the hands of Sir William Thomson, and we are struck with his enthusiasm and with the amount of energy which he is able to throw into his work. We are thankful to him for bringing out a new instrument when it marks a distinct advance on what has been hitherto done, although perhaps, for some of us, these changes come pretty often and involve expenditure. Yet these are the steps by which we may attain to higher things. In this extensive array of standard measuring instruments each one who is engaged in electrical research may be able to find that special instrument which is the most satisfactory for his purpose. Very few will probably want to have instruments measuring from the hundredth part of an ampere up to 25,000 amperes ; but, whatever we may require, there may be some one instrument within this extensive range which will be exceedingly satisfactory for the purpose, and without which it will be impossible for us to. get on very much farther. In fact, we feel, when we have new standard measuring instruments of this kind, that we must go with the times and make use of the excellent tools provided for us, and do what we can for the progress of electricity as an exact science. I would yield to no one in my admiration of Sir William Thomson's work in this direction.

Dr.
Fleming.

Dr. J. A. FLEMING : As Professor Ayrton has already said, Sir, criticism in regard to these beautiful instruments is altogether out of the question ; we can only look at them with the most intense admiration, and listen with respectful interest to the

<div style="float:left">Dr.
Fleming.</div>

explanation that Sir William Thomson has given of their action. I am, perhaps, fortunate, however, in being able to contribute my quota to the debate in stating the experience I have had with them in the last two years. I have had the pleasure, from time to time, to hear from Sir William Thomson of the progress he has made, and to see in his laboratory the stages through which these instruments have ·passed in their evolution. The instruments before us, I think, are, so to speak, the surviving members of a long series of gradually perfected instruments. For the past two years I have had two or three of these instruments in my laboratory in almost daily use, and I can speak with confidence of the comfort that it gives anyone who is in the habit of making electrical measurements to have some instruments for measuring electric current on which absolute reliance can be placed. I have two instruments in use—one of which I think I am right in calling a deca-ampere balance, reading from about 2 to 100 amperes ; and another, which is not represented by those on the table, which reads from about a small fraction of an ampere to 3 amperes ; and it is this 3-ampere balance that is the most useful instrument that I possess. Some may be interested to hear that although the instruments look a little complicated at the first glance, yet, notwithstanding, they are exceedingly simple to use, and with these balance instruments it is possible to take a reading, with an accuracy to one-tenth per cent., nearly as quickly as it can be obtained by a tangent galvanometer, provided always that the current is steady. I must say that it gives me pleasure to be able to bear my testimony this evening, for what it is worth, to the great and satisfactory addition that Sir William Thomson has made to the long list of instruments for which the electrical world owes him an enduring debt of gratitude for giving us instruments for measuring electric currents which are absolutely reliable and which are perfectly simple in use.

<div style="float:left">Mr.
Swinburne.</div>

Mr. J. SWINBURNE : We have heard a great deal about what Sir William Thomson has done for instruments, but he has not given us a description of his mho-ohm-drum, a specimen of which I would like to see on the table. A class of instruments is coming

into use just now in France called the Deprez-d'Arsonval, and at Mr. Swinburne. first sight it might be thought, remembering how much superior they were to the ordinary Thomson reflecting galvanometer for many purposes, that Sir William had been cut out; but not a bit of it: it will be found that the principle is that of the siphon recorder, but the instrument is somewhat modified.

Mr. W. P. GRANVILLE: I would like to ask Sir William Mr. Granville. Thomson what provision has been made in the first instrument described to-night to guard against a possible source of error peculiar to all sensitive instruments which depend for their action upon the magnetism induced in soft iron.

When a rod of soft iron is magnetised by a *feeble* magnetising force, the amount of magnetism induced depends largely upon whether the iron is quiescent or in a state of mechanical agitation, the soft iron being unable, unless assisted by mechanical vibration, to fully respond to the magnetic action. It therefore occurs to me that such instruments are somewhat liable to error, especially when used for very weak currents, and when likely to be subjected to the slightest mechanical vibration.

Sir WILLIAM THOMSON, in reply, said: Professor Ayrton asked Sir William Thomson. whether I had any experience of platinoid being liable to oxidation. I cannot say that I have found any signs whatever indicating such effect. I do not say that there may not be specimens of platinoid that show it, nor do I say that platinoid may not develop that quality, since my experience of it has not been sufficiently long and varied. It is only about two and a half or three years since it was made for the first standard instrument. It is a kind of German silver with a very little tungsten added, though perhaps nearly all of the tungsten is expelled again. I have found it remarkably free from all ordinary signs of oxidation, and I have tried it very much in one very telling manner as regards oxidation—in the new depth-recorder of my deep-sea sounding machine. I use platinoid for the spiral spring, main air-vessel, and chief parts of the instrument, and I find remarkably little signs of the unsightly corroding action that was experienced with brass in the use of this instrument, which is let down to the bottom of the sea in

Sir William
Thomson. deep water, brought up, and left hanging between wind and
water for months together, always moist with sea water. No
change whatever in the strength of the spring, so far as any
tests that I have been able to apply could show, has yet been
found. With reference to the platinoid wire of this marine
voltmeter, I have only had about a year and a half's test, and it
has shown absolutely no sign of change, and, on the whole, I
should not expect that it would show anything of the kind even
in a million years; but that is not a statement that anyone has
a right to make dogmatically, and therefore I recede from the
assertion that this instrument a million years hence will be more
constant than the gravity instrument, because, as Professor Ayrton
has pointed out, platinoid is certainly liable to oxidation. I
formerly used platinum for such appliances, as being absolutely
free from liability to change through oxidation, but it is not
nearly so satisfactory in respect to its elasticity as platinoid.

I would just say that there is one fault in platinoid, as
hitherto supplied, and that is, that it is very varying in its
electrical qualities—different pieces of the same wire even are
different—so that you cannot trust to its efficiency for tempera-
ture variation of resistance being absolutely the same in two
adjacent parts of a wire of absolutely the same gauge through-
out its length. Mr. J. T. Bottomley has found that platinoid
varies exceedingly little in its resistance with change of tem-
perature; and, both on this account and on account of its high-
class elasticity, it is a material which deserves very careful
investigation. I suppose, being a new metal, that the mode of
working it is not altogether settled, and that the mixing in the
crucible is not perfect, so that when an ingot comes out it may be
slightly different in different parts; but that objection is perhaps
only temporary, and a few years hence we may find another alloy
better and perfectly uniform. It is worth while endeavouring to
attain perfection in the manufacture of so valuable a material.

I was asked about this rheostat (Fig. 6) on the table, but for
the same reason that I did not explain the electrostatic voltmeter,
so with this. It is an improved Wheatstone rheostat, with cylinders
and wire of platinoid, and may be turned backwards or forwards

suddenly and at a very rapid rate without the possibility of Sir William Thomson.
putting it wrong. It is an exceedingly convenient laboratory
apparatus; but it is not to be taken as an exact measurer of
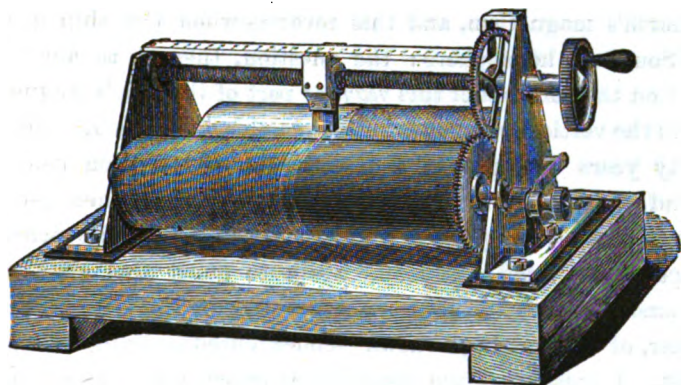
Fig. 6.—Improved Wheatstone Rheostat.

resistance, on account of platinoid wire not being the same in all
its parts. Owing to the freedom from oxidation of platinoid, the
platinoid cylinders and platinoid wire work better than the brass
cylinder and brass wire of the original Wheatstone rheostat.
The improvement on the Wheatstone rheostat consists in the
tightening-spring keeping the wire stretched and the fork guid-
ing it, so that without grooves on either cylinder the whole
works in an exceedingly convenient way.

As for my mho-ohm-drum alluded to by Mr. Swinburne, I wish
I could answer that question satisfactorily. I am still working hard
at it in my laboratory and in White's workshop, trying all I can
to bring it into practical shape, with good hope of an early
result, after a serious attempt thirty years ago, and con-
tinuous effort for the last nineteen months. I may possibly be
allowed the privilege of bringing one before the Society when I
succeed in finding something of the kind that I think worth
showing.

Mr. Granville's question suggests very interesting considera-
tions regarding magnetic induction in iron, which I first learned in
working in quite another subject—the Flinders bar for the correc-
tion of the mariner's compass, so that the compass, if corrected for
one latitude, may be also correct in any other; that a compass

Sir William
Thomson. corrected for the British Channel, for instance, could be also
correct at Port Elizabeth or the Cape of Good Hope.   A large part
of the magnetism of the ship is due to the vertical component of
the earth's magnetism, and this reverses when the ship goes to
the Southern hemisphere : the question, then, is to annul the
effect on the compass of this varying part of the ship's magnetism
due to the vertical component of the earth's magnetism.   Flinders,
eighty years ago, placed a vertical bar of soft iron before or
behind the compass for that purpose, but it did not then get into
general use.   Some persons who wished to improve the mariner's
compass have tried again and again to effect this annulment,
and among others Mr. James Napier—one of the sons of Robert
Napier, of engineering renown—endeavoured to bring about this
result.   I questioned him about his experience with the Flinders
bar, and said : " If that bar gets a kick from anyone's foot when
" the ship is heeling over, its magnetism will be reversed, and the
" compass will show a permanent change when the ship is upright
" again ; " and I might have added, " particularly if she is on an
" east or west course."   He answered, " That is just what we have
" found ; " and a *long thin* bar really is exceedingly subject to the
disturbances to which Mr. Granville has alluded.   If you take a
long bar of soft iron in such proportions as this pen—of length
more than twenty times its diameter (a common domestic poker
answers well)—and hold it vertically, or, better, parallel to the
terrestrial magnetic force (the line of " dip "), and place a pocket
compass near one end of it, you will see splendidly interesting
results.   First, you will generally find that the upper end acts
as with " blue magnetism " (which is the same as the mag-
netism of the north pole), and the lower end with " red."   If
you invert it very gently you will find that the end which
was upper still acts as with " blue magnetism " on the compass.
If you now give a very gentle tap with the finger to the in-
verted poker, in an instant the action on the compass is reversed
—the " blue magnetism " changes to " red."   I do not know
whether this result of tapping with the finger is generally
known, but I mention it as interesting to the meeting; it can-
not be observed with short thick bars.   In trying to bring the

Sir William Thomson.

Flinders bar into practical use I simply tried a bar of a certain diameter, and shorter and shorter lengths, and found whether by inverting it by the side of the compass and striking it with blows from a mallet I could make a difference in its magnetic effect. When the length of the bar was thirty times its diameter, then it was very sensitive to the blows of a mallet; but when its length was not more than eight or ten times the diameter, I found I could invert it in the neighbourhood of the compass and give it a blow without it showing any sensible retentiveness under the small force employed. Under the influence of a large magnetising force the effect would of course be magnified, but under the feeble force of the earth's magnetism a bar whose length is not more than eight or ten times its diameter shows no sensible effect of retentiveness—no effect sensible in comparison with the directive effect of the earth's magnetism on the compass. I simply adopted that principle—shortening the bar—for electric measuring instruments of the marine voltmeter class. The practical result is that until a current of two or three times the strength of the greatest current provided for is passed through the marine voltmeter we find no signs of retentiveness. If I work from zero up to 120 milli-amperes in this instrument, up and down, and with reversed currents, I find no signs of retained magnetism; but a current of two or three hundred milli-amperes leaves a large effect of residual magnetism, which, however, is easily shaken out by turning the little reversing key which forms part of the instrument. I have to thank you for the patience and kindness which you have shown during the time I have been addressing you.

The PRESIDENT: At the conclusion of his remarks Mr. Preece somewhat anticipated the duty which now devolves upon me; but, nevertheless, I think I should be excessively remiss if I did not ask you to join in passing a most cordial and hearty vote of thanks to Sir William Thomson for his kindness in being amongst us to-night, and for the very interesting explanation he has afforded us of his new measuring electrical instruments.

The President.

The motion was carried unanimously.

The meeting then adjourned.

The One Hundred and Eightieth Ordinary General Meeting of
the Society was held at the Institution of Civil Engineers,
25, Great George Street, Westminster, on Thursday, May
31st, 1888—Mr. EDWARD GRAVES, President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced, and this
being the last General Meeting before the recess, it was agreed,
upon the motion of the President, that, following the custom of
previous years, the candidates should be balloted for that evening.

The following transfers were announced as having been
approved by the Council, viz. :—
From the class of Associates to that of Members—
William John Hancock.
From the class of Students to that of Associates—
Walter Charles Garrard.

Donations to the Library were announced as having been
received since the last meeting from the Institution of Civil
Engineers; G. T. Carruthers, Esq. ; W. Geipel, Associate ; Robert
K. Gray, Member; C. L. Madsen, Foreign Member.

Having announced these donations, the SECRETARY added :
I have great pleasure in stating that the Postmaster-General
has been kind enough to present to the Society a very valu-
able assortment of obsolete telegraph apparatus, of great
historical interest, comprising thirty different instruments ;
and that Mr. Leonard Wray, jun., Associate, has sent a further
collection of botanical specimens of rubber-producing plants
from Perak, completing the series of which he kindly presented
the first portion in 1886.

A hearty vote of thanks was accorded to the donors.

The following paper was then read :—

THE INFLUENCE MACHINE, FROM 1788 TO 1888.

By Professor Silvanus P. Thompson, D.Sc., &c., Member.

Electrical influence, or the effect which an electrified body exerts upon a body brought into its neighbourhood, was discovered by John Canton, of Stroud, in 1753, and announced by him in a paper* read before the Royal Society on the 6th of December of that year. Four years later, Wilcke, then resident in Germany, investigated the same phenomenon, and in 1762 published† an account of an experiment in which a plate of metal held above the upper surface of a horizontal glass table was subjected to the influence of a charge of electricity imparted to a second metal plate placed beneath the under surface. This was followed' in 1776 by the very similar but much more renowned *electroforo perpetuo* of Volta‡—an instrument which brought the fact home to every experimenter that a conductor, if placed in the neighbourhood of an electrified body, and, while thus under influence, touched with the finger or other neutralising conductor, acquires an electric charge of opposite sign to that of the primitive charge. On the 10th May, 1787, the Rev. Abraham Bennet communicated to the Royal Society an account§ of a "doubler" of electricity—"a machine by which the least "conceivable quantity of positive or negative electricity may "be continually doubled, till it becomes perceptible by common "electrometers, or visible in sparks." One year later—on June 5th, 1788—a letter written by William Nicholson to Sir Joseph Banks, President, was read at the Royal Society, announcing his invention of an apparatus known at the time as a *revolving doubler*—"an instrument," to quote Nicholson's description, "which, by the turning of a winch, produces the "two states of electricity without friction or communication "with the earth." This instrument—the first of two invented by the same hand—was the earliest of rotating influence

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* Canton, *Phil. Trans.*, 1753, p. 350; 1754, p. 780.

† Wilcke, *Kön. Schwedische Akademie Abhandlungen*, xxiv., 213, 1762.

‡ Volta, *Journal de Physique*, viii., September, 1776.

§ Bennet, *Phil. Trans.*, 1787, p. 288.

machines, and may be regarded as the parent of the numerous forms that have in recent years become widely known.

It would not be right that the hundredth anniversary of so important an invention should be passed over in silence in the land that gave it birth. Accordingly the following account of the influence machine in its development from 1788 to 1888 has been prepared to mark the occurrence of the centenary. To gather up the scattered literature which has grown up around the invention is the least tribute which the electricians of to-day can offer to the memory of the departed inventor.

It will be convenient to follow the development of the machine under three periods—

> I. 1762 to 1787—The early investigations, and suggestions for doublers and multipliers of non-rotating type.
>
> II. 1787 to 1851—The revolving doubler of Nicholson, and the forms of apparatus that followed it.
>
> III. 1860 to 1888—The modern influence machines.

PERIOD I.

THE EARLY INVESTIGATIONS.

The discovery by John Canton of the main facts of electric influence was somewhat obscured by the language in which they were described. Canton's main idea—that every electrified body was surrounded by an electric atmosphere which affected or influenced other bodies that were brought near to it—was soon brought into sharp contrast with the more plausible ideas advanced by the mathematician Æpinus—that an electric charge could act at a distance according to some mathematical law. Wilcke, who repeated some of Canton's experiments, seems to have adopted Canton's way of regarding the subject. The contemporary views are probably accurately summed up in the following words taken from Priestley's "History and Present "State of Electricity" (1777):—

"The electric fluid, when there is a redundancy of it in any "body, repels the electric fluid in any other body, when they are "brought within the sphere of each other's influence, and drives "it into the remote parts of the body; or quite out of the body,

" if there be any outlet for that purpose. In other words, bodies
" immerged in electric atmospheres always become possessed of
" the electricity contrary to that of the body in whose atmosphere
" they are immerged."

Further experiments, really on the same point, though not
expressed in the same language, were made by various ex-
perimenters.   Franklin touched the subject very briefly, but
added little to our knowledge.   Prof. Æpinus, then in Berlin,
touched the subject from a somewhat different point of view.
Æpinus in particular explained all the things that he observed,
not by thinking of an atmosphere at all, but by introducing the
mathematical notion of action at a distance, and he gave a certain
mathematical theory to account for the facts that he observed.
In Italy Jacopo Beccari, working on almost exactly the same
problems, viewed them from a third standpoint, and discovered
that a charge of electricity on a body behaves quite differently
according to that which may be near it; that if you have a
charged body—say, a glass plate rubbed on one side—and you
bring something against the back of that plate, the charge on the
front of the plate behaves differently, as though it were a large
charge or a small charge, according to circumstances.   To this
peculiar action he gave the Latin name " *Vindex*."   I do not
know how we are to translate it.   The " vindication " of elec-
tricity would not convey any intelligible meaning, I suppose, to
our minds.   That which Canton was referring to as an electrical
atmosphere, that which Æpinus was referring to as an action at
a distance, is that which Beccari was referring to by talking
about electricity having a " vindicating " power, and it amounted
simply to this—that in the neighbourhood of a charged body
actions take place which are all explainable, which are all
consistent with themselves, provided you will understand that
the body which is brought into the neighbourhood of the charged
body is acted upon in such a way as to tend to drive out of it
electricity if the charged body has a positive charge, or to draw
electricity from outside into it if the charged body is negatively
charged.   In fact, the action which we now call *influence*, and
which for long was called *induction*, is nothing else than the

action which all these various experimenters referred to. And, for my own part, I think that John Canton's notion of atmospheres was at least as useful as any of the other notions; it at least suggested that the intervening medium took an essential part in the phenomenon.

Wilcke, in 1762, described to the Swedish Academy two *charging machines* (Ladungsmaschinen)—that is to say, two electric machines working on the principle of influence. One of these, alluded to above, had a mechanical arrangement of pulleys and counterpoise weights, enabling the experimenter to lower a metal-covered conductor into a position where it was under the influence of a second charged plate placed beneath a horizontal sheet of glass. The movable plate, after having been touched while under influence, was then raised up, and found to have acquired an opposite charge.

Volta's electrophorus*—first announced in a letter to Priestley, dated June 10th, 1775—was, as remarked above, little more than a convenient variation of the apparatus of Wilcke, but it attracted great attention and incited to further invention. Volta himself employed a double and reciprocal electrophorus, a description of which will be found in his collected works, and he also touched on the subject of multiplying condensers in a paper† of which a translation is to be found in the *Philosophical Transactions* of 1782. He explicitly states, however, that the idea of employing a condenser in connection with the electrophorus so as to accumulate and make evident the multiplied weak charges was due to Cavallo‡, who, after employing an air condenser as a mere collector, used a more complex arrangement, in which a metal plate standing on an insulating stem was moved to and fro on a lever, serving as the carrier of an electrophorus and giving up its charges to one plate of a condenser. This apparatus was known as Cavallo's multiplier.§

* Volta, *Scelta di Opuscoli di Milano*, viii., 127, reprinted in " Collezione delle " Opera " (edition of 1816), i., 118.
    † Volta, *Phil. Trans.*, 1782, 237, and Appendix, p. vii.
    ‡ Cavallo, *Phil. Trans.*, 1788, 1.
    § Cavallo: see his " Complete Treatise on Electricity " (4th edition, 1795), vol. iii., p. 98; or see his " Elements of Natural Philosophy " (1803), iii., 425; and also *Nicholson's Journal*, i., 394, 1797.

Lichtenberg,* in 1780, describes a species of double electrophorus, in which we meet for the first time with the reciprocal principle now adopted in all modern influence machines. Hitherto the electrophorus, whether in the older form of Wilcke or the later form of Volta, had consisted of one charged surface as inductor, and one movable plate as carrier of the induced charge. Lichtenberg, though he still uses one carrier, proposes to employ both positive and negative inductor charges. He took a stout plank of wood, twice as long as wide, and after nailing on a wooden rim he cast into it a mixture of resin, pitch, and turpentine. A spot of the surface near one end having been excited (negatively) by friction, the movable carrier plate (with insulated handle) was laid upon this, and touched. It was then lifted up and made to impart its charge to a spot near the other end of the cake. Then the carrier plate was laid on this spot, touched, and received a charge of opposite sign, which was in turn imparted to the originally charged spot, so increasing its charge. A very similar device was made by Professor Klinkoch,† of Prag, about the same time, but Klinkoch used two entirely separate cakes of resin instead of two spots on the surface of the same cake.

We now come to the name of the Rev. Abraham Bennet, curate of Wirksworth, the inventor of the gold-leaf electroscope, and of the *doubler*, which was the immediate forerunner of the influence machine. Bennet's doubler‡ consisted of three metal plates with insulating handles, the mode of use being as follows :—

Let the three plates be called A, B, and C. A is varnished on its upper surface, B on both sides (the insulated handle being attached at the side), C on the under side. The plate A having received a small initial charge, the plate B is laid upon it and touched with a finger of the hand which grasps the handle. B is

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* Lichtenberg, "De Novo Methodo" (1778); see also *Journal de Physique*, xv., 17, 1780.

† See Ingenhousz in *Phil. Trans.*, 1778, p. 1029.

‡ Bennet, *Phil. Trans.*, 1787, p. 288, "An Account of a Doubler of Electricity, or a Machine by which the least conceivable Quantity of Positive or Negative Electricity may be continually doubled, till it becomes perceptible by common Electrometers, or visible in Sparks."

then lifted up, and the third plate, C, is placed on B, and also touched by stretching out a finger of the other hand.   C is then applied to touch the under side of A, whilst B is brought down over A and again touched while under the influence of the joint charges of A and C.   It thereby receives an increased induced charge, is again raised, and C is again placed on it and touched, and so on in a regular succession of operations.   As a result of each repetition of the set of operations, the original charge on A is increased.   To put the matter in the familiar language of the text-books, the original small charge on A is put out at interest, giving rise to a somewhat smaller charge on B.   This charge on B being again put out at interest produces in turn a charge on C.   The interest which has thus accrued is added to the original principal, which thereby becomes nearly, but not quite, doubled, and each repetition of the operations again nearly doubles the charge on A.   You begin with a certain charge as your principal, you put that out to interest by the operation of touching the conductor whilst under influence, and every time you choose to perform this operation you carry away a fresh quantity of interest. But the curious point about this is that when you have a surplus —when you have a positive charge as your principal—the interest is a negative quantity ; and when you have a negative charge as your principal the interest is positive—which is contrary to the ordinary way of the world !   Hence the reason for the double operation.   If A is positive, the charge on B obtained by influence from it will be negative, and that obtained on C by influence from B will be positive.   This somewhat homely notion of deriving interest from principal is very convenient in explaining the double action of the *reciprocal* electrophorus.   In that instrument you have at one part a positive charge or surplus, at another part a negative charge or deficit. The interest on the surplus is a deficit, and is carried across and added to the original deficit; the interest on the deficit is a surplus which is carried across and added to the original surplus.   Hence both the original charges, positive and negative, grow by reciprocal accumulation.   In Bennet's original doubler, as there was but one original charge, it had to be increased by adding to it the interest on the interest, or by accumulating charges induced from an induced charge.

This ingenious device at once excited great attention, and Erasmus Darwin,* author of the "Botanic Garden," and the grandfather of the great—or, may I say, of the greater—Darwin, wrote enthusiastically of the doubler of electricity as "the greatest "discovery made in that science since the coated jar and the "eduction of lightning from skies." Darwin himself had been fired in 1778 by the accounts of Volta's electrophorus to devise an arrangement for mechanically performing the manipulations of that instrument. His name is mentioned in all the older treatises and cyclopædias as having made some such instrument as a mechanical doubler; but the references are extremely vague. Bennet, writing in his "New Experiments" (1789)† about the doubler, and of the mechanical performance of its consecutive operations, says : " Dr. Darwin, at the desire of " Lord G. A. Cavendish, made the first attempt with two plates " moving between two others by a lever, so as to bring them " exactly to the same position in each operation. This contrivance " he soon improved by another instrument, in which the plates " stood vertically and moved by rack-work in a direction exactly " parallel to each other. . . . This instrument was sent to " Mr. Partington." A clearer light is thrown upon the nature of this instrument by the following passage from an article by Nicholson,‡ dated 1797 :—" In the month of December, 1787, " Mr. Partington lent me an instrument contrived by Dr. Darwin, " and consisting of four metallic plates, two of which were mov- " able by wheelwork into positions which required them to be " touched with the hand in order to produce these effects." All inquiries as to the subsequent history of this instrument have proved fruitless, but a record has been found of an earlier form, possibly that referred to by Bennet. Mr. Horace Darwin, who has interested himself in the inquiry, has procured from an old manuscript note-book of Dr. Darwin, now in the possession of Mr. Reginald Darwin, of Fern, Buxton, Derbyshire, a page of notes and sketches, from which Fig. 1 is taken. It is not certain that

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* Darwin : see remarks at end of his "Zoonomia."

† Bennet, "New Experiments in Electricity," 8vo, 141 pp. (Derby, 1789).

‡ See *Nicholson's Journal*, i., p. 397, 1797.

this sketch refers to precisely the same instrument as that referred to by Bennet.   In the centre was a double glass plate with a metal sheet in between, to which an initial charge was given.
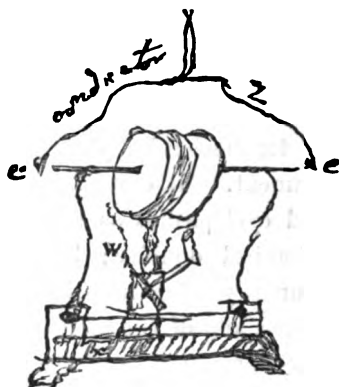
FIG. 1.—Facsimile of Sketch by Erasmus Darwin, Esq., 1778.

Right and left of this were two movable brass plates which, being mounted on cranked levers, could be made to approach or recede from the central plate by turning the winch, which acted by a sort of cam upon the tails of the levers.   At x was a spring which pressed upwards.   " When the brass planes recede," says the note-book, " they are supply'd again from the fork w, which is " express'd in dots and joins with the pedestal or may be " insulated."

Darwin's instrument was almost immediately followed by the rotating machines which are considered in our second section, but instruments of the non-rotating types still made their appearance from time to time.   Amongst these were Wilson's " condenser, doubler, and multiplier,"[*] a modification of Cavallo's multiplier, and several lever arrangements due to Bohnenberger.[†] Another modification of Bennet's doubler is due to Péclet,[‡] and is known as the *three-plate condenser*.

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[*] Wilson, *Nicholson's Journal*, ix., 19.

[†] Bohnenberger: see below.

[‡] Péclet, *Ann. Chim. Phys.* [2], lxviii., 442, 1838; and [3] ii., 100, 1841 ; see also Daguin's "Traité de Physique," vol. ii., p. 393; and *Annals of Electricity*, vol. iii., p. 462, 1838-9.

PERIOD II.

REVOLVING DOUBLERS.

The second period opens with the epoch-making paper of William Nicholson, which was read to the Royal Society on June 5th, 1788, under the following title :—" A Description of " an Instrument which, by the Turning of a .Winch, produces " the Two States of Electricity without Friction or Communica- " tion with the Earth."* Writing about this instrument nine years afterwards, Nicholson states that the idea of a *revolving doubler* was suggested to him by conversation with Rev. A. Bennet, whose ideas, however, were directed toward a very different type of machine. Bennet, in his " New Experiments," published one year later than Nicholson's paper, gives Nicholson entire credit for the revolving doubler. Its construction was, briefly, as follows :—

Two fixed plates of brass (A and C, Fig. 2), each two inches in
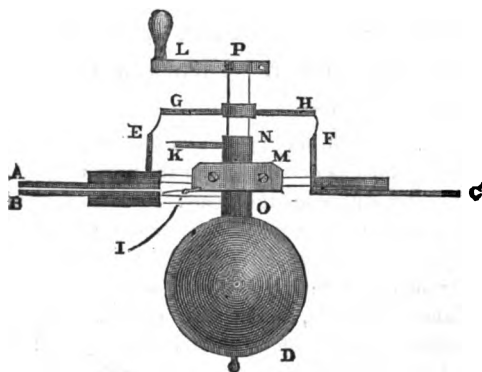
FIG. 2.—Nicholson's Revolving Doubler (plan view).

diameter, are separately supported on insulating arms in the same plane, so that a third plate (B), which revolves, may pass very near them without touching. A brass ball (D), two inches in diameter, is fixed on the end of the axis that carries the plate B, and is loaded within at one side, so as to act as a counterpoise to the revolving plate B, so as to keep it at rest in any position. The axis P N is made of varnished glass, and so are the axes that join

* Nicholson, *Phil. Trans.*, 1788, p. 408; see also *Gren's Journal*, ii., 61, 1790.

the three plates with the brass axis N O. The axis N O passes
through the brass piece M, which stands on an insulating pillar
of glass, and supports the plates A and C. At one extremity of
this axis is the ball D, and the other is connected with a rod of
glass (N P), upon which the handle L is fixed, and also the piece
G H, which is separately insulated. The pins E, F rise out of the
back of the fixed plates A and C, at unequal distances from the
axis. The piece K is parallel to G H, and both of them are
furnished at their ends with small pieces of flexible wire that
they may touch the pins E,F in certain points of their revolution.
From the brass piece M there stands out a pin (I), to touch against
a small flexible wire or spring which projects sideways from the
rotating plate B when it comes opposite A. The wires are so
adjusted by bending that at the moment when B is opposite A,
B communicates with the ball D, and A communicates with C
through G H; and, half a revolution later, when B comes opposite
C, C communicates with the ball D through the contact of K
with F. In all other positions A, B, C, and D are completely
disconnected from each other. Nicholson thus described the
operation of the machine:—

" When the plates A and B are opposite each other, the two fixed plates
" A and C may be considered as one mass, and the revolving plate B, together
" with the ball D, will constitute another mass. All the experiments yet made
" concur to prove that these two masses will not possess the same electric
" state. . . . The redundant electricities in the masses under consideration
" will be unequally distributed: the plate A will have about ninety-nine
" parts, and the plate C one; and, for the same reason, the revolving plate B
" will have ninety-nine parts of the opposite electricity, and the ball D one.
" The rotation, by destroying the contacts, preserves this unequal distribution,
" and carries B from A to C at the same time that the tail K connects the
" ball with the plate C. In this situation, the electricity in B acts upon that
" in C, and produces the contrary state, by virtue of the communication
" between C and the ball; which last must therefore acquire an electricity of
" the same kind with that of the revolving plate. But the rotation again
" destroys the contact, and restores B to its first situation opposite A. Here,
" if we attend to the effect of the whole revolution, we shall find that the
" electric states of the respective masses have been greatly increased; for the
" ninety-nine parts in A and B remain, and the one part of electricity in C has
" been increased so as nearly to compensate ninety-nine parts of the opposite
" electricity in the revolving plate B, while the communication produced an
" opposite mutation in the electricity of the ball. A second rotation will, of

"course, produce a proportional augmentation of these increased quantities;
" and a continuance of turning will soon bring the intensities to their maxi-
"mum, which is limited by an explosion between the plates."—*Phil. Trans.*,
1788, p. 405.

The limitation of the powers of the instrument was further
criticised by Professor Robison,[*] who remarked that the plates
approached one another edge-on—a circumstance unfavourable to
retention of charge. He suggested that the dissipation of the
charge might be lessened by tipping the wires with little balls.
He also made the retrograde suggestion of substituting for the
rotatory movement an alternate motion like that of a pump-
handle.

It may be remarked in passing that in Nicholson's instru-
ment the sense of the rotation is immaterial: it may be turned
either right-handedly or left-handedly.

In 1797 Nicholson[†] described another apparatus—the *spinning
condenser*—the nature of which was as follows :—

The spinning condenser consisted of a little metallic vase, to
be spun, like a teetotum, between the finger and thumb ; its axis
of rotation was an upright slender steel shaft supported upon an
adjustable footstep pivot. Below the vase was fixed a circular
glass disc $1\frac{1}{2}$ inches in diameter and $\frac{2}{10}$ inch thick. A similar
glass plate, having a central hole, was fixed, beneath the former,
to the top of the stand, being separated by a small distance from
the revolving disc. Upon each of the adjacent faces were fixed
two segments of tinfoil, which nearly covered the faces, but were
separated by sufficiently wide gaps for proper insulation. The
edges of the fixed plate were drilled to receive two metallic
hooks, one of which was insulated, but the other communicated
with the segment nearest it. The edges of the upper plate also
were drilled to receive two flexible metal wire brushes or " tails,"
one communicating with each segment on the upper plate.
These tails were so bent as to strike, in revolving, against the
hooks in the lower plate. When it was desired to examine the
charge of any feebly electrified body, the body was put into

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[*] See Robison's " Mechanical Philosophy," iv., 157.
[†] *Nicholson's Journal*, i., 394-99, 1797, " On the Multiplier of Electricity."

communication with the insulated hook, and the apparatus was set spinning, when presently there was collected on the other hook a sufficient charge to affect an electroscope. The whole apparatus was only five inches high.*
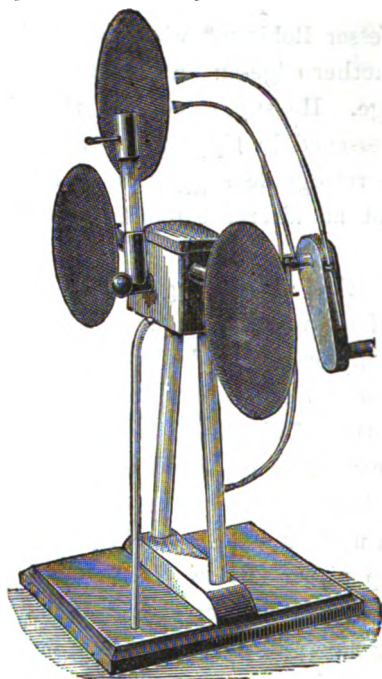
FIG. 3.—Model of Nicholson's Doubler recently constructed by Mr. Wimshurst.

The doublers shown to the Society on the present occasion are three in number. Here is one of Cavallo's pattern with a mere lever movement; and here is a Nicholson's revolving doubler exactly resembling Fig. 2. These are both old instruments, and are kindly lent from the collection of the London Institution, in whose possession they have been for many years. The third is a modern model of the Nicholson doubler, of somewhat larger size (Fig. 3), kindly lent by Mr. Wimshurst, who himself constructed it. It is self-exciting, and with a very few turns charges an electroscope. Nicholson states† that he made but

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* This spinning condenser, which, according to Nicholson, is a close approach to Bennet's unpublished suggestion of 1787, was criticised by Cavallo in a letter addressed to *Nicholson's Journal*, vol. i., p. 394.

† *Nicholson's Journal*, vol. i., p. 17.

one instrument of this type, and that it was given to Professor
Martinus van Marum, of Haarlem.   Possibly it may still exist in
the historical collection of the Teylerian Museum in that city.
The drawings given in various Continental works* purporting to
depict Nicholson's revolving doubler differ in several respects
from the original design, and have earth-contacts.   The source
from which these drawings have been derived is unknown to me.
Volta† in due time became acquainted with Nicholson's doubler,
which he esteemed and made use of in his own researches.   He
refers to it as " *il duplicatore a molinello di Nicholson.*"

In a very few years other forms had been devised.   There was
a Mr. John Read‡ who read a paper to the Royal Society in 1794,
in which he described a doubler very like Nicholson's, the only
difference being that the two fixed discs stood on upright stems
and were made of glass, from which circumstance it was called
the " spectacle doubler."   With this apparatus he made some
truly curious investigations.   One may judge of the strange sort
of thing which Read brought out with his doubler from the
following quotation :— " Knightsbridge Charity School fills up
" a piece of ground between the north end of the chapel and
" Hyde Park wall, and the main sewer at that neighbourhood
" runs at no great depth under it.   The number of children
" educated in this school is thought by some to be too great for
" the size of the school.   On this account it becomes infected
" with a very disagreeable stench, especially when the doors and
" windows are shut up.   I have sometimes found the noxious
" effluvium so very strong in this school that I have hastened
" out to breathe a purer air.   I have often examined the electrical
" state of the air in this school with a doubler, and have always
" found it strongly negative;" and he made a number of un-
savoury experiments in order to discover whether there was any

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    *See, for example, Wiedemann's "Lehre von der Elektricität," i., 151;
Mascart's "Traité d'Électricité Statique," ii., 272; Wallentin, "Die Genera-
"toren hochgespannter Elektricität," 71.

    † See "Collezione d. Op. di Volta," ii. [2], 47.

    ‡ *Phil. Trans.*, 1794; see also his work, "A Summary View of the Spon-
"taneous Electricity of the Earth and Atmosphere" (Lond., 1793), p. 25.; also
an abstract in *Ann. Chim. Phys.*, xxiv., 327 1797.

real connection between the electrification which he could get out of a doubler and the atmosphere in which that doubler was worked.

Next we come to a curious book, published in 1798 by the German pastor Bohnenberger,[*] which book is absolutely full of influence machines. Some of these were mere doublers consisting of three plates, on Bennet's original plan, but with mechanical
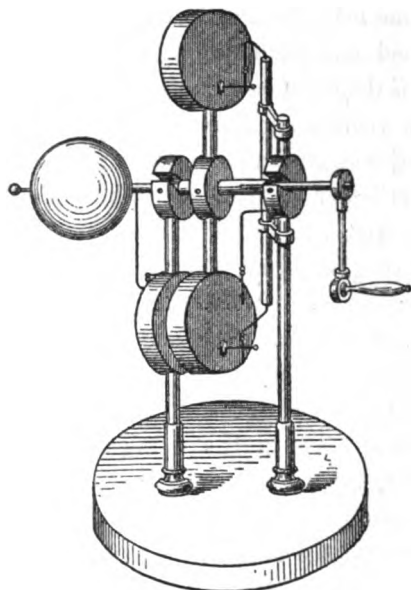
FIG. 4.—Bohnenberger's Doubler.

devices for superposing them and bringing them into contact; some had the moving parts fixed on levers, like Cavallo's multiplier; others were rotating apparatuses after Nicholson's plan, one of them (Fig. 4) very closely resembling Nicholson's revolving doubler, but constructed very simply with pieces of wood, cork, and glass rod, such as any amateur might make up.

In 1804 Desormes and Hachette[†] gave an account of Nichol-

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[*] Bohnenberger, "Beschreibung unterschiedlicher Elektrizitätsver-"doppler" (Tübingen, 1798). See in particular his new form of doubler described on page 17, and depicted in Plate II., figs. 3 and 4. See also article "Duplicator" in Gehler's "Handwörterbuch II." (1826), written by Pfaff, in which the apparatus of Bennet and of Read is described.

[†] Desormes and Hachette, *Annales de Chimie et de Physique*, xlix., 45, 1804, "Du Doubleur d'Électricité;" see also *Bull. de la Soc. Philomathique*, No. 83.

son's and other doublers, and described a form of their own devising, constructed for them by Dumouliez, having earth-contacts to touch the revolving disc while under the influence of the stationary disc. The apparatus had discs about 3 inches in diameter, and was about 15 inches in breadth and length.

The late Sir Francis Ronalds* published in 1823 a form of doubler in which the moving plate was attached to the bob of a pendulum, which moved it to and fro in front of the two fixed plates; the fixed and movable plates being all 4 inches in diameter. Save in mechanical working, its action was precisely the same as that of Nicholson's doubler. Ronalds proposed to use it to keep his telegraph line constantly charged, and says that he found the instrument convenient not only for this, but for all experiments of the kind which require a constant flow of small quantities of electricity. He says further that it has been proposed to adopt large doublers instead of the common (frictional) machines for exciting electricity in large quantities, "with "a view to saving labour;" but he doubted the practicability of the suggestion, owing to the tendency of the apparatus when highly charged to discharge itself by sparking across. He added that Read had hinted at a possible method of causing the rotating plate to recede from the fixed plates in proportion as the charges advance, but that he had died without making his plan public.

I remarked above that Ronalds's apparatus was electrically identical with Nicholson's. The two fixed discs were to be electrified with charges of the same sign. The moving disc was first to be touched under the influence of one of the fixed discs while a momentary contact was established between the two fixed discs; then it was to be moved across, and the second fixed disc was to be touched by a neutralising brush while under the influence of the moving disc; then the moving disc returned to its first position, and the action was repeated.

Nicholson had died (May 21st, 1815), and the interest in

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* Ronalds, "Descriptions of an Electrical Telegraph and of some other "Electrical Apparatus" (London, 1883), p. 53, "Description of a Pendulum "Doubler;" see also *Edin. Phil. Journal*, ix., 323, 1823.

statical electricity had waned before the newer discoveries of the voltaic battery and of the magnetic effects of electric currents. Ronalds's apparatus was the last of those which are strictly to be called doublers, for though in all those that succeeded we find a moving part and two fixed parts, from this time forward the *reciprocal* principle first introduced by Lichtenberg (*supra*, p. 573) is adopted. The two fixed parts henceforth have charges of opposite sign, and the charge induced on the moving carrier by the one is carried over and imparted to the other, so that they reciprocally serve to augment each other's charges.

This leads me to speak of the nomenclature which I have found convenient. As in the ordinary pattern of dynamo you have a rotating part between two fixed poles, so in the ordinary influence machine you have a rotating part between two fixed plates which I ˜propose to call, and have for some time called, the "field-plates." Some people call them the ".armatures," some people call them the "inductors;" there are various names for these two fixed plates, but "field-plate" seems to me the name that is most comfortable to employ. As in the dynamo machine we call that part which provides the magnetic field the "field-magnet," so in the influence machine I call by the name of "field-plate" that part which provides the electric field.

The first revolving apparatus in which the process of reciprocal influence was utilised was that of Belli* in 1831. After mentioning briefly the doublers of Nicholson's pattern, he describes one of his own (Fig. 5) which has this peculiar arrangement of two fixed field-plates, one of which has become positive at the same time that another one becomes negative. In this case the field-plates are made of metal folded round on themselves, one at each side, and the two rotating parts are metal discs of about 1½ inches diameter, carried at the ends of a glass stem. As these go round they are touched while under influence. You will see here at the

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* G. Belli: "Corso Elementare di Fisica Sperimentale " (Milan, 1831-38), vol. iii., p. 395, describes the *Doubler;* p. 486 describes the *Macchina ad Attuazione.* See also Belli's Memoir in *Ann. di Sci. d. R. Lomb.-Venet.,* vol. i., 111, 1831, " Di una Nuova Maniera di Macchina Elettrica."

back, standing also on an insulating stem, though it need not be insulated, a conductor. This has spring ends and extends down to the lower side of the field-plate on the one hand and to the upper part of the field-plate on the other hand. A little way
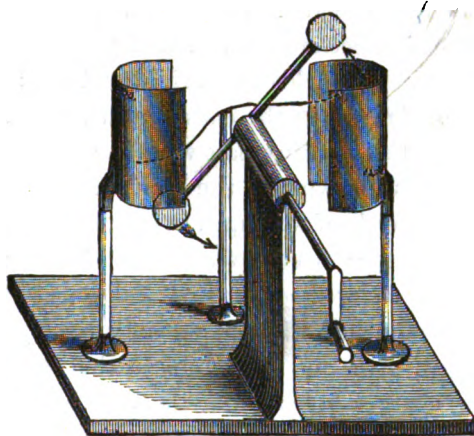
Fɪɢ. 5.—Belli's Doubler.

down from the top of one field-plate and a little way up from the bottom of the other plate there are a couple of small spiral springs soldered on inside, so that they touch these rotating carriers as they go round. Suppose, then, we start with a small charge, and one field-plate be slightly positive (the rotation is supposed to be left-handed in Belli's drawing), this little disc will go between the folds of the field-plate and be touched by the diagonal conductor just the last thing before it comes out. It is then negatively charged while it passes round underneath ; as it goes up on this side the first thing it does is to touch the internal spring and give up its negative charge to the plate on this side. Then as it goes up the last thing that happens before it comes away from the negative field-plate is that it is touched by the contact spring on the other end of the diagonal conductor, and goes off positively charged, giving up its positive charge to the little spring on the inside of the other field-plates. Now this regenerating action is typical of what happens in all the modern influence machines. Some time ago—in fact, long before I knew of this particular apparatus of Belli's—for the purpose of illustrating lectures on this

subject, I devised* an almost identical piece of apparatus (which I also have here), the only essential addition being the pair of discharging combs to receive any superfluity of charge remaining over. Curiously enough, almost an identical thing, though different in form, is the little replenisher which we have in Sir William Thomson's electrometers. All the essential features are present. There are two field-plates, right and left; there are two insulated pieces of metal on the ends of arms, which go round. I would like to call this rotating part with the two sectors on it an "armature." In the dynamo the part that rotates with the various sections or coils of wire upon it is the analogue of the part which in the influence machine rotates and carries around the metallic sectors or charged conductors. Each one of these metallic conductors being touched, while under influence, by a little brush, goes off charged, gives up its charge to the other side, and so this machine charges itself up. It will be seen that this apparatus differs from the doublers that preceded it in one important respect, namely, that its action depended on the sense of the rotation. If the direction of the motion were reversed, the apparatus, even if highly charged to begin with, discharged itself. In this respect it agrees with the modern influence machines.

Beside this very simple doubler, Belli devised another machine having a rotating plate of glass† with metal sectors affixed

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* I am not the only person who has stumbled on Belli's form of doubler. See Elster and Geitel's apparatus depicted in *Wiedemann's Annalen,* xxv., 493, 1885, which is almost a fac-simile of Fig. 5.

† This is the first occurrence of the use of glass plates to carry the moving conductors. It may be pointed out that the glass plates in influence machines exercise a much more important function than that of mechanically supporting the metallic conductors: they prevent discharge by sparking between the inducing and the influenced parts. That *two* thicknesses of glass are requisite to the good working of an influence machine was first pointed out to me by Mr. Wimshurst. If there is only one thickness of glass, the effect of the charged conductor attached to it is neutralised by charges which accumulate on the other face, against which, if unprotected by a second sheet, the oppositely charged neighbouring conductor will emit a discharge. Compare the researches of Riess (quoted briefly below) on effect in Holtz machine of cutting away the back plate. In Belli's machine, though the sectors were exposed to induction both above and below, the glass plate was below them only.

to it. To this machine he gave the name of "macchina ad "attuazione." I thought this must be a misprint for *attrazione*— the *attraction* machine; but no, it is *attuazione* in two places, and as the Italian dictionaries do not give that word I conclude it means an "actuation machine"—a machine which works, not by friction, but by *actuating*, or by being *actuated*, or something of that kind; that is the nearest translation I can get for it. In this apparatus the rotating part is a glass disc, having four metallic sectors upon its face, mounted on a vertical spindle by means of which it can be set into rapid rotation by pulleys and a driving band. Enclosing this disc was a rectangular metal box, divided into two halves which were insulated from one another. These half-boxes served as field-plates, and through two holes in their upper surface there passed in the two neutralising rods to touch the metal sectors or carriers as they rotated. The two half-boxes were furnished with projecting knobs above to serve as dischargers.

From the time of Belli there is a great gap; very little was done. There was a certain Mr. Goodman,* of Birmingham, who made up a large machine which is supposed to work on the influence principle, though I am not at all sure that it does. Then Svanberg† in 1846 devised another arrangement of a double electrophorus, where a disc had to be carried over the top from one electrophorus to the other. A similar, but more complicated, reciprocal electrophorus was described in the same year by Munck af Rosenschöld ;‡ and in 1851 an elaborate theory of the reciprocal condenser was published by Billet.§ Then there comes a blank, and the science of the construction of influence machines absolutely died out, to be revived again by Varley.

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* Goodman, *Annals of Electricity*, vi., 97, 1841. This machine is described as being "for polarising frictional electricity." It resembles Carré's machine, but had numerous revolving plates on one long shaft.

† Svanberg, *Institut.*, xv., 683, 1847; also *Brit. Assoc. Rep.*, 1846, p. 31, "On "a New Multiplying Condenser." See Daguin's "Physique," ii., 393.

‡ Munck af Rosenschöld, *Ofvers. af Vetensk. Akad. Förh.*, ii., 298, 1846.

§ Billet, *Archiv.*, xx. (Bulletin), 53, 1852; see also *Dijon Acad.* [2], i., 66, 1851.

<div align="center"><i>PERIOD III.</i></div>

<div align="center">

THE MODERN INFLUENCE MACHINE.

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In 1860 Cromwell Fleetwood Varley* described an influence machine which had in it almost everything that is found in the modern ones, and, indeed, it is a machine of a very advanced type.
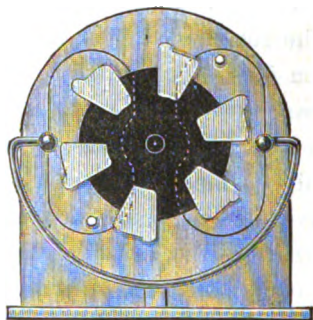
FIG. 6.—Varley's Influence Machine.

Speaking of his method of using charges of static electricity in cable-testing, Varley says:—" I obtain these charges by a new " method, which can also be used for obtaining electricity gene- " rally for any other purpose.   I place an insulated plate or con- " ductor near or between two other suitable conductors; these " being charged statically, induce in the former the contrary state, " the inner plate being momentarily connected to earth to give " or to take positive electricity.   This plate is then moved and " placed between two other plates, and then allowed to touch " them, by which it gives them its required charge, then moved " out of contact, but while between them it is made to touch " earth, by which it acquires through induction a charge of oppo- " site kind to the first one ; it is then taken back to the first pair, " to which it gives up its charge, then moved out of contact, but " while between them it touches earth, which renews the first " charge, but a little stronger; thus by continuing the process the " charge rapidly augments to the required amount.   Figs. 16, 17, " and 18 [of the specification] show this principle developed into a " machine for producing statical electricity without friction or

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" chemical action, mechanical force causing induction to act
" alternately positive and negative."

In Varley's apparatus (Fig. 6) you have, as before, two field-
plates; they were pieces of tinfoil stuck against glass—a large
sheet of glass. They are those two oval patches in the figure. In
front of them (or, rather, between them, for there are two sets, one
in front and one behind, connected together) rotated an armature,
a disk of ebonite or glass, having carriers of metal or wood upon it,
which, as they rotated, were touched while opposite to one field-
plate, and then went round to give up the charge so acquired to
the other field-plate. Those two knobs which are joined together
by a connecting arc are the parts which correspond to the
neutralising brushes which touch the carriers as they go round.
Knobs were used in some of Varley's actual machines, though he
himself suggests that for certain purposes it would be better to
use spring contacts. The knobs which received the charges from
the carriers, and so regenerated the charges on the field-plates,
may be seen in the figure on the upper part of the right-hand
field-plate and on the lower part of the left-hand field-plate. The
rotations are in this figure supposed to be right-handed. From
the field-plates, right and left, there projected up (not shown in
the drawing) metal rods terminated with metal balls to serve
as dischargers. In fact, at this date the notion of a separate
discharging circuit had not been developed. Both Belli and
Varley contemplated that the field-plate when sufficiently charged
should give up its charge. Varley, who apparently did not know
of the earlier instruments, since he describes the induction method
of obtaining charges as new, designed the more complex form
(Fig. 7) having six rotating discs. With this apparatus he got
sparks six inches long, the initial source of electrification being a
single Daniell's cell. The double field-plates or "cheeks," marked
e, are mounted on glass pillars (*d*). *k, k* are earth-plates. The
sectors or carriers (*c*) are shaped with a projecting lump to touch
the internal contact springs as they go round.

Varley's apparatus of 1860 was followed by a host of beautiful
devices. In Germany, Holtz and Toepler; in France, Carré,
Piche, and Bertsch; in Great Britain, Sir W. Thomson, indepen-

dently, and almost simultaneously, devised new forms of machine.

Toepler's* apparatus of 1865 (Figs. 8 and 9) consisted of two discs rotating in the same direction upon the same shaft.
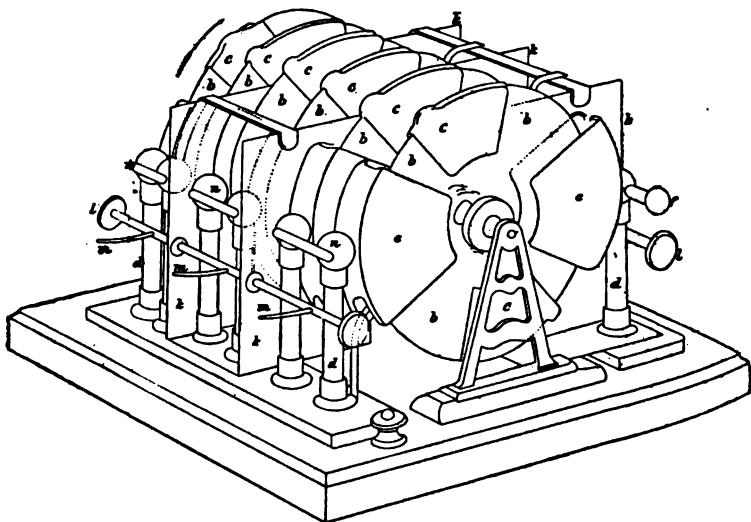
FIG. 7.—Varley's Six-plate Machine.

Each disc was provided with two strips or carriers of metal foil, each extending over nearly a semicircle. Behind each disc was a single field-plate, that behind one disc being positive, that behind
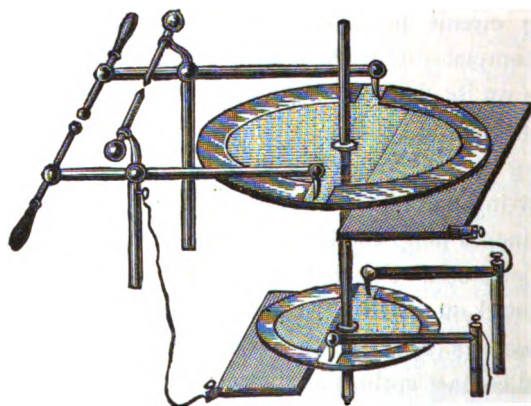
FIG. 8.—Toepler's Machine (1865), with "Regenerator" below.

* Toepler : see *Riga'scher Zeitung*, Jan. 7, 1865 ; also *Pogg. Ann.*, cxxv., 469, ; and *Ann. Chim. Phys.* [4], viii., 313, 1865.

the other disc negative. The carriers which were touched under
the influence of the positive field-plate passed on and gave up a
portion of their negative charge to increase that of the negative
field-plate; whilst in like manner the carriers which were touched
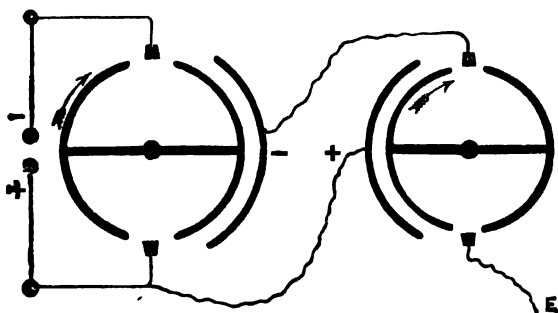
Fig. 9.—Diagram of Toepler's Machine (1865).

while under the influence of the negative field-plate send part of
their charges to augment that of the positive field-plate.   In this
apparatus one of the discharging rods communicated with one of
the field-plates, but the other communicated with the neutralising
brush opposite the other field-plate, not with the field-plate itself.
Consequently, one of the field-plates would always remain charged
when a spark was taken at the discharging terminals.   Toepler
himself considered the lower revolving apparatus as a mere con-
trivance to keep the field-plate of the upper part charged, and
he termed the lower part a " regenerator."   A year later,* having
meantime had the opportunity of comparing his machine with
that of Holtz, he discusses the question whether the rotating
plates are better with or without metal carriers, and decides in
favour of those with carriers, on the grounds that they more
readily excite themselves, and that they are less liable to be
affected by atmospheric conditions.   They have a definite spark-
length, but the currents yielded by them are discontinuous.   In
the same paper he describes a modification of his machine: the
two rotating armatures are now of equal size, upon a horizontal
shaft, though at some distance apart; between, but close to their
respective rotating plates, and at the same side of the shaft, stand
two fixed field-plates of opposite sign, each of them being

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* *Pogg. Ann.*, cxxvii., 177, 1866.

regenerated by supplies taken from the charges induced in the armature beyond the other field-plate. There is a separate neutralising circuit, consisting of a conductor joining together two combs which stand outside the two armatures exactly opposite the two field-plates. The two discharging knobs are attached directly to the two field-plates. The arrangement differs from the earlier one in being symmetrical. Again a year passes over, and further modifications are introduced * in which the principle is
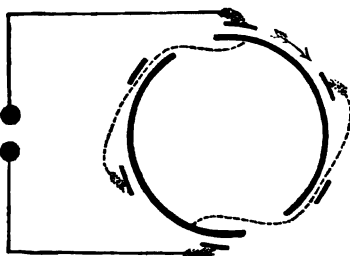
Fɪɢ. 10.—Diagram of Toepler's Machine (1867).

now found of placing both the field-plates behind a single armature; but the charges on these are still fed or regenerated by bringing round from the field-plates cross conductors terminating in brushes which touch the carriers as they come opposite the other field-plate of the machine. Some years later Professor Toepler constructed some larger multiple-plate machines,† one of which was shown to the writer at Dresden in 1879. One of these was sent to the late Mr. Spottiswoode. These machines were of the newer type,‡ in which the metallic sectors or carriers of metallic foil on the rotating armature were furnished with a raised button or knob to ensure metallic contact, when passing the brushes, without scraping the surfaces of the foil. The discharging knobs in this form of machine were inserted in the neutralising circuit;

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* *Pogg. Ann.*, cxxx., 518, 1867.

† One of these, with twelve rotating plates, is depicted in Wiedemann's " Die Lehre von der Elektricität," vol. ii., 227, fig. 70. Another, with thirty rotating plates, was shown at the Vienna Exhibition in 1883.

‡ *Berl. Monatsberichte*, Dec., 1879, p. 950; also *Elektrotechnische Zeitschrift*, i., p. 56, 1880; or *Wied. Beiblätter*, iv., 398. Also described in Wiedemann's " Die " Lehre," &c., vol. ii., 224, figs. 68 and 69.

they had to be put into contact in order that the machine might charge itself up.

The researches of Holtz, independently begun, were even more fruitful.   In 1864 he had already constructed (and had shown to
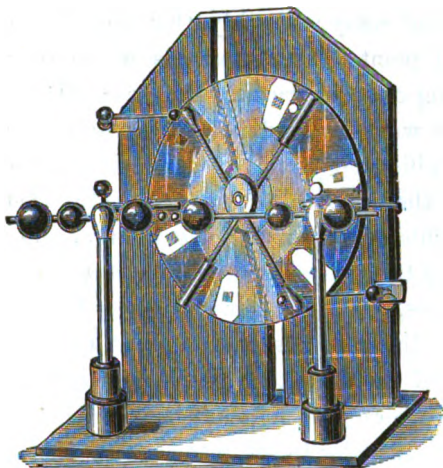
FIG. 11.—Toepler's Machine (1880).

Professor Paalzow) a machine having one fixed and one rotating disc.   His first publication was, however, in April, 1865.*   The

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* The following is believed to be a complete list of Holtz's published contributions on influence machines :—

1. *Berl. Monatsberichte*, April, 1865, '' Ueber eine Influenzmaschine.''
   Phil. Mag. [4], xxx., 159, 1865.
2. *Pogg. Ann.*, cxxvi., 157, 1865, '' Ueber eine neue Elektrisirmaschine
   '' (Influenzmaschine).''
   Phil. Mag. [4], xxx., 425, 1865.
   Ann. Chim. Phys. [4], viii., 201.
3. *Pogg. Ann.*, cxxvii., 320, 1866, '' Ueber eine neue Elektrisirmaschine
   '' (Fortsetzung).''
4. *Pogg. Ann.*, cxxx., 128 and 168, 1867, '' Ueber die höhere Ladung isoli-
   ''renden Flächen durch Seitenanziehung, und die Uebertragung
   ''dieses Princips auf der Construction von Influenzmaschinen
   '' (Maschine mit zwei entgegengesetzt rotirenden Scheiben).''
5. *Pogg. Ann.*, cxxx., 287, 1867, '' Ueber Influenzmaschinen für hohe
   '' Dichtigkeit, mit festen influenzirenden Flächen.''
6. *Pogg. Ann.*, cxxxvi., 171, 1869, '' Zwei ältere Influenzmaschinen in neuer
   '' Gestalt.''
7. *Pogg. Ann.*, clvi., 627, 1875, '' Einige weitere Versuche zur Verbesser-
   '' ung der einfachen Influenzmaschine.''
8. *Pogg. Ann.*, Ergänzungsband vii., 332, 1875, '' Ueber das Maximum der
   '' Rotationsgeschwindigkeit der Influenzmaschinen.''

peculiarities of Holtz's early machines were, firstly, that no metal carriers were provided upon the rotating disc; secondly, that the field-plates were of varnished paper; thirdly, that in the fixed glass sheet, upon which the field-plates were mounted, windows or openings were cut away; fourthly, that through these openings there projected pointed paper tongues which took the place of the regenerating or appropriating brushes; fifthly, that the discharging knobs were inserted in the neutralising circuit, which united two metallic combs lying on the front side of the rotating disc, opposite the two field-plates; sixthly, that whilst the neutralising combs were thus on the front of the rotating disc, the regenerating tongues were situated at the back of the same. All these features, which are found in the description given in memoir 2 of the list (the first full account published), are noteworthy. Most of them are to be found in the Holtz machines as they exist in commerce, and some of them are characteristic of this type (Fig. 12). In order to set into action machines of this type

9. *Pogg. Ann.*, Ergänzungsband vii., 497, 1875, "Einige Formveränderun-"gen der Batterie und ihr Gebrauch bei Influenzmaschinen nebst "Behscreibung einiger sehr schöner Entladungsphänomene."

10. *Götting. Akademieberichte*, Marz, 1876, "Einigie wesentliche Verbesser-"ungen an einfachen und zusammengesetzten Influenzmaschinen."

11. *Pogg. Ann.*, Jubelband, 486, 1876, "Berichtigung betreffend die "angebliche Vorzüglichkeit des Ebonits an Stelle des Glases bei "Influenzmaschinen."

12. *Berl. Monatsberichte*, Aug., 1876, 501, "Ueber die Hülfsconductoren der "einfachen und zusammengesetzten Influenzmaschinen."

13. *Pogg. Ann.*, Ergänzbd. viii., 431, 1877, "Ueber die neueste Form der "einfachen Influenzmaschine und ihren Gebrauch."

14. *Mittheil. d. Naturwissensch. Vereins für Neuvorpommern u. Rügen.*, ix., 125, 1877, "Zur Theorie der Influenzmaschine."

15. *Ibid.*, xi., 72, 1879, "Zur Construction der Influenzmaschine."

16. *Zeitschrift. f. d. ges. Naturwissenschaften*, liii., 124, 1880, "Zum Gebrauche "der Influenzmaschine."

17. *Carl's Repertorium d. Physik*, xvii., 612, 1881, "Ueber Influenzmaschinen "mit unipolarer Erregung."

18. *Wied. Ann.*, xiii., 623, 1881, "Experimentelle Beiträge zur Theorie der "Influenzmaschine."

19. *Uppenborn's Zeitschr. f. angew. Elektr.*, iii., 198, 1881, "Zur Kentniss und "Vervollkommenung der Influenzmaschinen und ihrer Wirkungen."

20. *Ibid.*, iii., 243, 1881, "Ueber Influenzmaschinen für lange Funken."

21. *Uppenborn's Centralblatt f. Elektrotechnik*, 688, 1883, "Ueber die Influenz-"maschine mit zwei entgegengesetzt rotirenden Scheiben."

the two knobs of the discharging circuit must be put into contact. Then the machine is put into rotation, and while so rotating an initial charge must be given (either by contact or influence) to
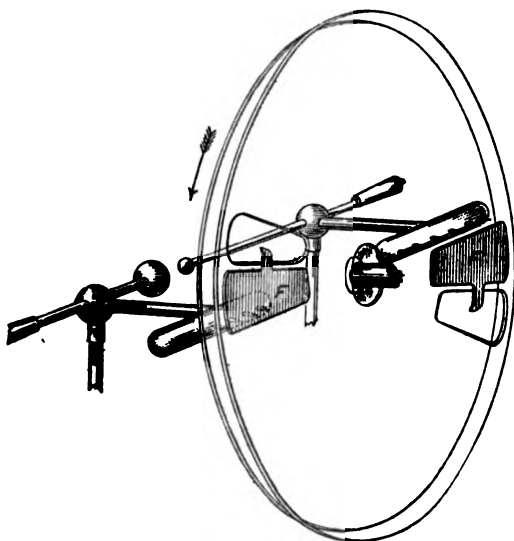
FIG. 12.—Holtz's Machine (typical form).

one of the paper field-plates. This was usually accomplished either by touching it with the knob of a small Leyden jar which had been previously charged from some frictional machine, or by holding behind the field-plate a small piece of sheet ebonite which had previously been rubbed with warm flannel or with fur. The machine then charged itself, and as the discharging rods were drawn apart a torrent of sparks rushed across between the knobs, succeeded, as the distance was further widened, by a strong brush discharge. Such was the usual pattern of machine. Holtz, however, mentions some possible variations: the use of a diagonal conductor to act as a second neutralising circuit, when the knobs of the discharging circuit are open; the use of four field-plates and four windows instead of two; and the advantage, where long sparks are desired, of unipolar excitation— that is to say, of using but one field-plate and one window opposite a single rotating disc. This unipolar type of machine (to which, indeed, Toepler's first machine and Nicholson's spinning

condenser alike belong) formed the subject of memoirs 5 and 17 of Holtz's series.   The use of the diagonal neutralising conductor (which is in influence machines the analogue of the shunt circuit in dynamos, and keeps the machine excited when the main circuit is opened) was discussed in 3, 6, and 12.   The use of ebonite instead of glass (which Holtz does not advocate) was considered in 7 and 11.   The effect on the discharge of various arrangements of Leyden jars was experimentally investigated in 9 and 16, resulting in the practice of connecting two jars, one to each discharging conductor, their outer coatings being connected together also.   Whenever a discharge is taken at the terminal knobs, there also occurs a rush of electricity in the wire that connects these coatings : in some machines a special discharging apparatus is interposed in this circuit.   Various complex forms of machine were designed also by Holtz, some of which deserve notice. In (5) 1867 a machine "of the second order" (Fig. 13) was produced.
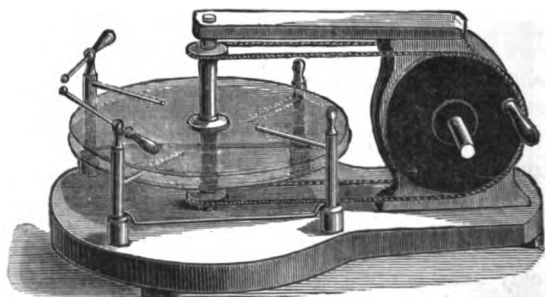
FIG. 13.—Holtz's Machine of Second Order.

By this term appears to have been meant* a machine in which all coatings on the glass plates, whether of foil or of paper, were abolished.   This was accomplished by making both glass discs movable, and imparting opposite rotations to them, around a vertical axis.   Two insulated combs were fixed above the upper disc, at opposite extremities of one diameter, and two other combs below the lower plate, at right angles to the first.   Fig. 13 shows how the motion was communicated, and how the discharging knobs were arranged.   This kind of machine, though further noticed by

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* See Wiedemann, "Die Lehre v. d. Elektricität," ii., p. 211.

Poggendorff* and by Musæus,† does not appear to have at that time established itself as a successful machine. None were introduced into this country. In 1876 Holtz (10) modified this type, giving the machine eight combs—four above the upper plate, at right angles to one another, and four below the lower plate, alternating in position with the upper four. Yet another modification had been made in 1869, in which (6 and 21) there were two neutralising conductors, set in crossed diagonals, and furnished with combs. Holtz himself suggested (10 and 19) that this construction might have been made self-exciting by the expedient of fastening metallic carriers on the discs and replacing the combs by contact-brushes; but, added he in 1883 (21), " the self-exciting mania " about influence machines only began later than this." Yet another type of machine (17) consisted of two discs rotating in the same direction, close to one another, on two separate parallel axes, so that the two adjacent parts of the plates moved past one another. Outside these were two field-plates, fixed on the outer sides of two stationary glass sheets. These two field-plates received opposite charges (and were replenished by the action of the machine itself), so that between them there was an intense electric field. It was in this interspace that the active parts of the two rotating discs moved past one another, being momentarily put into contact as they passed one another by a double comb lying in the gap between them.

It is impossible here to narrate a tithe of the details which successively were examined by Holtz; the wonder is that this indefatigable worker left anything for others to discover.

Amongst those who interested themselves in the work of Holtz and of Toepler were Professor Poggendorff and Professor P. Riess. Already, in 1867, Poggendorff had studied the action of the diagonal conductor ;‡ and later he devoted attention to multipolar machines,§ to the circulation of currents in the various parts,‖ and

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* *Berl. Monatsberichte*, 1872, p. 817 ; and *Pogg. Ann.*, cl., 1, 1873.

† *Pogg. Ann.*, cxliii., 285, 1871 ; and *Ibid.*, cxlvi., 288, 1872, including a special study of the diagonal conductors in this type of machine.

‡ *Berl. Monatsberichte*, Feb. 18, 1867.

§ *Ibid.*, April, 1869 ; and *Pogg. Ann.*, cxxxix., 158, 1869 ; also *Ann. Chim. Phys.* [4], xix., 480, 1869.

‖ *Berl. Monatsberichte*, 1870, 275 ; *Pogg. Ann.*, cxli., 161, 1870 ; also *Ann. Chim. Phys.* [4], xxiii., 335, 1870.

to the reversals of sign of the charges* which in Holtz's machines occur spontaneously in a very perplexing way. He also suggested that the power of the machine might be increased by doubling the number of its plates†—putting two machines, in fact, side by side on one axle. This he did by setting the two rotating armatures at some distance apart, with the fixed plates outside them and the discharging apparatus in between them. This construction was later modified by Ruhmkorff,‡ who put the two fixed plates back to back—a type that has been largely adopted. Poggendorff also discussed the construction of machines of the " second order;"§ investigated the effect of thickness of the rotating plate,‖ and found it immaterial; and tried experiments on the difference between rotating one plate and both plates.¶ He suggested** that the real advantage of using varnished paper as a material for the field-plates lay in its semi-conducting properties, preventing it from becoming as completely discharged as a metal coating might. He studied the action of the pointed tongues and the general effects of influence on non-conductors.††

Riess‡‡ sought to explain the rather mysterious action of the original type of Holtz machine, and the apparent activity of both faces of its rotating disc, by a principle which he termed " double " influence." This term referred to a phenomenon, investigated by him in 1854, of the following kind:—If a disc of some dielectric material (such as glass) is set into rotation between an insulated charged body and a neighbouring conductor, it is found as an experimental fact that whilst the nearest surface of the latter acquires an induced charge of the same sign as that of the charged body, the interposed moving disc acquires on *both* its surfaces a charge of the opposite sign to that of the charged body. The

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* *Pogg. Ann.*, cliv., 643, 1870.

† *Ibid.*, cxli., 161, 1870, &c. See drawing in Wiedemann's " Die Lehre " v. d. Elektricität," ii., p. 220.

‡ See Mascart's " Traité d'Électricité Statique," ii., pp. 287, 288.

§ *Pogg. Ann.*, cl., 1, 1878.

‖ *Ibid.*, clii., 562, 1874.

¶ *Ibid.*, cliii., 80, 1874.

** *Berl. Monatsberichte*, April 15, 1869.

†† *Ibid.*, July 19, 1869, p. 590.

‡‡ *Ibid.*, April 4, 1867, p. 183.

same phenomenon was investigated by Faraday[*] in 1856. Riess studied from this point of view the action of the electrophorus and of various electrophoric machines, which he divided into four categories—(1) simple electrophoric machines with moving metallic conductors, such as Toepler's (1865) machine; (2) electrophoric machines having a single moving glass disc, such as Holtz's (1865) machine; (3) electrophoric machines having two glass discs rotating in the same direction, such as one of Toepler's[†] of 1866; (4) electrophoric machines having two glass discs rotating in opposite directions, such as Holtz's machine of the "second order" (1867). Riess proposed the name of "influence machines" for all the apparatus comprised in these four classes. He considered the phenomenon of double influence to enter into all except the first, but he regarded those of the second class as being the only machines in which all the *three* induced charges peculiar to double influence were actually utilised. Riess further devoted some attention to the historical antecedents of these machines,[‡] to the use of the diagonal conductor for preventing reversal of charges,[§] to the effect of the size of the glass plates[||]—concerning which he observed that small pieces of glass to support the paper field-plates do not answer so well as the large fixed sheets of glass used by Holtz—to the decomposition of water by means of Holtz's machines,[¶] and to the direction of the currents in the diagonal conductors.[**]

Sir William Thomson, whose researches were begun independently of those of Toepler and of Holtz, showed even greater originality of conception. The best known of his machines is probably the little "replenisher" as used to charge the Leyden jar of his quadrant electrometers. In its original form as designed for use in connection with cable-signalling the machine

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[*] *Phil. Mag.* [3], xi., 8, 1856.

[†] *Pogg. Ann.*, cxxvii., 177, 1866.

[‡] *Berl. Monatsberichte*, 6th Dec., 1869, p. 861.

[§] *Ibid.*, 6th Jan., 1870, p. 1; and *Pogg. Ann.*, cxl., 168, 1870.

[||] *Berl. Monatsberichte*, 20th Nov., 1873, p. 765; and *Pogg. Ann.*, cliii., 584, 1873.

[¶] *Berl. Monatsberichte*, 16th March, 1874, p. 196.

[**] *Ibid.*, 10th April, 1876, p. 234; and *Pogg. Ann.*, clx., 486, 1876.

was described* as follows:—" I have designed a combination of
" the electrophorus principle with the system of reciprocal induc-
" tion explained in a recent communication to the Royal Society.
". . . A wheel of vulcanite, with a large number of pieces of
metal (called carriers, for brevity) attached to its rim, is kept
" rotating rapidly round a fixed axis. The carriers are very
" lightly touched at opposite ends of a diameter by two fixed
" tangent springs. One of these springs (the earth-spring) is
" connected with the earth, and the other (the receiver-spring)
" with an insulated piece of metal called the receiver, which is
" analogous to the 'prime conductor' of an ordinary electric
" machine. The point of contact of the earth-spring with the
" carriers is exposed to the influence of an electrified body
" (generally an insulated piece of metal) called the inductor.
" When this is negatively electrified, each carrier comes away
" from contact with the earth-spring, carrying positive electricity,
" which it gives up, through the receiver-spring, to the receiver.
" The receiver and inductor are each hollowed out to a proper
" shape, and are properly placed to surround, each as nearly as
" may be, the point of contact of the surrounding spring. The
" inductor, for the good working of the machine, should be kept
" electrified to a constant potential. This is effected by an
" adjunct called the replenisher, which may be applied to the
" main wheel, but which, for a large instrument, ought to be
" worked by a much smaller carrier-wheel attached either to the
" same or to another turning shaft.

" The replenisher consists of two properly shaped pieces of
" metal called inductors, which are fixed in the neighbourhood
" of a carrier-wheel such as that described above, and four fixed
" springs touching the carriers at the ends of two diameters.
" Two of these springs (called receiver-springs) are connected
" respectively with the inductors; and the other two (called
" connecting springs) are insulated and connected with one
" another (one of the inductors is generally connected with the
" earth, and the other insulated). They are so situated that they

* Phil. Mag., 1868; see also Sir W. Thomson's "Reprint of Papers on
" Electrostatics and Magnetism" (1872), p. 330.

" are touched by the carriers on emerging from the inductors
" (Fig. 14), and shortly after the contacts with the receiver-

<div align="center">Section.   Elevation.</div>
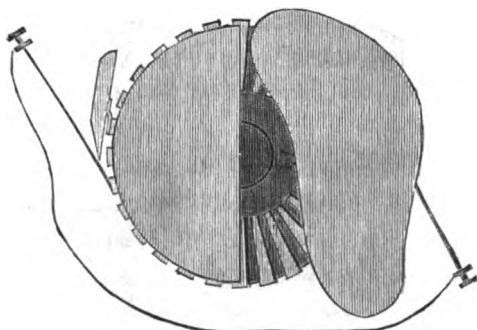
<div align="center">FIG. 14.—Sir W. Thomson's Replenisher (type of 1867).</div>

" springs. If any difference of potential between the inductors
" is given to begin with, the action of the carriers, as is easily
" seen, increases it according to the compound interest law as
" long as the insulation is perfect. Practically, in a few seconds
" after the machine is started running, bright flashes and sparks
" begin to fly about in various parts of the apparatus, even
" although the inductors and connectors have been kept for days
" as carefully discharged as possible. . . . The only instrument
" yet made is a very small one (with carrier-wheel only two inches
" in diameter), constructed for the Atlantic Telegraph application;
" but its action has been so startlingly successful that good effect
" may be expected from larger machines on the same plan.

    " Several useful applications of the replenisher for scientific
" observations were shown by the author at the recent meeting
" of the British Association (Dundee)—among others, to keep
" up the charge in the Leyden jar for the divided-ring electro-
" meter."

    The instruments thus introduced by Sir W. Thomson under
the very descriptive title of " Electric Machines founded on
" Induction and Convection," were developed subsequently into
the *mouse mill* of the modern siphon recorder, and the cylin-
drical form of *replenisher*.

    The modern form of the replenisher is shown in Figs. 15
and 16, whilst a diagrammatic representation of its working

parts is given in Fig. 17. There are two "field-plates" or "inductors" (F, F), cut from portions of brass tube. There are two metal "carriers" (C, C), fixed to a cross arm of ebonite.
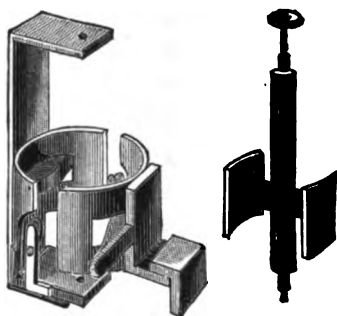
Figs. 15 and 16.—Replenisher for Quadrant Electrometer (two-thirds of actual size).

Through holes cut in the field-plates there enter the two "connecting springs" or "neutralising brushes" (n, n), the connecting arc which joins them corresponding to the connecting arc
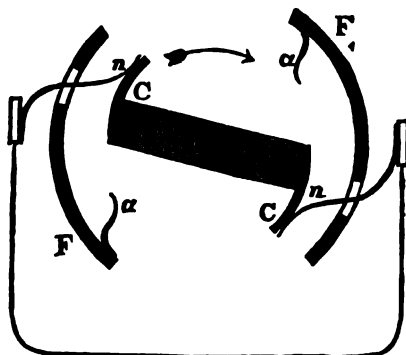
Fig. 17.—Diagram of Sir W. Thomson's Replenisher.

of the Varley machine, or to the diagonal conductor of the Holtz machine. The two "receiving springs" (a, a) appropriate and convey to the field-plates the charges brought round by the carriers, precisely as do the internal spiral springs used in Belli's doubler. In Fig. 14 the neutralising springs enter holes in the field-plates; whilst in Fig. 15 the receiving springs are those that enter the holes, the neutralising springs rising from the base. In both these figures the rotation is supposed to be left-handed.

In the *mouse mill*, used for electrifying the ink vessel of the

siphon recorder in cable-signalling, is a much more highly
organised instrument.   It is both an influence machine and an
electro-magnetic motor in one.   A voltaic battery drives the
motor, and the motor drives the influence machine.   The motor
is of the Froment type, having ten parallel iron bars arranged
on the periphery of a drum, to be attracted successively by an
electromagnet.   But the same iron bars which thus serve as
magnetic armatures in the motor serve as electric carriers in
the influence machine, they being mounted on insulating
supports.   Surrounding the rotating part on either side are two
field-plates of nearly semi-cylindrical form.   Attached severally
to the ten carrier bars are ten pins, which project (like the com-
mutator of a dynamo machine) at one end of the revolving part.
These are touched as they go round by two receiving springs
(a, a) and two connecting springs (b, b, Fig. 18), exactly as in the
replenisher.*
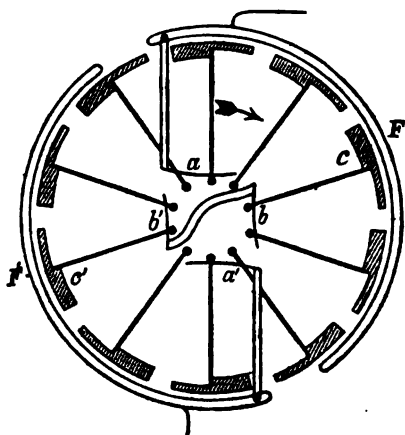
FIG. 18.—Sir W. Thomson's Mouse Mill.

Amongst the applications hinted at in the above description
of the replenisher was the " potential-equaliser," consisting of a
disc with carriers touched by two springs leading to the elec-
trodes of an electrometer.   By means of this apparatus† it was

* See Ewing's "Description of Sir W. Thomson's Siphon Recorder"
(Edinb., 1876) for full description of this instrument.   An account is also given
in the *Electrotechnische Zeitschrift*, vi., 339, 1885.

† Figured at pp. 834, 335 of Thomson's "Reprint."

found possible to demonstrate the slight electrification of a tourmaline crystal, and to test the small difference of potentials between zinc and copper brought into contact in the air. Sir W. Thomson suggested, for machines to give high electromotive force, that it would be better to substitute for the carrier-wheel " an endless rope-ladder, as it were, with cross-bars of metal and " longitudinal cords of silk or other flexible insulating material" —a suggestion which, in slightly modified form, was subsequently revived by Righi* in 1872. It will have been noticed in all the preceding machines Sir W. Thomson proposed to employ actual spring contacts and metallic carriers. In his postscript of 1868 he, however, adds (and this point I confess I do not quite understand) that the great power of the Holtz machine depended on the abolition of metallic carriers and of metallic make-and-break contacts.

Here are some examples of Sir W. Thomson's machines— a small replenisher, and a large model differing only in size from the actual apparatus used in electrometers. This is 11 inches in height; it is self-charging. It was constructed by my assistant, Mr. Rousseau. Sir W. Thomson has also been kind enough to lend me for exhibition two other machines. They are both intermediate forms in the development of the mouse mill, and in both the analogies between the influence machine and the dynamo machine can be readily traced out.

I should, however, do a great injustice to Sir W. Thomson if I led you to imagine that these were his only contributions to the subject. His replenisher dates, it is true, from 1867, but prior to this he had worked at the principle of the reciprocal electrophorus in a novel and most interesting manner. In 1860, in a lecture at the Royal Institution,† Sir William Thomson had described a method of equalising the potential of a conductor with that of the air at a point above it by causing drops of water to break away from the nozzle of an uninsulated can and fall upon the conductor in question, so obtaining a self-

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* *Nuovo Cimento*, vii. and viii., 123, 1872.

† See Thomson's " Reprint," p. 233.

acting electric condenser. Early in June, 1867, Sir William further pointed out* that if, owing to electrified bodies in the neighbourhood, the potential in the air surrounding the place where the stream breaks away in drops is positive, the drops themselves fall away negatively electrified; they have, in fact, been virtually touched while under influence. He further proposed to make two such dropping arrangements act reciprocally on each other, the charge acquired by the falling drops from one nozzle being used to charge an insulated conductor placed near to the jet from a second nozzle. The inductors (or field-plates) in the water-dropping influence machine (Fig. 19) consisted of open vertical metal cylinders (b) so fixed around the two nozzles (a) that the respective jets break away into drops about the middle of the cylinder; thence they drop into lower collecting vessels (c) pro-

FIG. 19.—Principle of the Water-dropping Influence Machine.

vided with internal funnels, through which they again break away. These lower vessels serve as the receiving springs of the replenisher, and are cross-connected each to the inductor surrounding the other jet. Leyden jars are added to collect the charges. Fig. 20 shows the complete water-dropper. Sir W. Thomson further suggested that a larger number of streams of water might be used, the receiver of the first stream being connected to the inductor surrounding the second, the receiver of this to the inductor of the third, and so forth.

A few months ago I thought it might be more convenient if the construction of the water-dropping machine were somewhat simplified, and accordingly I have here a much more portable form of Sir W. Thomson's water-dropper, which I showed to the Physical Society, and have described in the *Philosophical Magazine*. The water drops here through a cylinder at the top into a second cylinder, which has an internal funnel. From this it again drops—being touched in the act of dropping, by a wire

* *Proc. Roy. Soc.*, June 20, 1867; also see Thomson's "Reprint," p. 319.

underneath, while under influence—into a third cylinder, which is, in fact, a mere can to catch the water. This can is metallically connected to the topmost cylinder. I find that less than half a
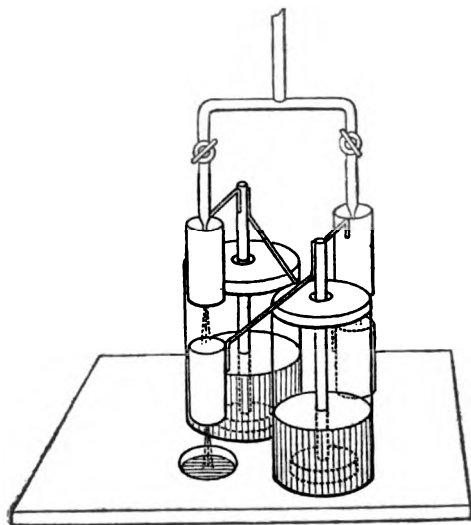
FIG. 20.—Sir W. Thomson's Water-Dropper.

pint of water is sufficient to charge the instrument up. The insulation is effected, as shown in Fig. 21, by hanging the cylinders and can upon silk strings well paraffined.
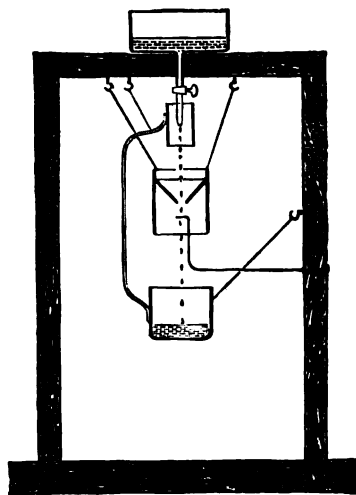
FIG 21.—Modified Water-dropping Influence Machine.

I have described at some length the researches of Toepler, of Holtz, and of Thomson.   It is not remarkable that many other persons should have had their attention drawn by these researches to the same subject.   In France, in Germany, in England, and in the United States experiments were made and details worked out far too numerous to describe.   Only a few of these can be here touched upon.
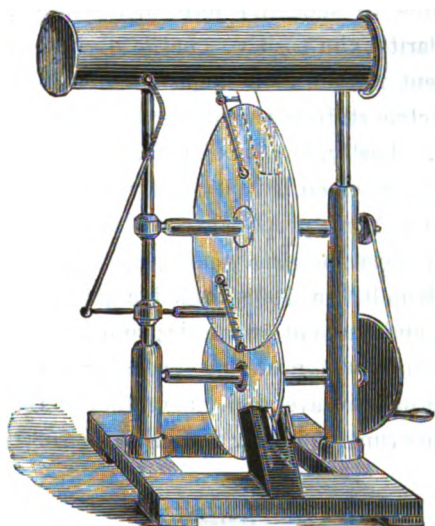
FIG. 22.—Carré's Machine.

Carré,* in 1868, devised a machine (Fig. 22) in which both friction and influence are utilised; there being two discs, one of which rotates slowly between friction-cushions, whilst the other, the surface of which receives induced charges from a metallic comb opposite the rubbed plate, acts as a continuous carrier and conveys the charges to a receiving comb connected to the prime conductor at the top of the apparatus.   Carré used ebonite for his discs.   His machine was self-exciting, and not liable to reversals, but unequal in power to a Holtz machine of similar size.

Schwedoff,* in 1871, wrote a remarkably clear and full

* Mascart's " Traité d'Électricité Statique," ii., 292; see also *Carl's Re-pertorium,* vi., 62, 1870.

† *Pogg. Ann.,* cxliv., 597, 1871.

account of several varieties of influence machine. One of his forms completely anticipates Voss's construction. Another is of the second order, with oppositely rotating discs.

Meantime, improvements came about in other directions. The difficulties that were found in the construction and use of Holtz's machines led to various suggestions in detail. The machines were not self-exciting, but required an initial charge. They were liable to suddenly lose their charge, or to become reversed in polarity, the positive charge abruptly changing, often without apparent reason, to a negative one. Their sensitiveness to the hygrometric state of the atmosphere made them capricious in their action. Lastly, the peculiar form of the fixed plate, with its two windows, involved liability to fracture. In 1866 Professor Morton,[*] of New York, proposed to replace the Holtz plate, with its windows, by pieces of glass coming up from four sides.

Professor Kundt,[†] in 1868, added a friction disc to provide initial charge and prevent reversals; and about the same time a similar addition was made by Professor Clerk-Maxwell in a Holtz machine in the Cavendish Laboratory at Cambridge.

Voss,[‡] an instrument maker of Berlin, in 1880 devised a form
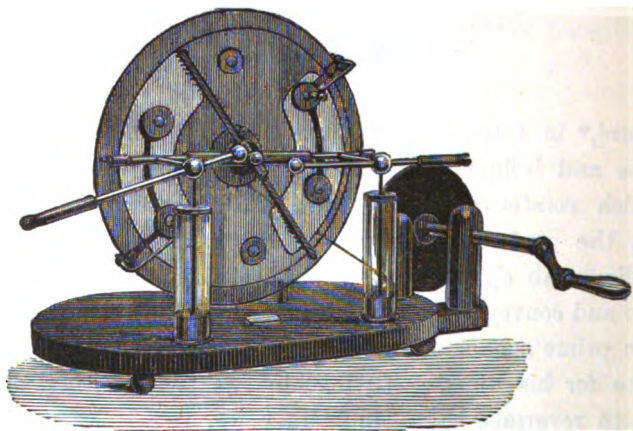
FIG. 23.—The Voss Machine.

* See *Journal of Franklin Institute* [3], liii., 119, 1867; see also *Les Mondes*, xiii., 390, and xv., 489.

† *Pogg. Ann.*, cxxxv., 484, 1868.

‡ *Dingler's Polytechnisches Journal*, 1880, p. 476; see also Nebel, *Exner's Repertorium der Physik*, xxiii., 324, 1887.

of machine (Fig. 23) in which it is claimed the principles of the
Toepler and Holtz machines are combined. The carriers of metal
foil are provided with projecting buttons against which the
brushes touch as the armature revolves, as in Toepler's later
machines. The field-plates are of foil, backed up by larger pieces
of varnished paper. There are no windows, but the appropriating
brushes are mounted on short conductors which project round
from the back of the fixed plate to the front of the rotating plate.
The general design and symmetrical construction, the manner of
support of the rotating plate, the use of the diagonal conductor,
and the employment of the two Leyden jars, are all amongst the
features borrowed from Holtz. Such machines are cheaper to
construct than machines in which windows are cut in the fixed
plate, and they are not so liable to fracture.

I now pass to the recent work done by Mr. James Wimshurst*
—a name well known to you as being that of one who has found
time amidst his official duties to contribute largely to the stock
of knowledge in respect of the construction of really reliable
machines. About 1878, when Mr. Wimshurst was thinking of
the problem of the influence machine, it occurred to him that
the best way to make those windows in the Holtz machine was
to take two separate rectangular pieces of glass and fix them a
little apart from one another, with the paper field-plates pasted
on behind, the tongues projecting through the gap between
them. That, indeed, was the beginning of Mr. Wimshurst's
researches. His first step was to adopt this small improvement.
There is here on the table a small 12-plate Holtz machine devised
at that time by Mr. Wimshurst on this plan. A larger machine
on the same plan, with twelve discs, each 2 feet 7 inches in
diameter, was constructed at the same time. This machine,
which still stands in Mr. Wimshurst's house, gives an extra-
ordinary volume of discharge, but it is not self-exciting. A
year or two later, Mr. Wimshurst, who had no knowledge of
Holtz's later machines, hit upon a form of machine which in some

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* The best account of Mr. Wimshurst's researches is in a lecture by
Mr. Wimshurst at the *Royal Institution*, April 27, 1888, a report of which,
with illustrations, will be found in *Engineering*, xlv., 430, May 4, 1888.

respects resembles Holtz's machine of the "second order," though it has many points of difference. Certain things suggested to his mind that it would be better to make both plates go round, and to make them go round in opposite directions. That idea was soon put into shape, and we have here on the table the original machine exactly as it was the day that it was completed—a pattern which has hardly been altered even in the most recent of Mr. Wimshurst's machines. The two discs are driven in opposite directions; each of them is furnished with a number of metallic sectors as carriers. There are two sets of neutralising brushes going across diagonally, front and back, opposite alternate quadrants; and there are, right and left, a pair of collecting combs connected to a pair of discharging knobs. This machine charged itself the first time it was tried, and now charges itself as I turn the handle.

This machine at once established itself as a permanent type (Fig. 24), it being at once simple to construct* and, when properly adjusted, self-exciting without fail. A very large machine of this type, having two revolving plates, each 7 feet in diameter, was constructed by Mr. Wimshurst† in 1884, and is now in the National Science Collection at South Kensington Museum.

From time to time other patterns embodying the same idea have been produced. Mr. Wimshurst has kindly lent me a machine, made in 1882, with two rotating cylinders, one inside the other—a pattern which has lately been made up differently with ebonite cylinders.‡ I have here a larger machine by Mr. Wimshurst, which he has kindly lent me. It has four pairs of rotating plates.§ There is no trouble about exciting this kind of machine; it excites itself with a very small number of turns of the handle. Like some of the later Holtz machines, it is enclosed in a glass case; but it requires, unlike Holtz's machines, no

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* See "The Influence Machine: How to Make it, and How to Use it" (Pewtress & Co., London, 1886). A short reprint of some popular articles by J. Wimshurst, edited by Thomas Gray.

† For full description and drawing of this giant machine, see *Engineering*, xxxix., 60.

‡ See *The Electrician*, May 18, 1888, p. 45.

§ See *Engineering*, xli., 495.

desiccating substance (such as chloride of calcium) to be put inside the case.

In machines of this type the metal sectors serve a double
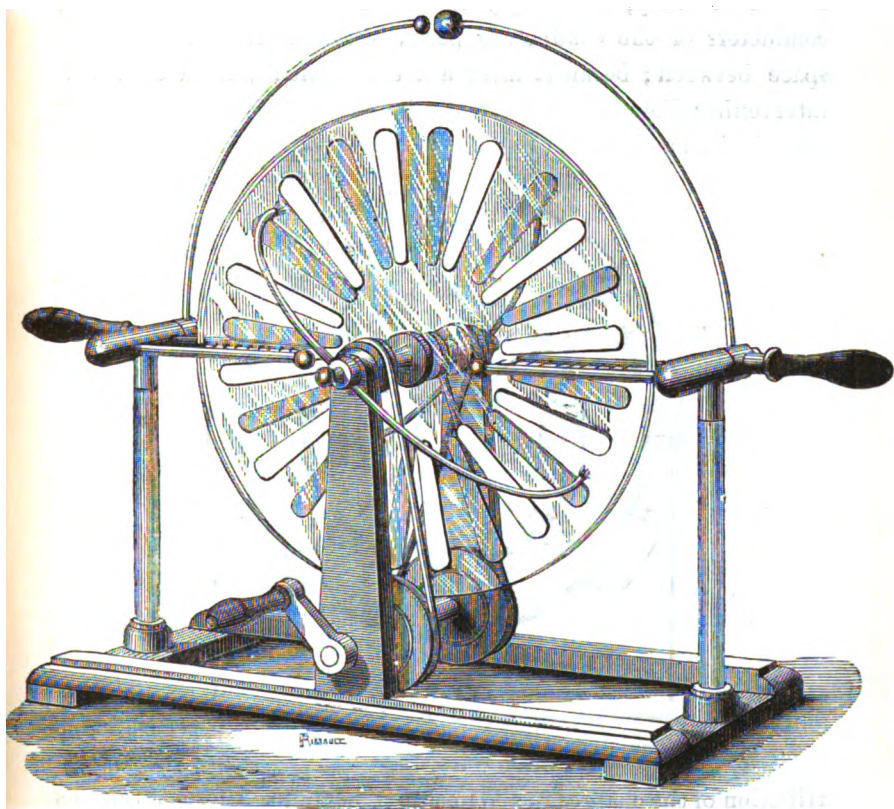
FIG. 24.—Wimshurst's Machine (type of 1881).

function : they act as *carriers*, receiving (by being touched while under influence) induced charges and conveying them round ; and whilst so acting they, during a portion of their rotation, act also as *inductors* or *field-plates*, influencing the sectors of the other disc as they pass.

Now it has occurred to me that it may be useful to have a somewhat different method of representing the facts than has hitherto been common.   The common method of representing the action of a machine by drawings such as depict the parts as sections of a cylinder is due to Bertin.   I have used it here in diagrams

explaining Toepler's and Sir W. Thomson's machines, also of Clarke's gas-lighter (Fig 32). It has occurred to me that it was better to have some kind of a drawing in which one would be able to study, not simply the charges on the surfaces of the conductors or the conducting parts, but also the action in the space between; because, after all, that which goes on in the intervening dielectric is the really important thing. Therefore I have devised a diagram (Fig. 25) to represent the facts of the dis-
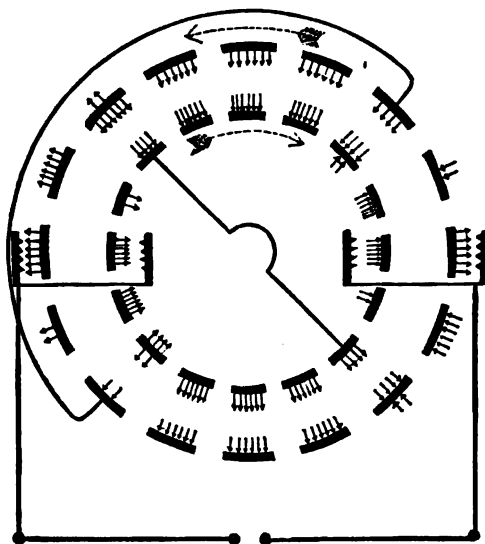
Fig. 25.—Diagram illustrating Action of Wimshurst's Influence Machine.

tribution of the charges in a Wimshurst machine. Those two circles of black marks represent the two sets of metallic sectors or carriers on the two rotating plates; each plate, front and back, has its set of carriers. Here they are represented as one being outside the other, and as this is drawn the inner set must be considered as going round right-handedly, and the outer set as going round left-handedly. Now consider what would be the result if a sector at any point possessed a positive charge. It will exercise an influence all round it. You will have lines of electric force proceeding from it into the field. I have not attempted to draw these lines, but I have attempted to suggest them. If you have an insulated charged conductor in the middle of a space, its

electric lines may be considered as radiating out in all directions. If you have a disc charged—say, positively—from every unit of the charge on its surface, there will go out lines of electric force in different directions.   Suppose you have a negatively charged plate, you will also have lines coming in from different directions. In Fig. 26 I suggest that there are two insulated metal discs, seen
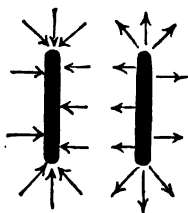
FIG. 26.   FIG. 27.

edgeways, the left-hand one charged negatively, the other positively, at some distance apart.   The charges are distributed most strongly at the edges of the discs, but also to some extent on their flat surfaces.   In Fig. 27 the same two discs are represented as having now been put close together, with the result that their charges now act strongly upon one another, nearly all the lines of electric force being concentrated across the intervening layer of non-conducting air or glass.   You have these positive and negative charges attracting one another, and accumulating on the two surfaces which are nearest together, the electric forces acting across the dielectric, which lies between.

I have chosen to represent the positive units by drawing arrows with their heads proceeding out from the surface, and the negative charges are represented by the arrow-heads turned with the head inwards towards the charged surface.

Now in the Wimshurst machine, to which this diagram with its two circles relates, some of the metal carriers are charged positively, others negatively.   Consider the carrier on the back (or outer) set positively charged, and situated on the left at 45 degrees above the horizonal diameter.   Opposite this positively charged sector there is a metallic sector in the front (or inner) set, going across from left to right.   As it passes it comes into contact with a brush of fine wires on the end of the diagonal

conductor. This neutralising brush touches it while under influence. As a consequence electric displacement will occur across the dielectric at that point, and this metal sector will become negatively charged, some electricity flowing out of it down the diagonal conductor. As all the front (inner) sectors pass this point they go on, over the top, negatively charged; whilst for precisely analogous reasons the back (outer) sectors behind them come over the top from right to left positively charged; they have been touched by the other neutralising brush while under the influence of the negative charges on the front (inner) sectors. The sectors of each set, while acting as carriers of their own respective charges, act also as field-plates to influence the sectors of the other set. Further, while the positively and negatively charged carriers are passing one another, their charges attract one another, and there would occur sparks across between them if it were not for the plates of glass which separate them. As each carrier comes up to be touched by the neutralising brush, there will be a rush of electricity between the two ends of that diagonal conductor, where similar actions, but of opposite sign, are taking place. I have drawn my diagram as though each (back) carrier coming over the top from right to left brought 6 units of positive charge. Each carrier of the front (inner) set, as it passes under the neutralising brush at 45 degrees above the horizontal on the left, is considered as having electricity driven out of it by the influence of these positive charges, and as being left in a state of negative charge to the amount of 6 negative units. In the lower half of the machine similar actions are going on, with the result that the positive charges are brought to the left side of the machine by both discs, whilst negative charges are brought by both discs to the right side. A pair of discharging combs face the two discs on the left side to receive the surplus positive charges, and on the right there is a pair to receive the negative charges. These combs are connected to the discharging rods and knobs.

By way of comparison I have given, in Fig. 28, a similar diagram of the action in a machine of Voss pattern. The field-plate on the left is positively charged, that on the right

negatively.   Consider the sector which is just leaving the
presence of the upper end of the left-hand field-plate : it has
been (and still is) touched by the neutralising brush whilst under
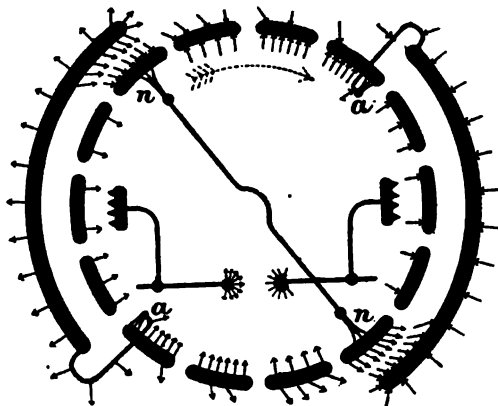
FIG. 28.—Diagram illustrating Action of Toepler-Voss Machine.

influence.   The result is that electricity has been urged down
the diagonal conductor from the place where (in the neighbour-
hood of the positive field-plate) the potential was high toward
the other end, where the potential is low, and consequently the
sector has been left negatively charged.   As long as it is in the
neighbourhood of the field-plate the opposing positive and
negative charges attract one another across the dielectric; they
would spark across were it not for the thickness of glass between.
Since these charges attract one another work has to be done to
separate them; and as each sector is carried over from left to
right its charge is no longer attracted back toward the left field-
plate, but becomes " free."   As it goes on it meets the receiving
brush or appropriating brush which reaches out from the right-
hand field-plate, and takes part of its charge.   Similarly, at the
lower right-hand part the sectors are being touched while under
the influence of the negative field-plate, and go on from right to
left positively charged, giving up the charge which they have
acquired, or part of it, to the receiving brush on that side, so as
to regenerate the charge of the right-hand field-plate.   Any
surplus after the regenerating action is achieved is available for
the discharging apparatus, which, in itself, forms no essential
part of the machine.

This leads me to speak of a very important point in the construction of all influence machines that is not properly understood by the ordinary instrument makers, and has never before been clearly set forth.   In every influence machine the neutralising conductors and the receiving (or regenerating) conductors ought to be furnished with metallic brushes; but the discharging rods ought to be furnished with combs only.   If you have only combs, as in Holtz's machines, or knobs, as in some of Varley's machines, those machines will not excite themselves.   You require an initial charge in order to make a sufficient effect to produce a discharge across the intervening air.   If, however, you have metallic springs or brushes at the ends of the neutralising conductors, actually touching the carriers, then, as a general rule, the machine will excite itself.   If you have all the contact parts, the two neutralising brushes in front, the two neutralising brushes behind, and *also* the discharging apparatus, made with brushes, the machine may excite itself; but then something else will happen.   If you touch one of the discharging apparatuses so as to take a great discharge from one side, you are discharging through a metallic circuit, and you do more than you intend to do : you will probably reverse the machine in the very act of taking the discharge.   You therefore ought to have, not *brushes*, but *combs*, on the discharging part of the apparatus; and in all the recent examples of Mr. Wimshurst's machines—and, for that matter, I think in the latest Voss machines (Fig. 23) too—you will find that, while the neutralising apparatus is provided with brushes, the discharging apparatus is provided with combs.   I believe that the reason why Mr. Wimshurst's own machines never fail, whatever the humidity of the atmosphere, to excite themselves, is because good metallic neutralising circuits are in every case provided.   That they reverse their charges I believe to be due to the facts, first, that the dischargers are so set that when a spark takes place the field-plates are not discharged, and, secondly, that the discharging circuit is terminated by combs and not by brushes.

Returning to the Wimshurst machine, I have already pointed out that in the region at the top the two sets of carriers acquire

charges of opposite sign, which attract one another.   The same
thing is true of the region at the bottom, but the respective signs
are reversed.   But at the sides, right or left, the charges of the
two plates are of the same sign, and they will of course repel one
another.   Now here is a piece of apparatus made by Mr. Wims-
hurst, which enables me to show you this in the most beautiful
way.   It is simply the mechanism of a machine with two plates,
and with diagonal neutralising rods, furnished with brushes, front
and back ; but there are no discharging rods or combs.   These
discs are made, not of glass, but of that rather vague substance
dermatine—a composition of india-rubber, and I do not know what
other materials, flexible, closely resembling leather.   If I excite
this machine the discs actually bend towards one another top and
bottom, but bulge out away from one another on the right and
left.   Here is another form of Wimshurst's machine (Fig. 29),
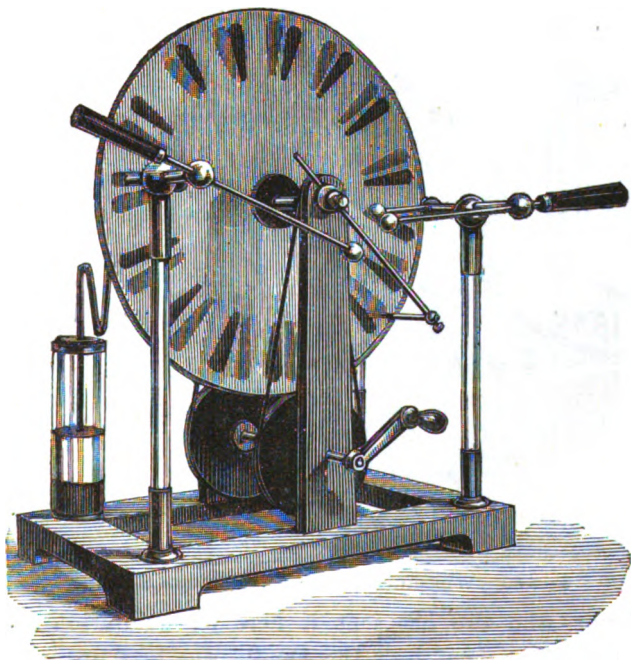
Fig. 29.—Wimshurst Machine (Harvey & Peak's form).

kindly lent for exhibition by Messrs. Harvey & Peak, who
have themselves introduced some small but not unimportant

improvements in the details of mounting the discs on the driving pulleys and in the form of the brush-holders. I will now invite your attention to this very large machine with twelve plates, each 2 feet 6 inches in diameter (Fig. 30), constructed by Mr.
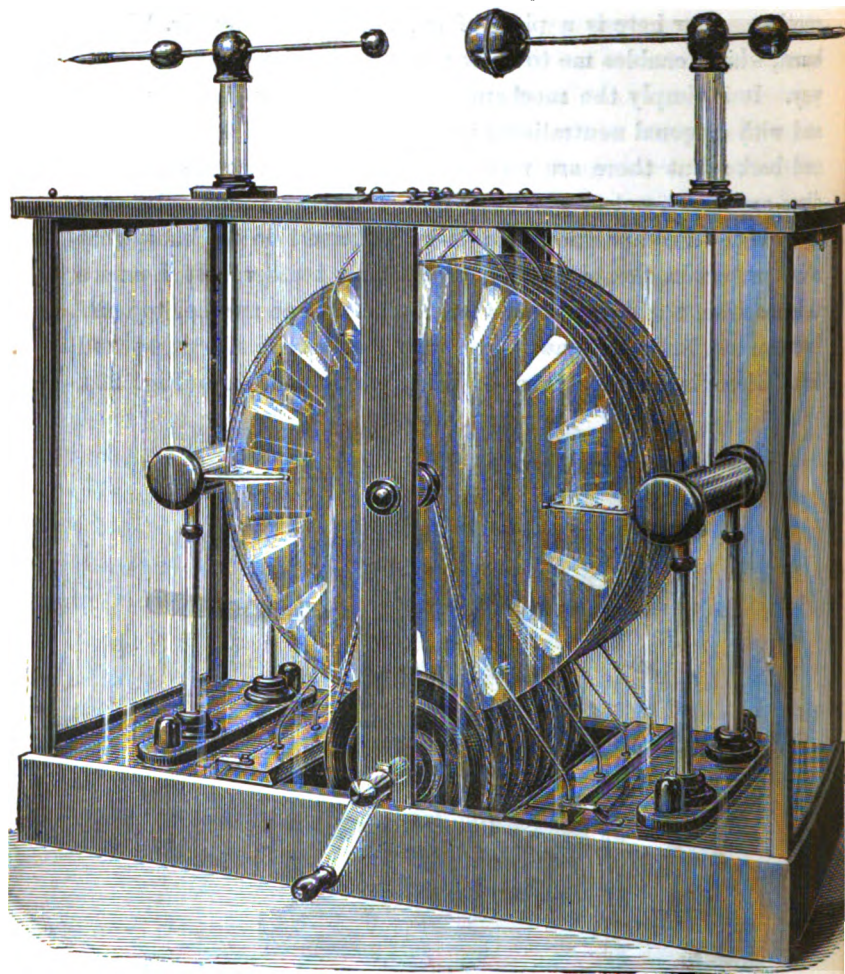
FIG. 30.—Wimshurst's large Twelve-plate Machine.

Wimshurst, who also has sent it here for exhibition to-night. When I left it at six o'clock the handle was tied up, and it has not been moved since; it has been tied up all day. Therefore there is no possibility of its having had any recent charge. I want you to see that this machine will excite itself, and I ask

you to notice with what an extremely small amount of turning the machine does excite itself. I will take this very simple electroscope and connect it on, and then I will turn as slowly as I can. You are to tell me when the machine excites itself. [Professor Thompson here experimented with the machine, which had strongly excited itself when the handle had been slowly moved through about one-tenth of a single revolution.] There is not much doubt about the power of that machine to excite itself; in fact, those machines, made as they are with good metallic connections for the two neutralising circuits that constitute the real working part of the machine, never fail to excite themselves. [Mr. Wimshurst here assisted Professor Thompson in experimenting with the machine. Fig. 31 gives in reduced size a
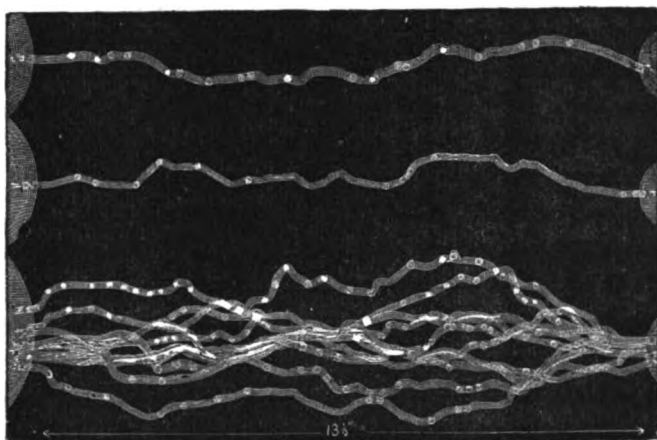
Fɪɢ. 31.—Photographs of Sparks from a Wimshurst Machine.

photograph by Mr. Dredge of some of the sparks, nearly 14 inches long, discharged from this most powerful machine.]

My account of influence machines would not be complete if I did not mention a most pregnant little paragraph in Maxwell's treatise,* wherein that celebrated physicist proposed a form of influence machine (which, so far as I know, has never been made) having a device for avoiding the loss of energy that there necessarily is in the fact of internal sparks occurring

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* Maxwell's " Electricity and Magnetism," vol. i., art. 213, p. 298 (2nd edition).

in the machine. This he proposed to effect by adding certain supplemental conductors, which entirely obviated, at any rate in theory, the occurrence of internal sparks, and which therefore would add to the efficiency of the machine.

Many other individuals have described modifications in the form of the influence machine, and methods of performing experiments with. I have embodied in an appendix such references to these as have come under my notice.

Further, my summary of this subject would not be by any means complete if I did not make some reference to the more theoretical researches of Rossetti, of Righi, and of Kohlrausch and others upon the yield of these machines, and upon their internal resistance. The electromotive force of any given one of these machines is practically constant, no matter what their velocity; but the internal resistance decreases as the velocity increases, once more showing that a moving charge of electricity on a succession of moving charges of electricity is the equivalent of a current. This is a topic which one might with advantage pursue a good deal further.

Kohlrausch, using a Holtz machine with armature 16 inches in diameter, connected by a wetted string with a fine-wire galvanometer, showed* that the quantity of current furnished was proportional to the speed, and was independent of the breadth of gap between the discharging comb and the rotating disc. The current was so feeble that in 40 hours only 1 cubic centimetre of mixed gas would be produced by the electrolysis of water. The current was little affected by atmospheric conditions, but the spark-length that the machine would yield was greatly affected.

Rossetti,† who examined the question even more carefully, used one of Ruhmkorff's double Holtz machines driven by a descending weight. The speed was accurately counted by an electromagnetic counter; a galvanometer and a liquid rheostat

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* *Pogg. Ann.*, cxxxv., 120, 1868; *Ann. Chim. Phys.* [4], xvi., 484.

† *Nuovo Cimento* [2], xii., 89, 177, 205, 1874, and xiv., 5, 1875; *Pogg. Ann.*, cliv., 507, 1875; *Atti d. R. Instituto Veneto di Scienze Lettere ed Arti* [4], iii., 1872, 2159; an abstract of the latter, by its author, in *Journal de Physique*, iv., 65, 1875 *Ann. Chim. Phys.* [5], iv., 214.

were used to measure the current. The results were as follows :—
(1) The current is nearly, but not exactly, proportional to the
speed : it increases a little more rapidly ; (2) the work expended,
above that spent on mere friction, is exactly proportional to the
strength of the current ; (3) the effective turning moment is
constant, whatever the strength of the current ; (4) the ratio
between velocity and current increases with the humidity of the
surrounding air ; (5) the ratio between the work spent and the
current produced diminishes as the humidity increases ; (6) the
effective turning moment is greater on dry days, less on wet ;
(7) with increasing distance between the two plates the output is
less, and the effective work spent is also less (in proportion to
friction) ; (8) the electromotive force and internal resistance are
constant if speed and hygrometric state are constant ; (9) the
electromotive force is independent of speed ; (10) it diminishes
if the humidity increases ; (11) the internal resistance is in-
dependent of the hygrometric state ; (12) when the speed is
increased, the internal resistance is more than proportionally
decreased ; (13) the effective turning moments are proportional
to the electromotive forces. The electromotive force was about
55,000 volts. At two revolutions per second the internal resist-
ance was 2,550 meghohms ; at eight revolutions, 538 megohms.

Mascart* has compared the output of several kinds of influence
machines with that of other sorts of electrical machines, and
found the double Holtz machine (Ruhmkorff pattern) far
superior to any friction machine, but considerably inferior to an
induction coil.

Roíti,† who re-examined the questions raised by Rossetti, using
an electrometer method, arrives at the contradictory conclusion
that the electromotive force is proportional to the speed, and that
the internal resistance is independent of the speed.

Bouchotte‡ measured the actual work imparted to the moving
disc of a Holtz machine by the device of balancing the fixed plate

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* See Mascart's " Traité d'Électricité Statique," ii., 322.

† *Nuovo Cimento* [3], iii., 163, and iv., 79, 1878 ; see abstracts in *Beiblätter*, ii.,
416 and 709, 1878.

‡ *Comptes Rendus*, lxx., 933, 1870 ; see also Du Moncel's " Exposé " (3rd
edition), ii., 276.

in such a way that the turning moment (due to its reaction on the moving plate) was counterpoised by a known variable turning moment applied in the contrary direction. He found when the machine was running at 622 revolutions per minute, and was giving in that time 106 sparks, each 4 millimetres long, that the work actually expended in the minute was 944·8 gramme-metres. At 279 revolutions, giving 48 sparks, the work spent was 423·8 gramme-metres. The conclusion is that the quantity of electricity delivered by the machine (with electromotive force thus limited by the spark-length), and the work spent, were both proportional to the speed.

Time does not permit me to add more than a brief reference to the experiments made by Christiansen,* by Poggendorff,† by Holtz‡ himself, and by Gruel,§ on the reversibility of the functions of the machine, how it can be made to act as a motor, receiving currents from another machine and revolving. I showed this experiment at the Society of Arts in 1881; it is a more or less familiar illustration of the principle of reversibility in the transformations of energy. The tourniquet of Ruhmkorff‖ may be considered as a reversed influence machine.

My account would not be complete if I did not refer to two of the commercial applications of this very interesting scientific instrument. I mean first of all the suggestion of Professor Lodge¶ that such instruments should be used for collecting dust, smoke, and fog, and in that way dissipating them; and, secondly, the gas-lighters which have been made on this very principle. I presume that was not in Nicholson's idea, inasmuch as there was no gas to light in his days. Some years ago Mr. Molison** took out a patent for a form of influence machine, in which he proposed a fixed disc and a movable disc, the movable disc being weighted so that you could swirl it round; but this

* *Pogg. Ann.* cxxxvii., 490, 1869.

† *Ibid.*, cxxxix., 173, 1870.

‡ *Ibid.*, cxxxix., 513, 1870.

§ *Ibid.*, cxliv., 644, 1871.

‖ See Mascart's "Traité d'Électricité Statique," i., 179.

¶ See Address on "Dust" to British Association (Montreal Meeting), 1884.

** Specifications of Patents Nos. 1294 and 5036 of 1883.

weight in the more modern machine has been replaced by a little
spring trigger, which actuates a driving gear. There is one of them
here opened to show how the trigger sets the disc in rotation.
Then even before Molison's had come by any means to its present
efficient form, Clarke* had produced a form of gas-lighter with
which many of us are familiar (Fig. 32). The internal structure
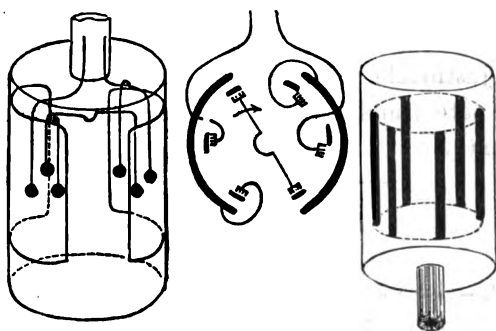
FIG. 32.—Clarke's Gas-Lighter.

of this is, I may say, almost identical with that of Sir William
Thomson's replenisher.   There are two tinfoil field-plates, and
there is a rotating cylinder of ebonite, which has eight pieces of
metal foil upon it to serve as carriers. The essential parts are
these two field-plates, the set of carriers on the rotating armature,
a pair of neutralising brushes fixed to a cross conductor, and a
pair of appropriating brushes that take the induced charges as
they come over and give them to the field-plates, and maintain
their charges. Last of all, there are a pair of discharging wires,
connected through the stem of the instrument to the sparking tip.

Here I terminate my review of the progress made in this
department of science since 1788. If it be true that as yet the
influence machine (save in these two minor applications) has not
yet proved to be of service to mankind at large, that is yet no
reason for neglecting the study of so admirable an apparatus.
Though Faraday discovered the fundamental principle of all
dynamo-electric machines in 1831, it was not until about 1877,
mainly in consequence of the improvements in details of Gramme,
that the world at large realised the commercial significance. I

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* Specification of Patent No. 5992 of 1883; see also *Electrical Review*, xv.,
146, 1885.

have shown you to-night that the modern influence machine is
not only self-exciting, but does not lose or reverse its charges
while in action. It is not, like the older machines, dependent
on atmospheric caprices; not one of my machines to-night has
been warmed or even artificially dried. If commercial applica-
tions do not come about, it will not be because the modern
influence machine lacks reliability. And, indeed, whilst we
celebrate the centenary of the first of influence machines, we
may take a justifiable pride in reflecting that not only the first,
but also the latest and best, of these appliances is a product of
our own country.

APPENDIX.

ATKINSON, P.—See his book on "Static Electricity" (New York,
1886), p. 110. His machine, a modification of Toepler's,
embodied certain improvements, subject of two U.S.
patents of April 10, 1883, and December 8, 1885—
(1) Nickel-plated outer coating of Leyden jars, into which
the jars themselves slip; (2) takes current for shocks in the
connecting circuit between jars, putting a switch across to
short-circuit the handles when not in use; (3) the appro-
priating brushes are carried through a hole drilled near edge
of the fixed plate; (4) the diagonal conductor rigidly fixed
at 45°.

BERNARDI.—*Nuovo Cim.* [2], iv., 337. On Holtz machine details;
discusses effect of cutting down fixed plate.

BERTIN, A.—*Ann. Chim. Phys.* [4], xiii., 190. Mostly a transla-
tion of Riess's paper in *Pogg. Ann.*, cxxxi., 215. Into this
paper, however, Bertin introduces the very valuable dia-
grammatic method of depicting influence machines as
though the parts were arranged as cylindrically concentric.

BERTSCH.—Described a machine working partly by influence,
which was subject of some controversy at the time with
M. de Parville. See *Comptes Rendus*, lxiii., 771, Nov., 1866;
Les Mondes, xii., 480, 492, 539, and 662; xv., 372 and 659.
See also *Carl's Repertorium*, iii., 229.

BLEEKRODE.—*Pogg. Ann.*, clvi., 283, 1875. Suggested an ebonite disc, which might be rubbed to give initial charge. Also used as diagonal conductor a tube with two gas flames at ends.

BOUTY.—*Journal de Physique,* iv., 135, 1875. A criticism on the calculations of Rossetti.

CARL, P.—*Carl's Repertorium,* iv., 106, 1868; v., 279, 375, 1869. Observed effects of atmospheric conditions on a Holtz machine during 73 different days, on three of which it refused to excite itself. In 200 different experiments the sparks were on 145 occasions over 10 centimetres long, on 41 occasions between 5 and 9·1 centimetres, on 14 occasions less than 4·5 centimetres. Also mentions that Ruhmkorff used radiation of a petroleum lamp to keep plates dry; also that Kirchhoff was the first to put a Holtz machine inside a (glass) case with chloride of calcium to desiccate.

CHESTER, C. T.—See *Morton.*

DEMOGET.—Dispute with Carré. See *Comptes Rendus,* Jan., 1869.

ELSTER AND GEITEL.—*Wied. Ann.,* xxv., 114, 1885. Describe a simple water-dropping apparatus. The same authors (*op. cit.,* p. 493 (describe a simple influence machine identical with Belli's described above.

FERRINI, R.—*Nuovo Cimento* [2], x., 49, 1873. Discusses luminous phenomena at points, and the action of the diagonal conductor.

  Inst. Lomb. Rendiconti, vi., 286, 326, 1873. Deals with inversions of the charge in a Holtz machine with horizontal plates.

FULLER, G.—*Proc. Physical Society of London,* ii., 83, 1876. Describes a double-acting electrophorus worked by cranks, having certain points of resemblance to Darwin's apparatus.

GAMAULT.—See "Étude sur l'Exposition de 1867," par Lacroix, vi., 351. Includes a report on Holtz and Toepler machines.

GAUGAIN, J.-M.—Wrote abstract, in French, of Toepler's paper in *Pogg. Ann.,* cxxv., 1865.

GIRARBON.—*Les Mondes,* xix., 358. His machine same as Carré,

but alleged of date 1854. See Du Moncel's "·Exposé des "Applications," 2nd edition, vol. i., p. 400; 3rd edition, vol. ii., p. 285.

GLÄSER.—See *Lewandowski*.

HEMPEL.—*Comptes Rendus*, lxii., 58, 1866. Says that small slits will not do as substitutes for ·the windows in the Holtz machine. Questions whether nitric acid is formed by action of machine. Also discusses theory of action.

HEMPEL, W.—*Berichte der Deutsch. Chem. Gesellschaft*, xvii., 145, 1884. Discusses effect, on output of a Toepler machine, of chemical nature and pressure of surrounding gas. Found in hydrogen only 9 discharges, as against 45 in air at same pressure, with same speed. Found that in air an addition of the atmosphere to the ordinary pressure sent up the number of discharges from 15 to 32, while reduction to half atmosphere stopped the working of the machine altogether.

HUMBLOT.—*La Lumière Électrique*, vi., 86.

KAISER.—*Les Mondes*, xx., 665, and xxi., 256. Constructed a double Holtz machine, without diagonal conductor, and found its output more than twice that of single disc. See also *Bleekrode*.

KUNDT, A.—*Pogg. Ann.*, cxxxv., 484, 1868. Prevents reversals by adding an auxiliary friction machine.

LABORDE.—*Les Mondes*, xxiii., 775. Merely describes some experiments with Holtz machine.

LEBLANC.—*La Lumière Electrique*, iv., 72, 1881, "Nouvelle "Machine Cineto-électrique." A peculiar species of influence machine, having very short insulation, and a commutator like that of a dynamo machine.

LEUNER, O.—Exhibited at the Vienna Exhibition of 1883 a Toepler machine with 20, and another with 30 rotating plates, and artificial heating appliances. See Wallentin's "Die Generatoren hochgespannter Elektricität" (Vienna, 1884), p. 163.

LEWANDOWSKI.—*Zeitschrift für Elektrotechnik*, v., 215, 1888. Article on Gläser's machine, a form intermediate between Holtz's and Winter's.

LEYSER.—Constructed in 1873 a peculiar kind of Holtz machine, a figure of which is given in Wallentin, *op. cit.*, p. 117.

LOMMEL, E.—*Wied. Ann.*, xxv., 678, 1885. Suggests improving Holtz machine by not cutting windows, but bringing round an arm to front of rotating disc. See Holtz's own suggestion in *Mitth. d. naturw. Ver. f. Neuvorpommern u. Rügen,* xi., 72, 1879.

MORTON, H.—*Journal of Franklin Institute*, liii., 119, 1867. Morton's pattern of Holtz machine as constructed by C. T. Chester is described at p. 253, and as constructed by E. S. Ritchie at p. 345 of same volume.

NEYRENEUF.—*Ann. Chim. Phys.* [5], v., 397, 1875. Discusses conditions of reversals, &c.

PICHE.—Devised machine of same kind as Bertsch. See correspondence with Bertsch.

PIERUZZI.—*Nuovo Cimento* [3], xvi., 131, 185, 1876. Discusses reversals in Holtz machines of the second order.

POUCHKOFF.—*La Lumière Électrique*, x., 477, 1883. Suggested as modification of Holtz, to obviate windows, affixing a paper band over edge of fixed plate.

PROVENZALI.—*Atti Nuov. Lincei*, xxiv., 271, 1871, and xxv., 371, 1872, " Sulle Macchine Elettriche ad Influenza."

RIGHI.—*Mem. di Bologna* [3], vi., 87, 1875. A general theory of Holtz machine. Also see his paper, cited above, on Volta's principle, in *Nuovo Cimento*, vii. and viii., 123, 1872.

RIECKE.—*Wied. Ann.*, xiii., 255, 1881. Researches on the output of machines of the second order.

ROSSETTI, F.—In addition to memoirs cited above, see *Nuovo Cimento* [2], ii., 5, 1884; v. and vi., 407, and vii. and viii., 22, 1875.

SCHLÖSSER.—*Pogg. Ann.*, clvi., 496, 1875. Suggested ebonite disc instead of glass.

SCHWANDA.—*Pogg. Ann.*, cxxxiii., 18 . Suggests electro-medical use of discharge of Holtz machines.

VAN DER WILLIGEN.—" Een Paar Opmerkingen betreffende de " Elektriseermachine van Holtz " (Amsterdam Acad., 1870).

WEINHOLD, A.—*Zeitschrift für dem Physik. u. Chem. Unterricht,*

i., 8, 1887.   Describes a special form of influence machine
having one plate (rotating), and fixed inductors of wood,
with special object of preventing reversals of polarity.

ZENGER, K. W.—Describes a method of adding to an ordinary
friction machine a disc with carriers to work by influence.
See Zenger's "Die Spannungs-elektricität" (Vienna, 1884),
p. 149.

Mr.
Wimshurst.

Mr. WIMSHURST [on the invitation of the President]: I feel
that it would be almost out of place for me to say anything after
the able manner in which Professor Thompson has dealt with
this subject.   I would rather, with your permission, listen to the
remarks of others, and if there is any special point then on which
I can offer a few remarks I shall be very glad.

Professor
Ayrton.

Professor AYRTON: I am sure we have all listened with the
greatest interest to this charming historical account we have had
given us.   I think there is nobody—certainly no other English-
man—who could have given us so complete an historical record of
the development of the influence machine as the author of this
paper.   It is certainly on a par with his paper given some short
time ago to the Society of Arts, in which he dealt in the
same complete way with the development of another apparatus of
great value to electrical engineers, viz., the vacuum pump.

There is one peculiarity in this Darwinian development of a
Darwinian idea which I do not think Dr. Thompson has quite
sufficiently laid weight on, and that is, that instead of one machine
developing out of the other—which, of course, is the general idea of
progress—the subject has been re-invented over and over again,
entirely *de novo*, without any knowledge apparently existing in
the inventors' minds that anything had been done with influence
machines before their time.   I took occasion at the last meeting
to speak of the marvellous inventive genius of the author of the
paper given on that occasion—Sir William Thomson—and I think,
perhaps, that the members who listened to him on the last
occasion may perhaps have failed to discover this evening that
the same genius was really applied by him in devising a machine
which apparently is exactly the same as that of Belli.   Dr.

Thompson is right, I believe, in saying that every feature possessed **Professor Ayrton.**
by the well-known Thomson's replenisher is to be found in the
Belli machine; so that it would not, I am sure, be understood
what great merit was due to Thomson for having in 1867 created
an instrument which had already been made in 1831—36 years
previously.   But as I happened to be with Sir William Thomson
in Glasgow in 1867, I knew the history of the development of the
Thomson replenisher.   It was in no sense—odd as it seems—a
development of any known machine well known at that day.
Why nobody seemed to know what had been done on the subject
of influence machines is certainly very puzzling, but that was the
state of things.   Sir William Thomson was certainly not aware
that any such influence machine existed in 1867 as have been
described to us to-night.   He wanted to devise a machine which
has had, I think, a greater practical use than up to the present
date has any " gas-lighter," and also has led to greater practical
results than the most ingenious suggestion of Professor Lodge,
whatever may be the ultimate result to mankind evolved from
Dr. Lodge's experiments and suggestions.   Sir William desired
to construct a practicable recording instrument for submarine
cables, and he was therefore led to make that practicable appa-
ratus with which we are all acquainted, the siphon recorder,
which has had a long career of commercial importance down
to the present day.   He had, as you know, to electrify the
ink.   Now he said at the time: "I cannot bear the idea of
" employing labour — ' coolies,' in fact — to grind frictional
" machines all day and all night for electrifying the ink; therefore
" it is necessary to devise some form of apparatus to produce the
" requisite amount of electrical energy in a far more economical
" way; in fact, a machine must be devised having a far higher
" efficiency than a frictional electrical machine."   He started with-
out any knowledge that anything important had been done in
the subject, and after many trials he developed his well-known
replenisher, which now appears to be almost exactly like the
Belli's machine of 1831.   Finding that his mouse mill was so
useful for commercial purposes, he proposed to secure its use, and
he asked Mr. Bottomley and myself to go and consult the Patent

Professor Ayrton. Office records and see if by any remote chance anybody had ever done anything of the kind before.   Although Mr. Varley was not only a great friend of Sir William Thomson, but was practically a partner of his, so little importance had been attached to Varley's patent of 1860, and to any use Varley may have made of the contrivances   described   in   that   patent   specification,   that Thomson was not even aware that Varley had done anything in the matter, and it was quite to our astonishment, and to Sir William's, that we came across Varley's patent taken out seven years before that time.   Then Sir William saw that although he had developed, and, in fact, originated, his machine without an acquaintance with the history of the subject, he had really re-invented a machine which had been previously described by Varley.

There is one point brought out by the author of the paper to-night which seems new to me.   I did not remember that Varley proposed to use any springs to make contact with the moving carriers.   I was under the impression (of course I have not the patent before me, and it is twenty years since I consulted it) that Varley proposed to make contact by a brush discharge, so that his machine would not be self-exciting.

Professor Thompson. Professor THOMPSON: This is what the patent says: " When " I use this machine as an electrometer . . . I put spring " connections to h h_1 and g g_1; then on giving a charge to f, no " matter how feeble, and rotating the machine, the charge will " be augmented till it is of sufficient force to be measured."

Professor Ayrton. Professor AYRTON : That only shows that Varley was a cleverer man than we thought, and that the mode of making an influence machine   that   would   excite   itself   was   actually   put   forward   by Varley.

One question I would like to ask the author or Mr. Wims-hurst.   Dr. Thompson said if you used brushes for the conductors and combs for the discharging apparatus (which I think are better called " inductors "), the machine does not reverse.   My experience is that it does.   I should like to know why it reverses so extremely easily.   I do not think the combs get over that difficulty.   It was the great difficulty that Thomson had

Professor Ayrton.

originally in his form of machine in 1867.. That used to reverse, and I remember he made a suggestion to Mr. White, his instrument maker, for automatically disconnecting certain contacts, to prevent reversal. However, the Wimshurst machine— I speak rather of the machine without Leyden jars—seems certainly to reverse. Is it easy to avoid that in any way without the use of Leyden jars ?

There is another point I would like to ask about, and that is, why the machines as made by instrument makers are very often so extremely inferior to those made by Mr. Wimshurst himself. We bought a machine some time ago from a certain maker who advertised that he makes the machines. Well, it would not work at all. Then Mr. Wimshurst most kindly gave us two glass plates made by himself, and the machine worked well. Unfortunately one of Mr. Wimshurst's plates was afterwards broken, and, as we did not like to trouble Mr. Wimshurst again, we replaced it by one supplied by the instrument makers, which they said was of far better quality than those they originally supplied. The machine has therefore now one of Mr. Wimshurst's plates and a so-called "superior" plate supplied by the makers. But it still has some of the defects put into it, I suppose, by the instrument makers, because at times it will not work at all ; and one of my assistants, who spends his leisure in looking after this machine, says that by "doing some-"thing to it "—he is never quite sure what—he can make it sometimes work. I do not think that the fault can lie in the ebonite pillars, because a charming little machine, kindly lent me by Mr. Wimshurst some time ago, also has ebonite pillars, but this one works extremely well.

Just to sum up. Professor Thompson spoke of experiments that were made as to the efficiency of various machines. It is most interesting to consider such experiments. I remember in 1867 making experiments in Sir William Thomson's laboratory on the relative efficiency of a plate-glass machine and Sir William Thomson's replenisher. It is not necessary to go into figures to see the enormously superior efficiency of the influence machine as compared with a frictional machine. For, while it is difficult

Professor
Ayrton.

to make the plate, or cylinder, of a frictional machine revolve rapidly even while you are working hard turning the handle, you must all have noticed that long after Mr. Wimshurst had ceased working his machine—in fact, while he was walking away from it to his seat—the plates continued to revolve at a considerable velocity, and lightning flashes continued to rapidly pass between the knobs.

Mr. Mordey.

Mr. MORDEY pointed out that, according to Professor Thompson's diagram of the action of a Wimshurst machine, a certain number of plates were maintained in an unaltered condition through a considerable angle, and asked whether it would not be possible either to increase the effect produced in some way, or to reduce the diameter of the revolving plates for the same effect.

Professor
Hughes.

Professor HUGHES : I should like to ask just one question of Professor Thompson. In your list I find that you left out the name of Mr. Bertsch, who in 1865 had a very interesting machine, which he brought out in France, and as I was present at all his experiments I think I ought to mention his name in connection with this subject.

Mr. C. E.
Grove.

Mr. C. E. GROVE : I should like to add one word to what Professor Ayrton has said about these machines reversing. Professor Thompson told us that the Voss machine hardly ever reverses. So far as I have been able to judge (and I have had the opportunity of working with several of them), the Voss is one of the worst machines for reversing. This tendency to reverse appears to depend not merely upon the relation between the distribution of the combs and brushes, but also upon the position in which the diagonal conductor is put—a point on which nothing has been said yet this evening.

Mr. A. J.
Walter.

Mr. A. J. WALTER : There is one question I should like to ask Professor Thompson—why on the diagram indicating the distribution of electricity upon the surfaces of the plates of the Wimshurst machine no charge is depicted either on the inner surfaces of the plates opposite the points of the prime conductors, or upon the outer surfaces of the plates at the top and bottom of the diagram.

Is it that on those portions of the plates there is no electrical

charge? or is it that there is no free charge which can be
collected at those points?

Professor THOMPSON: My diagram is made to represent what
one finds at the different parts of the machine. At the top and
bottom you find that the charges are of opposite sign, and there-
fore attract one another towards the glass between, and you have
next to no other charge on the outside; whereas right and left, on
the one side positive and on the other negative, you have plates
with charges of the same sign and absolutely equal in amount.
The consequence is, they repel themselves from the glass, so that
you have all the charge virtually there. Perhaps when my paper
is read in detail it will be more clear.

In reply to the question about the Voss machine. I did not
intend to say that the Voss machine could not reverse itself.
Many Voss machines had brushes where there ought to be combs
only. That of itself is quite enough to make a machine unreliable
from that point of view. Also, I do not like Voss's arrangement
of tinfoil behind the field-plates. It seems to me rather a mistake
in construction. I take it that the Voss machine is simply a sort
of intermediate form which will sooner or later die out, and
that it has made its way because the makers saw that a machine
was wanted and could be made at a reasonable price. I have
certainly found Mr. Wimshurst's machines much less liable to
reverse. In fact, with his eight-plate machine the difficulty is to
get it to reverse. You cannot do it without trouble. Take a single
discharge to earth out from either side: that does not reverse
it. Turn it rapidly backwards, then forwards: it does not reverse
itself as does the Holtz machine. The only way to make it
reverse is this: You first charge a Leyden jar from one of the
discharging knobs; then discharge the machine by putting the
discharging rods together and turning the handle the wrong way;
then open out the discharging apparatus and communicate the
charge of your jar to the knob at the other side. You thus
literally do reverse the charge.

Professor AYRTON: Perhaps as a matter of capacity the
larger machines do not reverse. If the capacity is small, then
the discharge is reversed very frequently. The small machines

Professor Thompson. constantly reverse—I mean the two-plate machine; it has brushes for collectors and combs for inductors.

Professor THOMPSON: Has it numerous carriers, or a few?

Professor AYRTON: A great number. It excites itself quite easily.

Professor THOMPSON: With respect to the position of the diagonal conductor, the liability to reverse in Voss's machine is certainly greater when the diagonal conductor is more nearly horizontal. It is not, however, in my own mind very clear what the instability depends upon.

Professor Hughes asked me about Monsieur Bertsch. In the list of names hung upon the wall I have only given the principal. Reference to Bertsch's investigations will be found in the appendix to my paper.

Professor HUGHES: M. Bertsch wrote several papers in the *Comptes Rendus*.

Professor THOMPSON: There was a controversy in the *Comptes Rendus* between himself and Monsieur Piche.

Mr. Wimshurst. Mr. WIMSHURST: There are only two points raised by Professor Ayrton to which I need refer. The first is this: He asks why makers' instruments fail in certain points. Well, that certainly is too indefinite for me to answer. I should need to examine the instruments and find their faults before answering so general a question. The other point I am astonished to hear about—that is, the reversal of the current; for all through my experience, running over many years, I have never found one such case. There is no difference in behaviour between small and large machines. The machine standing near the table shall be the property of any person who can make it reverse, even by practising with it the whole evening. The small machine which I lent Professor Ayrton a short time since, having 6-inch glasses, did not reverse while it was in my possession. Therefore there must have been some alteration made in it causing the change in its behaviour: possibly the points of the collecting cones have become bent from their proper position. I will, however, examine it and point out any defects.

Let me, gentlemen, now thank you very sincerely for the

kind manner in which you have received my very humble Mr. Wimshurst.
doings.

Professor THOMPSON: I find that I did not answer Mr. Professor Thompson.
Mordey's question : it escaped my memory. He asked me why
it is that over the top and bottom of the machine the series of
carriers remains along a considerable arc in the same state of
charge.   Well, I believe it is because in between these two points
of that range nothing is done to them ; they are simply moving
past one another in precisely similar condition.   It is no gain to
shift the two sets of neutralising brushes nearer to one another
so as to diminish that, because what you want to have in a
machine of that kind is the most complete and absolute insulation
between the parts that are of opposite sign.   Moreover, each
neutralising brush ought to touch the carriers at a point where
they are under strong influence from the opposite set of carriers.
This is a necessary part of the action of the machine.   It would
be no advantage whatever to shift these interlacing brushes
towards one another unless one could in some way lengthen
the distance between the charges of opposite sign on the same
face.   The length of spark you can get out of machines of this
kind is practically limited to the distance across the plate, due
to the insulating spaces between carrier and carrier.

I cannot conclude my reply without saying that although it
has been a great pleasure to me to bring this subject before the
Society, it is much more due to Mr. Wimshurst than to myself
that this exhibition of apparatus is brought together.   I have
relied upon his kind offices in being able to show you to-night
these, the latest and the best of influence machines.

The PRESIDENT : I am sure you will all agree with me that The President.
the very lucid and interesting communication to which we have
listened is a striking proof of careful and exhaustive research, and
deserves a most hearty vote of thanks.   I beg, therefore, to move
that the cordial acknowledgments of the Society be presented to
Professor Silvanus Thompson for the discourse he has been kind
enough to give us, coupled with the name of Mr. Wimshurst,
who has furnished most of the interesting collection of apparatus
before you.

A ballot for new members took place, at which the following candidates were elected :—

Foreign Members :

Osuke Asano.
Daizaburo Awoki.
Senkichi Kanda.
Kotaro Morishima.
Nobuyoshi Nakayama.
Rentaro Nagayama.

Choyo Okazaki.
R. v. Picou.
O. B. Shallenberger.
Bentaro Tamaki.
Makoto Tsuboi.
Teruwo Tedzuka.

Associates :

Walter Blenkaru.
James Samuel Burroughes.
Lord Edward Churchill.
Patrick D'Aranjo.
Tom Charles Ekin.
Claud Hamilton.
Robert Hammond.

Edward Jennings Jennings.
Alfred Mavor.
John Pollitt.
Ernest Lee Simpson.
Alan Winslow Stewart.
Joseph William Swithinbank.
Benjamin Verity, jun.

Students :

Herbert Cecil Hodges.
Thomas L. Horn.

Edwin J. Selby.
Henry Edward Irving Taylor.

James Murray Young, jun.

The meeting then adjourned.

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ABSTRACTS.

T. H. BLAKESLEY—A METHOD OF DETERMINING THE DIFFERENCE BETWEEN THE PHASE OF TWO HARMONIC CURRENTS OF ELECTRICITY HAVING THE SAME PERIOD.

(Philosophical Magazine, Vol. 25, April, 1888, pp. 295, 296.)

The author desires to place on record his prior claim over Ferraris to the method of measuring the difference in phase of two harmonic currents by means of electro-dynamometers, the method having been first published by him in the *Electrician* of the 2nd October, 1885.

When the two coils of an electro-dynamometer connected in series in the usual way are traversed by a current, the instrument will measure $\frac{I^2}{2}$. If the two coils be placed in different circuits, then the instrument will measure $\frac{I_1 I_2}{2}$ cos. θ, where θ is the phase difference of the two currents.

From three successive readings of the same instrument—or, better, from three simultaneous readings of three different instruments—the constants of which are accurately known, it is thus possible to obtain three equations, the solution of which gives the value of θ.

RIGHI—ELECTRIC PHENOMENA PRODUCED BY RADIATIONS.

(Journal de Physique, Vol. 7, April, 1888, pp. 153–55.)

Two plates, one of solid metal, one of wire gauze, are arranged parallel to each other; the solid disc is connected to one pair of quadrants of an electrometer, the wire disc to the other pair of quadrants and to earth; the aluminium needle is kept at a constant potential. If the solid disc is put to earth momentarily and then light thrown on it, a deflection is produced which gradually increases to a maximum, which is more quickly reached if the radiant source is near and the surface of the plates large. If the two plates are very close together, and are then suddenly separated, the deflection does not change, showing that the radiations have reduced the two plates to the same potential. This deflection, therefore, gives in absolute measure the contact potential difference of the two metals of which the plates are made.

Sunlight does not produce these results; the flame of burning magnesium is better; and an arc light better still. Even the effect of the arc light may be increased by making one of the electrodes a metal, as, for instance, zinc. A plate of glass interposed in the path of the rays stops the action almost

entirely; while it may be assisted by making use of a quartz lens to concentrate the rays.

By arranging four "photo-electric" couples in series, the same electrostatic phenomena can be observed as with a battery on open circuit. The gauze plate may be dispensed with, and a very slow deflection obtained by allowing light to fall on the solid plate only, after it has been put to earth for a moment. By covering the solid plate with selenium it may be observed that the latter is more electro-negative than gas coke, and that it behaves like carbon when it forms a photo-electric couple with a metal. The experiments would seem to point to a sort of electric convection produced by the radiation, starting from the bodies on which probably there is a negative charge due to contact potential difference, and going towards those bodies on which there is a positive charge similarly produced.

SÉLIGMANN-LUI—TRANSMISSION ON SUBMARINE CABLES.

The paper has more especial reference to the method of transmission by means of telephones proposed by Mr. Ader, a description of which has already been given in an abstract (see *Journal*, vol. xvii., p. 470); but the considerations adduced by the author as pointing to the desirability of facilitating transmission in all ways are worthy of note.

The speed of transmission on submarine cables varies inversely as the capacity and the resistance of the conductor; hence, when once the proportion of insulation to conductor has been settled, the speed will increase in proportion to the section of the conductor. But this increase in size means a largely increased cost. The cost of the materials for a knot of cable, having a core composed of a conductor weighing 107 pounds to the knot and insulated with gutta-percha weighing 140 pounds per knot, will be about £56. To this must be added £12 for labour, and from £25 to £32 for supervision, testing, interest on plant, and profit. Omitting this last figure, which will not vary greatly for various cables, about 82 per cent. of the cost is absorbed by the price of the materials used, and it is this large fraction of the total cost which would have to be increased in proportion to the square of the diameter of the conductor if a larger cable were used.

The above considerations refer only to the manufacture of the cable; but when the laying of it is also taken into account, it is easy to see how even a relatively small increase in size would lead to a very great increase in cost. Not only would the laying machinery have to be increased, but it would probably be found necessary to make several voyages owing to the impossibility of coiling long sections of the cable on board the cable ship.

Such considerations have led to a reduction in the external diameter of the cable, whilst the relative weight of the conductor has been increased. This may be seen from the following tabulated statement:—

Date.	Cable.						Copper, lbs. per knot.	Gutta-Percha, lbs. per knot.
1865–66	...	...	...	...	...	...	264	396
1869–75	...	...	...	...	...	...	396	396
1874 ...	Atlantic ...	...	...	...	...		396	396
1874 ...	St. Vincent—Pernambuco		...	...			253	336
1879 ...	Aden—Zanzibar		...	...	...		250	250
1880 ...	Java—Australia		...	...	...		105	140
1880 ...	Atlantic	...	...	...	...		297	297

From calculation and experience it appears that the time necessary for the current at the receiving end to attain the desired intensity is—

For Morse instruments ...   . $414/10^9 \times C R$ seconds;

,, Hughes   ,, ... $105/10^9 \times C R$,,

,, mirror and syphon recorder   ... $47/10^9 \times C R$,,

(where C = capacity of the cable and R = copper resistance).

An apparatus which is very sensitive, and therefore answers at once to a very weak current, is for that reason some time before it comes back to rest, as it is some time before the current falls to so low a value as not to affect the instrument. This difficulty can, however, be got over by making successive emissions to line vary in duration, in direction, or in strength. The following details will give some idea of the advantage of such a system:—The Jay Gould cable from Penzance to Canso has a resistance of 8,320 ohms and a capacity of 939 microfarads. The ordinary speed is 18 words of 5 letters per minute, which, taking an average of 4 signals per letter, gives for the time of each signal and the succeeding interval 0·166 second. Supposing that a battery giving an E.M.F. of 20 volts is used, the intensity of the permanent current would be 2·4 milliampères; and if the sending current lasts for 0 115 second, the arriving current will have a maximum value of less than one-tenth, which will only be reached 0·92 second after the current starts. Supposing also that a current of 0·015 milliampère is necessary to affect the receiving instrument, the signal will be prolonged at least 0·35 second after the maximum; consequently the time of each signal and the succeeding interval would be in this case 1·04 second, or six times what it is in practice.

The tendency is therefore to employ the more and more sensitive instruments. The telephone fulfils this requirement thoroughly. Mr. Cross, of Philadelphia, has found that with a Bell telephone the following currents are necessary:—

For the letter o ...   .   0·000260 ampère;

,,   ,, a ...   .   0·000123   ,,

,,   ,, i ...   .   0·000103   ,,

Dr. Wietlisbach has calculated that the maximum current necessary to work a Siemens telephone is 0·0001 ampère.

MASCART—MEASUREMENT OF ILLUMINATION.

The mere comparison of the intensities of two sources of light is not sufficient; what we really want to know is the comparison of the illumination produced by the two lights.

The author has invented a photometer to effect this comparison. The standard lamp illuminates a plate of ground glass, an image of which is formed by a lens, and is thrown, after undergoing two reflections at an angle of 45 degrees, on one half of a disc of ground glass, called the test glass. The general diffused light of the room to be tested illuminates a translucid screen, the rays emitted from which are also reflected at an angle of 45 degrees, and then fall on the other half of the test glass. The light from either source can be more or less cut off by sectors. The standard lamp is of the moderator type, and is compared with a Carcel lamp at a distance of one mètre from the screen.

In lighting similar rooms of different sizes it would appear in the first instance that the sources of light ought to vary in intensity proportionally to the squares of homologous dimensions; in practice, however, it is found that the quantity of light varies as the cubic contents of the rooms. A consideration of the limiting distance at which the illumination of a source of light ceases to be efficacious leads to an idea of mean illumination. Suppose that the limiting distance is ten mètres, and the mean illumination that of one carcel at a distance of one mètre, then the illumination ought to be 0·16 candle per cubic mètre.

A comparison of the amount of illumination of public buildings extending over the present century leads to the inevitable conclusion that the public demand a much brighter illumination now than formerly, and that this increase of illumination has by no means yet reached its maximum.

A. STOLETOW—A KIND OF ELECTRIC CURRENTS PRODUCED BY THE ULTRA-VIOLET RAYS.

The experiments were carried out with a sort of condenser, one plate being entire, while the other was formed of wire gauze; the two plates were so arranged in front of an arc light that the solid plate was illuminated through the interstices of the other. A circuit was made of the above air-condenser, an astatic Thomson galvanometer, and a battery, the positive pole of which was connected to the wire gauze. As soon as the light from the arc is allowed to fall on the plates, the galvanometer needle is deflected, proving that a current traverses the air gap. Any screen which cuts off the light prevents the deflection; a glass screen has the same effect; a quartz plate has very little effect. On reversing the poles of the battery the deflection obtained was far less.

The effect of the ultra-violet rays is apparently to give the air a sort of

unipolar conductivity which allows of the passage of the negative charge. The following figures give an idea of the magnitude of the action, one division of the scale corresponding to 9×10^{-11} ampère; with the plates from 2 to 3 mm. apart, and the current of two Daniell cells, a deflection of 30 to 50 divisions was obtained. With 100 cells (zinc-water-copper), and the plates separated as widely as 100 mm., a current was observed. It appears that the current is proportional to the area of the illuminated plate; it also varies with the distance (l) between the plates, though not directly, but rather according to the law $i = \dfrac{E}{a + b\,l}$. The current increases more and more slowly the greater the E.M.F. used; the apparent resistance of the layer of air seems to increase as the E.M.F. is increased.

If plates of different metal are used, their own potential difference must be taken into account. This would lead one to infer that a current could be produced without an external source of electricity, and this was found to be the case. When a zinc plate pierced with holes was opposed to a solid silvered copper plate, and the light was thrown on them, a current was produced. By comparison of this current with that produced by one Daniell cell, the author calculated the potential difference of the two plates to be from 0·97 to 1·06 volt. The introduction of metals into the arc produces a great effect. Aluminium is the most active, then zinc and lead: these are all metals the spectrum of which is particularly rich in the violet; they are also the most highly positive metals in Volta's scale.

C. POLLAK—ARC LAMP REGULATED BY THE EXPANSION OF THE CONDUCTING WIRES.

The carbon-holders consist of two spiral springs of brass wire, to which are attached two stretched brass wires 0·45 mm. diameter, through which the current passes to the carbons; in so doing the wires are heated by the current. The carbons being at first in contact, as soon as the current is turned on, the stretched wires lengthen and pull the arc. As the carbons burn away, the resistance of the arc increases, less and less current passes, the stretched wires are not so much heated, they shorten, and the carbons are brought nearer together.

A. POEPPL—CONDUCTIVITY OF VACUUM.

The safest way to study the conductivity of any medium, independently of any secondary phenomena, is to form a closed circuit of the body under observation, and to watch the behaviour of a current set up in this circuit by means of induction. In putting this idea into practice in experimenting on the conductivity of vacuum, the author arranged his homogeneous closed circuit of two spiral glass tubes wound in the form of a solenoid. These two

solenoidal tubes were connected by cross tubes of glass, and could be exhausted to any desired degree by means of a Topler mercury pump. The axes of the two solenoids were at right angles to each other, and the vertical one occupied the centre of an ordinary coil of copper wire on a bobbin, so that the copper solenoid was the primary and the glass solenoid the secondary of an induction coil.

If now the vacuum had any degree of conductivity, when a current was sent through the primary copper bobbin, an induced current should have been set up in the vacuum tubes, and should have manifested itself by its action in the second of the two tubes, the axis of which was horizontal. Such manifestation of its presence might be by magnetic action, by the development of heat or of light. The last phenomenon is probably the most delicate, but its occurrence might be conveniently confirmed by the first; to this end a magnetic needle was suspended inside the coils of the second vacuum tube.

It was found that no current was induced in the vacuum tubes when the primary current, which in some instances was 1 ampère and in others as much as $22\frac{1}{2}$ ampères, was made and broken, as evidenced by the suspended needle not being deflected at all. If, however, the system of solenoidal vacuum tubes was replaced by solenoids of copper exactly similar in size and arrangement, the needle was deflected at once.

No glimmer of light was noticed in the vacuum tubes on making and breaking the primary circuit, although in some experiments the one solenoid was short-circuited by a short, straight tube.

From a calculation of the induced E.M.F., and from the known current, it would follow that the specific resistance of vacuum is 48,000 times greater than that of mercury. If the calculations are based on the amount of deflection of the suspended needle when the vacuum tubes were replaced by copper wires, the specific resistance comes out 4,400 times greater than that of copper. Either result is sufficient to show that vacuum should not be classed amongst the good conductors of electricity.

S. SHELDON—ALTERNATE CURRENTS AND ELECTROLYTES.

(Annalen der Physik und Chemie, Vol. 34, No. 5, 1888, pp. 122–38.)

There are two methods of experimenting on the resistance of liquids: first with continuous currents, when either the polarisation has to be taken into account or is supposed to remain constant during the experiment; secondly, with alternating currents. The former method has been regarded with some mistrust by physicists; although both Tollinger and Ostwald, who have employed both methods, have obtained results which, on comparison, were found to be very concordant.

The author has undertaken a series of experiments on potassium chloride sodium chloride, potassium sulphate, and magnesium sulphate—nine different degrees of concentration being used in each case. Before passing on to a description of his experiments with alternate currents, the author mentions an arrangement for experimenting by means of continuous currents.

The circuit contains, besides the source of electricity, a variable resistance and the electrolyte, this latter in a vessel of special form and provided with two pairs of electrodes; the one pair served in the usual way for the passage of the current. The second pair of electrodes were joined respectively to two adjacent quadrants of an electrometer, while the other two adjacent quadrants were joined respectively to the extremities of the resistance in circuit. This arrangement of a differential electrometer gives a means of comparing the resistance of the electrolyte with that of the rheostat.

In working with alternate currents the same apparatus was employed, but differently connected. The opposite quadrants of the electrometer were permanently connected together, as is usually the case, the needle being kept at a fixed high potential by means of an independent battery. One of the secondary electrodes in the special vessel was connected to one pair of quadrants; the other secondary electrode and the second pair of quadrants were connected to earth. By means of a suitable commutator the rheostat in the circuit could be connected to the one pair of quadrants of the electrometer and to earth, in place of the electrolyte, and a comparison of the resistances could thus be made. The results obtained by this method are tabulated, as well as those obtained with continuous currents and those obtained by Kohlrausch; a comparison shows that the alternate current method is very accurate, while at the same time it is much easier of application.

F. AUERBACH — THE STARTING OF THE DYNAMO-ELECTRIC CURRENT.

A dynamo-electric current is a magneto-electric current which is reinforced by the mutual action of the current and the magnetic field. In order, therefore, that any dynamo-electric current may be started, there must be some residual magnetism to enable the machine to work as a magneto-electic machine.

It is, however, by no means true that any, however small, residual magnetism will give rise to a dynamo-electric current. The fact is, that it is essential that the speed should exceed a certain number of revolutions per minute; and this speed depends on the amount of the residual magnetism. There is thus for every machine a certain critical speed.

This statement may be verified by experiment. A machine is given the residual magnetism (r) by a current from some external source, and it is then driven at a speed n. If there is some kind of current meter in the circuit, it will indicate either the existence of a very weak current or the pointer will be driven far up the scale. In the former case n is less than the critical speed, in the latter greater; in the former it is only a magneto-electric current which is set up, in the second it is a true dynamo-electric current.

The experiments made by the author on a Gramme and a Siemens machine go to confirm the exactness of Clausius' theoretical deduction, that for slow

speeds the machine gives no (dynamo-electric) current; but it first begins to work after having reached a certain speed. The two cases are, however, not exactly similar, as Clausius considered that the machine first began to run at a very slow speed which gradually increased; while in the author's case the speed is always below the critical, so that no dynamo-electrical current will be produced however long the machine is kept running.

The question may be looked at from another point of view. In the first instance, the residual magnetism (r) produces a magneto-electric current $p \, r \, n$. This current produces a magnetic field $p \, r \, n \, . \, q$, which again gives rise to a current $p \, r \, n \, . \, q \, . \, p \, n$, and so on. The current can therefore be expressed by the infinite series—

$$i = p \, r \, n \, (1 + p \, q \, n + (p \, q \, n)^2 + (p \, q \, n)^3 + \ldots).$$

This series is convergent or divergent, accordingly as n is less or greater than $1/p \, q$—i.e., accordingly as n is equal to or greater than a certain value which is constant for the same machine. In the above series, q is not a constant, but a quantity into which r enters as a factor, hence it follows that the critical speed decreases with an increase of the residual magnetism—a conclusion which is confirmed by the experiments.

J. POPPER—ALTERNATE-CURRENT APPARATUS TO REPLACE INDUCTION COILS FOR MEASURING PURPOSES.

(Beiblätter, No. 4, 1888, Vol. 12, p. 255.)

The author makes use of a Poggendorff inverser, in connection with a telephone, in which the noise of the sliding spring over contacts of metal points and mercury is avoided. The wooden roller is provided with two pointed discs separated from each other by means of paraffined paper, which dip into mercury contained in ebonite cups. The level of the mercury in the cups is regulated by screwing iron screws in or out of them; when not in use these screws are withdrawn so far as to allow the mercury to fall into hollows, where it is protected from dust. The duration of the current can be regulated to 1-280th to 1-560th of a second.

J. D. OLTEN—CONDUCTIVITIES OF FATTY ACIDS, AND THEIR RELATION TO TEMPERATURE.

(Beiblätter, Vol. 12, No. 4, 1888, pp. 259-60.)

The method is that given by Kohlrausch with alternate currents from an induction coil and measurement of the current by means of a Wheatstone bridge and electro-dynamometer. A glass cylinder 70 mm. high, 46 mm. wide, is closed by a ground glass plate 7 mm. thick. Through holes in this plate pass enamelled platinum wires to which are attached semi-cylindrical platinised platinum electrodes, 10 square centimètres in area, with their concave surfaces opposed to each other. Through other openings are introduced a capillary tube, in which the liquid can rise on heating, and a

thermometer. The experiments were carried out with formic, acetic, propionic, and butyric acids.

It appears that the resistances of the acids of a homologous series increase. In all acids the conductivity increases proportionately to the percentage composition of the solution up to a certain point, and then decreases. As the homologous series is ascended, the maximum occurs at a lower and lower percentage; thus with formic acid it is about 30 per cent., acetic acid 16 per cent., propionic and butyric acid 12 per cent. and 10 per cent.

The better the solutions of the same acid conduct, the more the temperature curves bend down towards the axis of the abscissæ, and the more convex do they become. The conductivities K_t at a temperature t are calculated from the formula

$$K_t = K_o (1 + at + bt^2),$$

the values of the coefficients being given for the four acids.

J. POPPER—STANDARD DANIELL CELL.

A small copper disc is covered with a fairly thick linen disc soaked in solution of copper sulphate, and on this is laid a larger disc of parchment paper. Then comes a linen disc as before, but soaked in solution of zinc sulphate, on which is laid first a very thin amalgamated zinc disc, and finally a thick zinc disc on top of all.

The cell is contained in a wooden box in which are three receptacles for the two solutions and distilled water, and others for copper and zinc wires and for copper and zinc forceps for taking hold of the discs. For each measurement new linen discs and a new thin amalgamated zinc plate are used.

L. von ORTH—A NEW METHOD OF TESTING BATTERIES AT WORK.

The new method is based on that of Mance, from which it differs by the introduction into the galvanometer circuit of an electro-motive force which is exactly equal and opposed to the potential difference at the ends of the galvanometer circuit. If the resistance of the galvanometer circuit is made very small, there will be no current through the galvanometer when the bridge circuit is open. The current has therefore to be allowed to increase much less than is the case in Mance's method (12 per cent. instead of 300 per cent.) in order to obtain a deflection of the galvanometer needle with a decrease in the ratio of the two parts of the mètre wire by about ½ per cent.

HARTMANN and BRAUN—NEW GALVANOMETER.

On the inside of a horizontal coil are fastened hollow semi-cylinders of

soft iron in the same line, and symmetrically placed. Heavy iron semi-cylinders are also fastened to a spindle passing through the axis of the coil and carrying a pointer. These suspended cylinders when at rest are not in line with the fixed ones, but hang down, and have only one edge between them. When a current is passed through the coil the suspended semi-cylinders tend to turn about their spindle in consequence of their attraction.

MÜLLER—A MEANS FOR JOINTING PHOSPHOR-BRONZE WIRES WITHOUT THE APPLICATION OF HEAT.

It is very desirable to avoid by all means the heating of phosphor-bronze wires, as this leads to a decrease of the breaking strain; and consequently some means of jointing had to be sought for which might replace the method of soldering used for iron wires. Moreover, experience has shown that some local action is set up between the tin solder and the iron wires, which in time leads to a weakening of the joint; such action would of course be still greater with phosphor-bronze wires, owing to the greater affinity of tin and copper.

The best substitute for solder is a copper amalgam prepared as follows:—Freshly precipitated copper oxide is heated in a tube in a stream of hydrogen until it is reduced to metallic copper. When thoroughly cooled, this fine copper powder is moistened with dilute sulphuric acid and well mixed with mercury, and is then washed. A simpler plan, though not such a good one, is to obtain the copper powder by reducing copper sulphate by means of metallic zinc.

In making use of this amalgam for jointing wires, it is pressed closely round them and allowed to set. On this account a twisted joint is not very very well adapted; a Britannia joint is better, as the amalgam penetrates inside the binding wire. But the best joint is made with a tube connection, the two ends of the wires being turned up at not too sharp an angle and secured with binding wire; the amalgam is then pressed well into the tube, where it sets quite hard.

W. KOHLRAUSCH—CALCULATION OF LIGHTNING CONDUCTORS.

The author's calculations had led him to similar conclusions to those arrived at by Vogel (see Abstracts, vol. xvii., p. 341). In using Vogel's formula we should, however, put in values of the specific heat and resistance which correspond, not to ordinary temperatures, but to mean temperatures between the ordinary and the melting point. The metals with which we are more generally concerned are zinc (on roofs), lead (water pipes), copper, and iron, the values for each of which are given in the following table:—

	Melting Point.	Specific Heat.	Specific Gravity.	Conductivity.
Zinc	410°	0·10	7·2	9·0
Lead	326°	0·033	11.3	3·5
Copper	1,200°	0·125	8·9	20·0
Iron	1,600°	0·18	7·5	2·0

Putting these values into Vogel's formula, we find that iron should have 2·5 times, zinc 3·2 times, and lead 8 times the section of copper. The figures given by Vogel expressly exclude all consideration of the mechanical disintegration of the conductor; but a momentary powerful current might be able to tear a conductor to pieces below the melting point.

We may arrive at some estimate of the quantity of electricity discharged in a lightning flash in the following way:—Flashes of lightning have not unfrequently occurred which have fused copper conductors 5 square millimètres in cross section. A length of 1 mètre of such a wire weighs 44·5 grammes, and would require about 6,700 gramme-calories to raise it to the melting point, 1,200° C. The average resistance of such a conductor between the temperatures of 15° and 1,200° would be about 0·01 ohm. The heat developed would be

$$Q = 0·24 \times I^2 \times t \times 0·01 \text{ gramme-calories.}$$

Now Q is 6,700, as stated above, and t, the duration of the discharge, may vary from 0·001 to 0·03 second. The current will therefore be between 52,000 and 9,200 ampères; or, multiplying by the time, the quantity of electricity would be from 52 to 270 coulombs.

That these figures are not entirely imaginary may be shown by starting from another point. Kayser has photographed a flash of lightning, which had a diameter of 3 dm., or, say, a cross section of 7 sq. dm. Suppose that the current was some 30,000 ampères: then, if the cross section of the electric arc in a lamp is proportional to the current between the carbons, the cross section of an arc produced by 10 ampères would be

$$\frac{70,000 \times 10}{30,000} = 23 \text{ sq. mm.;}$$

or it would have a diameter of 5·4 mm., which is about the value found in practice.

Dr. H. WEDDING—RELATION BETWEEN TEXTURE AND CONDUCTIVITY OF IRON WIRES.

(*Elektrotechnische Zeitschrift, Vol. 9, No. 7, 1888, pp.* 172–78.)

Experiments were undertaken on twenty-one different samples of wire in order to ascertain, if possible, the reason for the acknowledged superiority of Swedish iron wire. The wires were subjected to tests of the breaking strain; they were analysed in order to ascertain the exact amount of impurities in the iron; and they were carefully examined under the microscope for any differences of texture which the iron might present. The results are tabulated, the wires being arranged in order of their conductivity.

The tables show that, generally speaking, the strongest wires are the worst conductors; and the less the elasticity the less the conductivity. The effect of the presence of impurities in the iron cannot be so clearly seen. The percentage of carbon in the wire necessarily affects the breaking strain and the elasticity, and hence indirectly the conductivity, but no simple relation was found. Generally speaking, an increase in the percentage of phosphorus means a decrease of conductivity; the same result was observed in the case of manganese. Indeed, if the percentages of phosphorus and manganese are added together, the conductivity is seen to be inversely proportional to the amount of these impurities in the iron.

The microscopic examination of the wires led to the following conclusions: —The finer the grain the higher the conductivity; in wires of very even grain the conductivity increases with the evenness; small flaws, such as blisters, cinder-holes, faults in welding, when evenly distributed, have no particular effect on the conductivity; large blisters and welding flaws have only a small effect in diminishing the conductivity if they occur separately; cinder-cracks and welding flaws when they are numerous and run in the direction of the length of the wire diminish the conductivity considerably; cross flaws and cracks diminish the conductivity most. Classified according to the microscopical observations, the twenty-one wires fall into four groups—1st, the fine-grained even wires; 2nd, the coarse-grained wires with increasing amount of flaws and cracks; 3rd, the coarse- and uneven-grained wires; 4th, the very coarse-grained and uneven wires.

A wire of the first class (9·50 conductivity and above) should not have a higher breaking strain than 36 kilogrammes per square millimètre (23 tons per square inch), nor an extension of less than 12 per cent.; the total of all impurities should not exceed 150 thousandths, nor the total of manganese and phosphorus 125 thousandths; the texture must be even and fine-grained, without blisters, flaws, or cracks. A wire of the second class (7·75 to 9·50 conductivity) should not have a higher breaking strain than 45·1 kilogrammes per square millimètre (29 tons per square inch), nor an extension of less than 17·2 per cent.; the total of all impurities should not exceed 450 thousandths, nor the total of manganese and phosphorus 400 thousandths; the texture may be coarse-grained, but it must be even. A wire falls into the third class when, although its breaking strain and extension would rank it in the second class, its texture is uneven or full of flaws. A breaking strain of more than 50 kilogrammes per square millimètre (32 tons per square inch) and an extension of less than 8 per cent., together with a very coarse and uneven grain, bring a wire into the fourth class and render it unsuitable for use as a conductor or line wire.

In all probability the material for a wire of first-class conductivity can be obtained by smelting in a crucible or in a reverberatory furnace; the products of the Bessemer converter can never be suitable.

Dr. W. A. NIPPOLDT—CALCULATION OF LIGHTNING CONDUCTORS.

The author claims priority over Dr. Vogel in the use of the formula given by the latter. He also starts from the hypothesis of Arago that an iron conductor of 144 sq. mm. cross section is amply large enough. Already in the year 1874 he had shown that when iron is replaced by any other metal the rise of temperature caused by the passage of the electricity must serve as the basis of all calculation. In practice the choice of material lies between copper and iron, and as the square root of the product of the specific heat into the specific gravity is about the same for both, the equation reduces itself to the ratio between cross section and resistance.

The following table shows the values calculated by the author, all being referred to iron with a specific heat of 0·114, specific gravity of 7·8, and specific resistance of 0·16:—

	Melting Point.	Specific Heat.	Specific Gravity.	Specific Resistance.	Area. Sq. mm.	Weight. Kilos. per m.
Iron...	1,600°	1	1	1	144	1·123
Copper	1,100°	0·83	1·14	0·106	48	0·428
Zinc	450°	0·84	0·92	0·350	97	0·697
Lead	335°	0·27	1·46	1·223	253	2·880

The author omits the melting point, as it lies so high, for iron and copper, and he calculates the area from the formula,

$$\text{Area} = 144 \sqrt{\frac{\text{specific resistance}}{\text{specific heat} \times \text{specific gravity}}};$$

all three quantities being referred to iron as unity, as in above table.

He also draws attention to the recommendation of Dr. L. Weber (*Elektrotechnische Zeitschrift*, vol. vii., p. 446) to use carbon points for lightning rods rather than platinum ones.

LIST OF OTHER ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM,

Appearing in some of the principal Technical Journals during the month of
MAY.

(*Philosophical Magazine, Vol.* 25, *No.* 156, 1888.)

H. TOMLINSON—The Temperature at which Nickel begins to Lose Suddenly its Magnetic Properties. **O. HEAVISIDE**—Electro-magnetic Waves, and Forced Vibrations of Electro-magnetic Systems. **R. H. M. BOSANQUET**—On the Use of the Term "Resistance" in the Description of Physical Phenomena.

PROCEEDINGS OF THE ROYAL SOCIETY.

(*Nature, 24th May,* 1888.)

W. H. PREECE—On the Heating Effects of Electric Currents (III.). **Dr. J. C. EWART**—On the Structure of the Electric Organ of *Raia Circularis.*

(*Nature, 31st May,* 1888.)

Dr. G. GORE—Effect of Chlorine on the Electro-motive Force of a Voltaic Couple. **Professor EWING**—Magnetic Qualities of Nickel.

(*Annales Télégraphiques, Vol.* 14, *November—December,* 1887.)

VASCHY—Effects of Lightning Discharges through Telegraphic Apparatus. **G. DE LA TOUANNE**—Telephony between Towns. **A. BARBARAT**—Use of Copper Wire for Overhead Conductors.

(*Journal de Physique, Vol.* 8, 1888.)

J. VIOLLE—Comparison of the Total Energy emitted by Melting Platinum and by Melting Silver. **P. LEDEBOER**—Effect of Temperature on the Magnetisation of Iron. **GOUY**—Contact Differences of Potential. **J. BORGMANN**—Heating of the Glass of a Condenser by Successive Charges and Discharges. **J. BORGMANN** — Method of Explaining Kirchhoff's Second Law of Derived Circuits. **P. BACHMETIEFF** — Effect of Mechanical and Thermal Changes on Magnetism. **P. BACHMETIEFF**—Experiments on Thermo-Electricity. **N. KOBY-LINE and FÉRÉCHINE**—Magnetic Properties of Mixtures of Iron Dust and Carbon. **N. SLOUGUINOFF**—Kirchhoff's Second Law Deduced from the Principle of the Conservation of Energy. **J. BORGMANN**—Propagation of Electricity through Air.

H. PELLAT—Absolute Electro-Dynamometer and Standard Ampère. E. HOSPITALIER — Meters for Electric Energy. Anon.—Experiments made at the Central Laboratory on Fire Risks from Incandescence Lamps.

R. BLONDLOT — Theory of Diamagnetism. A. RIGHI — Electric Phenomena produced in the Ultra-violet Rays. E. BICHAT and R. BLONDLOT—Effect of the Ultra-violet Rays on the Passage of Low-tension Electricity through Air.

GOUY and H. RIGOLLOT—Electro-chemical Actinometer.

C. E. GUILLAUME—Measurement of Temperatures by Electrical Means. F. LARROQUE—Source of Atmospheric Electricity. A. PALAZ—Lecoultre's Switch. C. DECHARME—Relation between Magnetism and Mechanical Actions. C. REIGNIER—Jehl & Rupp's Disc Dynamo. E. MEYLAN—Central Station at Mende.

W. FRITSCHE—Central Stations for Electric Light. G. RICHARD—Details of Dynamo Construction. L. PALMIERI — Atmospheric Electricity. E. ZETSCHE—New Apparatus for Telephone Exchanges. C. DECHARME—Galvanometer based on Lateral Displacement.

J. LUVINI—Atmospheric Electricity. G. RICHARD—Details of Dynamo Construction. C. E. GUILLAUME—Measurement of Temperatures by Electrical Means. E. DIEUDONNÉ—Thomson Galvanometer with Removable Coils. J. KAREIS — New Water Motor for Rivers. A. GRAVIER—Improvements in the Gerard Dynamo. C. WEYHER —Waterspouts and Cyclones. E. MEYLAN—New Arc Lamps.

Dr. A. TOBLER—New Baudot Apparatus. A. LEDUC—New Methods of Measuring Magnetic Fields. H. HORNEMANN—Electric Light in European Theatres. A. GRAVIER—Magnetic Fields of Dynamo Machines. E. ZETSCHE—New Block System for Stations. E. GIMÉ—Pendulum with Electric Contact.

W. v. ULJANIN — Electro-motive Force of Illuminated Selenium. H. HERTZ—Induction Phenomena in Insulators. A. VOLLER—Measurement of High Potentials with the Electrometer. R. BÖRNSTEIN—A New Form of Electro-Dynamometer.

K. L. BAUER — Determination of the Poles of Influence Machines. **G. GUGLIELMO** — Modification of the Quadrant Electrometer. **WRIGHT** — Electrometer. **H. LUX** — Relation of the Conductivity of Electrolytes to the Dielectric Mediums contained in them. **E. SEMMOLA**—Production of Electricity by Condensation of Steam. **C. CATTANEO**—Electro-motive Force of the Amalgam in a Daniell Cell. **J. MIESLER**—Splitting Up of the E.M.F. of Cells. **A. RIGHI** —E.M.F. of Cells with Badly-conducting Liquids. **J. FARKAS**—Relation between Chemical and Electrical Energy. **P. WALDEN**—Calculation of the Size of Molecules of Salts from the Conductivity of their Solutions. **G. GRASSI** — New Method of Measuring very Strong Currents. **A. ROSEN**—Unipolar Induction. **P. CARDANI**—Electric Discharge in Highly-heated Air. **H. LUGGIN**—Researches on the Electric Arc. **C. MARANGONI**—Electric Discharge by Minerals. **V. ZAHRADA**—Determination of the Resistance of the Mercury Unit in Absolute Measure.

At a Special General Meeting of Members, held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, November 8th, 1888 — Mr. EDWARD GRAVES, President, in the Chair—

The notice convening the meeting was read by the SECRETARY.

The PRESIDENT: You are all aware that in February last a circular was issued to the members generally, stating the proposed alteration in the title of the Society. To that circular we have received 846 replies, being a clear majority of the Members and Associates of the Society. Of these answers 837 are in the affirmative—*i.e.*, they approve of the change of name as proposed; 9 are of an opposite tendency; but although it may be said that the proposal is thus agreed to by the mass of members, it legally requires the confirmation of a Special General Meeting of Members such as this, and I therefore have simply to put the following resolution :—

"That the name of the Society be changed to 'The Institution "'of Electrical Engineers.'"

Mr. ALEXANDER SIEMENS seconded the motion, which was carried unanimously.

The PRESIDENT: The second resolution which I now put is—

"That the office of Honorary Secretary be abolished, and "that the Articles of Association be altered by omitting all "reference to the Honorary Secretary in Articles 36, 38, 40, "and 43."

In the early days when the Society was forming, the Honorary Secretary was useful as a means of making the intentions of the Society known; but now that the position of the Society is established, and that for some time past the permanent Secretary has fulfilled all the duties pertaining to the post, it is possible that the appointment of an Honorary Secretary might create a division of responsibility which is most undesirable; at any rate, that is the opinion of the Council, and I therefore move that the resolution I have just read be adopted.

Professor D. E. HUGHES seconded the proposition, which was carried unanimously.

The PRESIDENT: I think I may now declare the Special General Meeting as closed.

The SECRETARY reminded Members that it was necessary for them to attend on the 22nd November, when another Special General Meeting would be held to confirm the resolutions just passed, and so make them legal " Special Resolutions."

The One Hundred and Eighty-first Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, November 8th—Mr. EDWARD GRAVES, President, in the Chair.

The minutes of the previous Ordinary General Meeting were read and confirmed.

The names of new candidates for election into the Society were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—
From the class of Students to that of Associates—
    William Henry Collis. | Frederick Paton.

Donations to the Library were announced as having been received since the last meeting from the Astronomer Royal ; the Institution of Civil Engineers ; the Executors of the late Sir William Siemens ; W. Larden, Esq., M.A. ; Messrs. Macmillan & Co. (Publishers); Mons. Pugnetti ; Messrs. Whittaker & Co. (Publishers); R. K. Gray, Member; Killingworth Hedges, Member ; Professor A. Jamieson, Member ; Professor Dr. A. Stoletow, Foreign Member ; Professor Silvanus P. Thompson, Member ; Professor John Tyndall, Member ; and Mons. E. Wünschendorff, Foreign Member.

The SECRETARY said : I have the pleasure of further announcing that a very fine lithographic framed portrait of our late President, Sir Charles T. Bright, has been presented to the Society by Lady Bright ; also that Sir David Salomons has kindly presented, for the new reading room which has lately been added to the Library, two handsome oak book-cases.

The meeting accorded a hearty vote of thanks to Lady Bright, Sir David Salomons, and the other donors for their presentations.

The following paper was then read :—

ON OCEAN TEMPERATURES IN RELATION TO SUBMARINE CABLES.

By Wm. Lant Carpenter, B.A., B.Sc., Member.

Just a year ago (November 10th, 1887) a valuable paper was read before the Society (so soon to be called the Institution of Electrical Engineers) by Mr. Stallibrass, entitled "Deep-Sea "Sounding in connection with Submarine Telegraphy."

In that paper, and in the discussion which followed, scarcely any reference was made to what is now known as "temperature "sounding;" and as some of the facts revealed by this process appear to have an important influence upon the electrical condition of submarine cables, I venture to think that they may not be without interest to the Society.

Reserving for a little the description of the methods employed for ascertaining deep-sea temperatures, I may point out that the deep-sea surveying expeditions, commenced in 1869, of H.M. surveying vessels "Porcupine," "Shearwater," and "Challenger," of the United States steamer "Tuscarora," and of vessels belonging to other Governments, have shown conclusively that the temperature of the great oceans may vary in the tropics from 85° Fah., at the surface, to 30° Fah. (or even occasionally slightly lower) at the bottom; and that the bottom temperature of inland seas of the same depth, such as the Mediterranean, Red, Celebes, and Sulu Seas, is often very much higher than 30°.

A submarine cable, therefore, is liable to be exposed at various portions of its length to a range of temperature between 85° Fah. and 30° Fah. Such a cable is affected in two ways by alterations in its temperature—(1) the resistance of the copper conductor; (2) the resistance of the insulation. I believe that the capacity of the cable is but little altered by changes in its temperature.

Figs. 1 and 2 show the amount of these in each case. A copper conductor having a resistance of 9 ohms per knot (sea mile)

at 50° Fah. would have a resistance of about 8·6 ohms at 30°
Fah., and of 9·6 ohms at 85°, the exact figures being—

30°	40°	50°	60°	75°	85°
8·632	8·803	8·974	9·144	9·40	9·57

or the increase of resistance between 30° and 85° is 10·8 per cent.
of what it is at the lower temperature. I am informed that in the
case of the extremely pure copper wire prepared by the new
Elmore electro-burnishing process (by which a film is burnished
during its electro-deposition), which has a conductivity of 102·38
in terms of the usual standard for "pure" copper, this tempera-
ture coefficient is slightly higher than that given by Dr.
Matthiessen for pure copper.

The insulation resistance is affected, however, to a very much
greater extent, the exact figures being, in megohms per knot—

30°	40°	50°	60°	75°	85°
8,085	4,292	2,308	1,242	490	263

Hence the insulation resistance of a cable may be *more than
thirty times* as great at 30° Fah. as it is at 85° Fah.*

I hope that these figures will be considered enough to show
the great influence of the temperature of the sea upon cables. I
may add that all insulating materials are not equally affected.
Thus, taking three cables coated with different insulators, but
each having an insulation resistance of 500 megohms per knot at
75° Fah., I am informed that at 28° Fah. the insulation resistance
of gutta-percha would have increased to 12,850 megohms per
knot, of Mr. Willoughby Smith's insulator to 15,200, and of
Hooper's compound only to 1,590.

Although I do not propose to discuss the effect of the pressure
at great depths, it may be as well to point out here, since the
whole question of the effect of the sea upon the insulation

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* I should add that these figures relate to the cable between Accra and
Sierra Leone (African Direct Company), laid in 1886, and that Mr. H. D.
Wilkinson has kindly assisted me in their calculation. The weight of copper
in this cable is 130 lbs. per knot, which is also the weight of the gutta-percha
employed, and the sheathed deep-sea portion of the cable weighs 3¼ tons per
mile.

resistance of a cable is under consideration, that Mr. Latimer Clark's formula enables this to be calculated, viz.,

$$R\,p = R\,(1 + 0\cdot00023\,p),$$

where R is the resistance at atmospheric pressure, and R p that at a pressure of p lbs. per square inch. An approximate datum for ascertaining this when the depth is known is, that 800 fathoms depth gives a pressure of very nearly one ton per square inch.

Having thus shown the importance of a knowledge of deep-sea temperatures to the cable electrician, I will now describe the means adopted for ascertaining these temperatures. Prior to 1868, but very few isolated observations on this subject had been made, and the results were so remarkable that in many cases they were attributed to errors of observation. I believe that it was the present Hydrographer to the Admiralty who first reported a bottom temperature of 31° in the Indian Ocean. The method of observation adopted was to fasten an ordinary self-registering thermometer to the sounding line immediately above the machine which carried the detachable weight, to lower it to the bottom, and to read it when hauled up again, assuming that the lowest recorded temperature was at the bottom.

One reason why there was an inclination to reject the earlier observations was, that they were inconsistent with the general idea propounded by Sir John Herschell, in his treatise on Physical Geography, " that in all deep seas a temperature of 39° Fah. will " probably be found to prevail." This idea was founded on the assumption that sea-water, like fresh water, attained its maximum density at 39° Fah., whereas in reality it continues to contract down to its freezing point, which is slightly below 28° Fah.

A real source of experimental error was, however, soon discovered : the pressure of the sea at great depths, acting on the bulb of the thermometer, compressed it, and forced the liquid up in the stem, thus indicating too high a temperature. Several devices were proposed to obviate this ; the one finally adopted in 1869 being due in the first instance to the suggestion of the late Professor Wm. Allen Miller, and practically carried out by Mr. Casella, and hence the instrument is known as the Miller pattern of thermometer. I ought to add, however, that this

suggestion of Professor Miller's was made in entire ignorance of the fact that thermometers had been previously constructed on the same principle by Messrs Negretti & Zambra for Admiral Fitzroy. We have therefore here another case of simultaneous invention of the same thing by two independent persons. The apparatus is shown in Figs. 3 and 4.* It consists essentially of the ordinary Six's pattern of self-registering thermometer, but round the bulb is placed an exterior bulb, the space between the two being partly, though not entirely, filled with spirits of wine, a bubble of vapour being left. Numerous experiments made on shore with thermometers with and without this protection, enclosed in iron boxes filled with water upon which hydraulic pressure to the extent of 4 tons per square inch was brought to bear, seemed to show that in this way the error due to pressure was eliminated.

In the spring of 1869 it was my good fortune to be a member of the deep-sea sounding and dredging expedition of H.M.S. "Porcupine," and to have, under Captain Calver, the first use at sea of these "protected-bulb" thermometers. The general results I obtained were entirely confirmatory of those on shore. I prepared at sea a curve showing the differences of the indications between the protected and the ordinary thermometers for a series of depths up to about 1,400 fathoms, and had the great satisfaction of being informed by Admiral Richards, who was then the Hydrographer to the Admiralty, that this curve was exactly the same as one obtained on shore by means of the hydraulic press arrangement before alluded to. Since then these instruments have been most extensively used in deep-sea temperature sounding. Another form has since been constructed by Messrs. Negretti & Zambra from suggestions kindly furnished to them by Commander Magnaghi, of the Royal Italian Navy. The bulb is protected in the way previously described, and the neck of the inner bulb is contracted immediately above it, the

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* At the meeting several examples of these and other instruments described, were exhibited, through the kindness of the Hydrographic Department of the Admiralty, of Messrs. Negretti & Zambra, and of Messrs. Siemens Brothers, Limited.

contraction leading to a small reservoir (Fig. 5). The purpose
of this contraction, as will appear presently, is to break the
mercury column. The bulb contains enough mercury to partly
fill the reservoir also. In using the instrument the bulb is in
the usual downward position (Fig. 6), and continues so until
the sounding line begins to be hauled up ; this reversed move-
ment places the thermometer bulb upwards (Fig. 7), the mercury
breaks off at the contraction, and falls into what is now the lower
part of the tube, when its length is measured on a scale, and
the temperature at which its position was reversed is thus
recorded. In deep-sea observations this reversal of position is
effected by means of a fan working a screw, which releases the
thermometer.

The so-called Miller pattern of thermometer, however, from
its extreme simplicity, is still largely employed in deep-sea
observations in H.M. Navy ; and I am informed by the Hydro-
grapher to the Admiralty that the reversing apparatus of Messrs.
Negretti & Zambra cannot always be depended upon in very deep
water.

The " Porcupine " Expedition, moreover, was supplied with a
most ingenious device of the late Sir W. Siemens for controlling
electrically the results of the thermometric readings. I well
remember my first introduction to him, in the spring of 1869, to
be instructed in the use of the apparatus, of which I was specially
in charge. Through the kindness of Messrs. Siemens Bros.,
Limited, I am enabled to show you the instrument to-night. It
consists essentially of two coils of wire, similar in all respects,
which form the two arms of a Wheatstone bridge. One of these
is attached to a cable, and can be lowered to any desired depth
in the sea ; the second, or comparison, coil being immersed in
water on deck. The bridge is balanced, not by changing the
resistances, but by altering the temperature of this water ; and
when equilibrium is obtained, this temperature is read off by a
thermometer, which is also the temperature of the submerged
coil. Owing to the fact that the galvanometer furnished to us
in 1869 was very far behind the beautiful marine galvanometers
of the present day, it was only possible to make observations in

exceedingly fine weather, when our small vessel of 400 tons was comparatively still.

To avoid the error which would be otherwise introduced by the leads to the resistance coil, the cable was constructed of a double core of insulated copper wire, protected by galvanised steel wire. One of the copper cores was connected to the arm B C of the bridge, and the other to the arm D C, and the steel wire served as the return earth-connection for both.
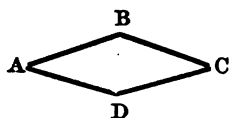
In the case of an instrument of this kind constructed for the American Government, and employed on board the surveying steamer " Blake " by Commander Bartlett, the resistance coil and comparison coil were made of silk-covered iron wire 0·15 mm. diameter, each having a resistance of 432 ohms at 66° Fah. To allow the coil to be readily affected by changes in the temperature of the water, it was coiled on a brass tube with both ends open, allowing a free passage to the water. Sir William Thomson's marine galvanometer, with mirror and scale, were in this case employed to determine the balance of the bridge.

In a paper by Sir William Siemens in the *Proc. Roy. Soc.* for June 15th, 1882, will be found the detail of a large number of comparisons, at various depths down to 800 fathoms, between the indications of this instrument and of the Miller-Casella thermometers simultaneously employed. The general result only is here given, in Sir William Siemens' own words :—
" The two instruments gave precisely the same readings at
" positions of maximum or minimum temperature, but in inter-
" mediate positions the electric thermometer in almost every
" instance gave a higher reading. This discrepancy may be
" accounted for, I think, by the circumstance that the electrical
" thermometer gives the temperature of the water actually
" surrounding the coil at the moment of observation, whereas
" the reading of the Miller-Casella instrument must be affected
" by the maximum or minimum temperatures encountered in
" its ascent or descent, which may not coincide with that at the

" points of stoppage. A strong argument in favour of the
" electrical instrument for geodetic and meteorological purposes
" has thus been furnished."

The physical, chemical, and biological results obtained by
H.M.S. " Porcupine " in the North Atlantic in the summer
of 1869, and in the Mediterranean in that of 1870, were of
sufficient interest to cause the fitting-out and despatch of H.M.S.
" Challenger " for a systematic survey of the great oceans, which
lasted for more than three years. The expedition left Sheerness
on December 7th, 1872, and returned to Spithead on May 24th,
1876, having altogether traversed a distance of nearly 70,000
miles, and having established, at intervals as nearly uniform as
possible along the course traversed, 362 observing stations, at each
of which the bottom temperature was accurately ascertained,
while at most of them the thermal stratification was determined
by " serial temperature " soundings. The vessels of other Govern-
ments have also contributed a good share of observations.

The work of the United States steamer " Tuscarora " (Com-
mander George E. Belknap), however, deserves special notice here,
because she was sent out by the United States Government, not
with a view to scientific research, but for the purpose of determin-
ing by deep-sea sounding the most practicable route for a cable
between the Pacific seaboard of the United States and Japan.
In addition to the ordinary bathymetrical determinations, bottom
temperatures were everywhere taken with Miller-Casella ther-
mometers, and the thermal stratification was also determined
down to about 600 fathoms by serial soundings. A very important
body of facts was thus collected in regard to the thermal condition
of the North Pacific along the two lines examined, viz.—the
southern route, passing directly across between the parallels of
20° and 30° N. lat. from San Diego, in California, by way of
the Sandwich Islands, to the Bonin Islands, and thence north to
Yokohama; whilst the northern route followed a great-circle
course from Yokohama along the line of the Kurile and Aleutian
Islands to Cape Flattery, the northernmost point of the United
States territory. Here such great depth of water—over 4,600
fathoms—was met with, having a bottom temperature of 32°, that

it was then deemed (1874) an impracticable route for a cable. During this cruise no fewer than 483 casts of the deep-sea lead were taken, and the results were plotted in the same manner as those of the " Challenger," and embodied in a systematic account of the work issued by the Hydrographic Office of the United States Navy, a copy of which I have here. The general result of all such observations, as regards the distribution of temperature in the open oceans, contrasted with that of seas more or less land-locked, such as the Mediterranean, I will now very briefly summarise.

By attaching thermometers to various points of the sounding line or wire, it is possible to ascertain the temperatures at various depths below the surface, as well as on the bottom.   Such a set of observations made vertically over one spot is called a " serial " temperature sounding."   The results of a number of such observations may be plotted upon a " temperature section," constructed upon two scales—a horizontal one of nautical miles, and a vertical one of depths in fathoms.   The lines, more or less inclined to the horizontal, which are drawn through points of equal temperature may be conveniently designated bathymetrical isotherms.   In the diagrams before you (reproduced without colour in Figs. 8 to 12 inclusive) the differences of temperature are indicated by coloured bands as well as by lines, blue in various shades indicating water below 40° Fah., green and purple being used for water from 40° to 65°, while warm water is shown by various degrees of pink.   It will be observed that all these diagrams are constructed upon two scales—a horizontal scale of nautical miles, and a vertical scale of depth in fathoms.

I will call your attention first of all to these temperature sections across the North Atlantic, from Madeira through the Azores and Bermuda, to Halifax, Nova Scotia (Figs. 8, 9).   You will observe, in the first place, that about two-thirds of the whole of this mass of water is below 40° Fah., the bottom temperatures ranging from 34·8° to 38·7°.   You will also notice the *extremely* small bulk, as shown by its sectional area, of the true " Gulf Stream," *i.e.*, the super-heated surface layers of water issuing from the Gulf of Mexico (Fig. 9) ; and, lastly, you will notice the continuity of the " cold " wall " (long known to the United States coast surveyors) with the

cold deep water of the Atlantic basin. This continuity was first traced, and the explanation of the phenomenon afforded, by H.M.S. "Challenger." In this diagram of the North Pacific you will observe the same general condition of thermal stratification prevailing, but that there is a much larger proportion of colder water, and the bottom temperatures are even lower. In the next place, let us notice the facts in the Equatorial Atlantic (Fig. 10), where you will observe that the colder water comes nearer to the surface than anywhere else; and as the surface temperature is extremely high, the rate of fall in temperature is very rapid, and the isothermal bands are very narrow.

Speaking generally, these are very fair examples of the thermal stratification of any one of the great oceans, whose communication with the others is unrestricted, not only at the surface, but also to great depths below. But now let me ask you to compare with these diagrams some which represent the thermal stratification of such seas as are more or less land-locked; and as a good example we may take the Mediterranean, on the bottom of which so many cables lie (Fig. 11). You will observe that, for the first 200 fathoms, the temperature falls in proportion to depth in much the same way as it does in the Atlantic outside the Straits of Gibraltar; but that after that point—about 55° Fah.—there is no further fall in temperature, notwithstanding that depths of 2,000 fathoms are reached, at which, in the Atlantic in the same latitude, and only a short distance west of Gibraltar, the temperature is 36°. We shall presently see reason to believe that in this and similar cases two factors are concerned in the result—first, the average winter temperature prevailing in the locality; second, and far the more important, the depth to which there is free communication between these partially land-locked seas and the open oceans.

In the case of some of these—the Red Sea, for example, whose only communication with the great oceans is through the shallow Straits of Babelmandeb—the mean winter temperature, or isocheimal, of the surface water has a considerable influence in determining the bottom temperature at great depths. The lowest temperature of this surface layer, even in its most

northern part, is 71°, and this temperature is uniformly carried down to the bottom at 450 fathoms depth. I am not aware of many deep-sea temperature observations, beyond those of Sir George Nares in the Gulf of Suez, having been made in the Red Sea; but it may be pretty certainly affirmed that no lower temperature than this will be found, even on a bottom exceeding 1,000 fathoms depth, since the lowest surface temperature in the southern portions is never below 75°, the highest being nearly 90°. Yet in the Arabian Gulf the temperature at a depth of 2,000 fathoms is not above, and is very probably below, 36·5°.

A similar contrast is shown by the temperature soundings of Commander Chimmo between the temperature of the Sulu Sea (a small area between the N.E. of Borneo and Mindinao) and of the China Sea. The former, though only partially land-locked, is so shut in by reefs and shoals as to have only a very superficial and limited connection with either the China Sea or the Celebes Sea. Notwithstanding this inclosure, the depth is very great, ranging to 1,603 fathoms, and the following bottom temperatures, taken along the line of the submarine cable between Singapore and Hong-Kong, make this point very clear:—

Sulu Sea.		Depth.			China Sea.
83°	...	Surface		...	84°
—	...	30 fathoms		...	77°
—	...	50–80	,,	...	71°
64·5°	...	100	,,	...	—
—	...	200	,,	...	51°
51·5°	...	308	,,	...	—
—	...	416	,,	...	41°
50°	...	500–1,603	,,	...	—
—	...	673–1,546	,,	...	37°

The Sulu Sea (Fig. 12, No. VI.) was also examined very carefully by the "Challenger," in its relations to both the China (Fig. 12, No. VII.) and Celebes Seas (Fig 12, No. V). From 82° at the surface, the fall is rapid to 60°, but the isotherm of 55° lies at about the same depth as the isotherm of 50° in the China and Celebes Seas; below this the fall is still

slower, so that the minimum of 50·5° is only reached at 400 fathoms, from which depth to the bottom, at 2,550 fathoms, the thermometer continuously indicates 50·5°. Hence the condition of this sea closely resembles that of the Mediterranean. The influence of a still less complete seclusion from the polar under-flow is shown in the Celebes Sea, where the bottom temperature at 2,667 fathoms depth is 38·5°; while at nearly the same depth in the Indian Ocean, a little to the west of Sumatra, the bottom temperature was found to be 32°.

Again, a "Challenger" sounding between New Zealand and the Fiji Islands gave 32·9° at 2,900 fathoms depth; but further west, towards Raine Island, at the entrance of Torres Strait, where the depth ranged from 1,350 fathoms to double that, the temperature never fell below the 35·1° found at 1,300 fathoms depth, "which proves undoubtedly," says Sir George Nares, "that below this depth the sea is cut off by a surround-" ing ridge, over which the greatest depth of water of any " channel through it is 1,300 fathoms. As this is about the " depth we found between the New Hebrides and the Fiji " Islands, we may take it for granted that from Sandy Cape, in " Australia, to New Caledonia, the New Hebrides, the Solomon " Islands, and New Guinea, there is a shallow bank with not " more than 1,300 fathoms depth of water. Below this depth, in " the hollow between the New Hebrides and Torres Strait, the " water is comparatively stagnant, as in the Mediterranean and " other cut-off seas."

What is the explanation of these remarkable facts—facts which have been verified over and over again, at various seasons of the year? The most probable one I believe to be (and there is much biological evidence in its favour, as well as physical) that a constant *vertical* circulation of the waters of the great oceans is going on, sustained by differences of specific gravity, which are due in the main to differences of temperature. This doctrine was accepted by Sir John Herschell only three weeks before his death, and was also formally referred to by Sir George Airy, in his presidential address to the Royal Society in 1872, as "certain in theory, and supported by observation." Speaking

generally, there is much evidence to·show that in the Atlantic, Pacific, and Indian Oceans the upper portions of the water in the Northern Hemisphere are moving in a north-easterly direction, while the lower layers are as constantly moving in a south-westerly direction. One very curious fact in support of this last assertion, and of especial interest to us, is given in the *Proc. Roy. Soc.* for 1870, vol. xix., p. 218. A buoy which was attached to the Atlantic Cable of 1865 broke away from the bulk of the cable, but carried with it a great length of the wire rope to which it was attached. Although the surface drift would have carried the buoy north-east of the spot where it was first floated, it did actually travel a long way to the southward, owing to the action on the long length of cable attached to it, of the lower layers of water.

This great vertical oceanic circulation, then, is an illustration on a gigantic scale of the convection currents in liquids with which we are all familiar in the hot-water circulatory systems in use for heating public buildings. In the case of the ocean, however, there is reason to believe that the polar cold at the surface, rather than the equatorial heat, is the prime cause of the movement. Into the discussion of that question, however, I must not enter here, and I content myself with calling your attention to this diagram (Fig. 13), which indicates the " general idea " of the movements of the water, which are modified in actual practice by the distribution of land and by the rotation of the earth.

If this theory of a general vertical oceanic circulation be accepted, the explanation of the phenomena presented by land-locked seas becomes at once apparent. They are more or less cut off by the surrounding barriers from this general circulation, and hence only receive a limited portion of the colder water. Thus, to take the case of the Mediterranean (Fig. 11), the greatest depth of the Straits of Gibraltar (its only communication with the general oceanic system) is about 200 fathoms, the temperature of the North Atlantic outside at that depth is about 54°, and hence no colder water than that can ,find its way into the Mediterranean ; and accordingly in that sea we nowhere find

a lower temperature than 54°. The same explanation applies, with variations in depth, to the other cases I have mentioned in the Indian Ocean and elsewhere, and in many of these instances coral reefs appear to be the barriers which cut off the cold-water supply.

The theory affords a reason, also, for the fact that under the equator the colder water is nearer to the surface than anywhere else in the tropics or sub-tropics (Fig. 10). Here the two streams of polar water meet—the Arctic and the Antarctic—and owing to this a double volume of cold water is present, which wells up to the surface quite rapidly. I may mention that this point was foreseen and predicted by the proposers of the theory before the observations were made; and subsequently these latter justified the expectations formed. There is, therefore, a continual ascent of glacial water under the equator; or, as has been well said, there is under the equator a belt of water which is colder than the water to the north and south of it. It would be very interesting to trace the effect of this great system of oceanic circulation upon climate, and to discuss the evidence for its existence. Our Society, however, does not concern itself with such matters, and I must forbear. I have striven to lay before you in very brief outlines the general facts of the distribution of temperature in the sea, and especially on the sea bottom, where our cables lie, and to point out the influence of these temperature conditions upon the cables themselves. I venture to hope that those members who have special knowledge with regard to the influence of temperature upon cables will give us the benefit of that knowlege in the discussion which I trust will now follow.

Mr.
Siemens.

Mr. ALEXANDER SIEMENS: We have listened to the exceedingly interesting paper from Mr. Lant Carpenter, and, with his permission, I will give some practical illustrations of the ocean temperatures which have been observed. The accompanying diagram shows the temperatures which we have observed on the Weston and Waterville section of the Commercial Cable Company's system. That company has kindly given permission to publish

the same. The observations, taken weekly, have been communi-
cated to our electrician—Mr. Frank Jacob—who has put the
diagrams together, and who has made a report to our firm, the
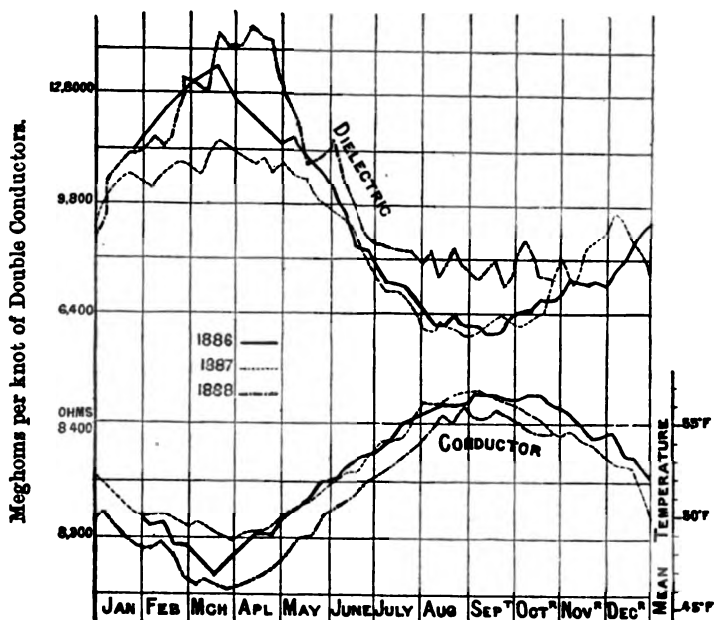
Annual Variation in Temperature, Weston-Waterville Cable.

substance of which I repeat. The length of the cable is about
320 nautical miles, and it encloses two conductors. The method
adopted for making the measurements was that for the con-
ductor resistance the two cores were connected in series, and
for the dielectric resistance one pole of the battery was con-
nected to one conductor and the other pole was connected
through the measuring instrument to the other conductor.
That method was devised by Mr. Frank Jacob for the particular
purpose of eliminating all influence of earth currents. The
lower curves of the diagram show the variations of tempera-
ture during three years : they give the conductor resistance
direct as the readings were taken from the instrument. The
upper curves show the insulation resistance for the whole length
of the cable, not in megohms per knot, but for the whole
length of the cable, and they were taken as the mean of 10

Mr.
Siemens.

minutes' electrification by readings at intervals of every 10
seconds. As the earth currents were eliminated by the method
of testing described, the electrification is as regular as in test-
ing a cable at the works during manufacture. It will be seen
that the variation in the resistance of the conductor as indicated
by the red line is also indicated, only inversely, by the varia-
tion of the resistance of the dielectric. The resistance of the
copper conductor while the cable was being manufactured at the
works was measured at the uniform temperature of 50° Fah.
For instance, in the beginning of May, 1886, and in 1887, the
cable was exactly at the same temperature as it was in the
tanks at the works. It is very curious that the observations of
1886 and 1887 are almost identical. In the beginning of June
the two lines always fall together, and also in September; and
altogether the two lines are exceedingly similar. It will be
further seen that in 1888, which was rather a cold year, the
curve shows the conductor resistance much lower all through
the year, as far as the curve goes, than it was in the two pre-
ceding years.

The maxima and minima fall in March and September in all
three years : in March the water was coldest, in September it was
hottest. If the temperature all along the cable had been exactly
alike, the temperature calculated from the resistance of the
conductor and the temperature calculated from the resistance
of the dielectric would be the same; but that is not the case,
and that seems to show that the water for the whole length of
the cable is not of a uniform temperature. In October, 1886, a
measurement was taken about 120 miles from the Irish end by a
thermometer in the way described by Mr. Carpenter, and the
actual temperature was found to be 52° Fah., whereas the resist-
ance of the cable gave it as 56° Fah. I will not go further into
details, but that discrepancy seems to point out that the waters
of the Bristol Channel have a great influence and vary more
during the year in their temperature than the water of the
deeper section of the cable.

It would perhaps be useful to point out that at 50° Fah. the
insulation resistance was 70 megohms for the total length of

320 knots; so, bearing in mind that we have two conductors, we find that the real insulation resistance per knot was 70 times 160, or 11,200 megohms. In a similar manner Mr. Jacob has furnished me with the average temperatures of some of the Atlantic cables which were laid by our firm, and it is curious that in four of them the average temperature comes out as 37° Fah., and in the fifth it is 38° Fah., as it lies in somewhat shallower water. Of course in these measurements the shore ends are included, therefore the temperature is a little higher than Mr. Lant Carpenter has shown the temperature of the bottom of the Atlantic to be. He said it was 34° Fah., so that the agreement of the two measurements is very close.

The PRESIDENT: May I ask Mr. Siemens what is the greatest depth of water that the Weston-Waterville cable is laid in?

Mr. ALEXANDER SIEMENS: 100 fathoms.

Mr. E. STALLIBRASS: It would be scarcely possible, Sir, to add much to the excellent paper that Mr. Lant Carpenter has read to us, and which was of special interest to me. I think that the Siemens electrical thermometer has not been used so much as it should have been on board telegraph ships, where there is every facility for using it, and where, I am sure, very excellent results would be obtained. The ordinary Miller-Casella thermometer has some disadvantages: great care is necessary for its successful use; the indices very often move at the least jerk; and except in careful hands that form of thermometer is to a certain extent unreliable. For shallow water I should prefer to use the capsizing thermometer of Negretti & Zambra, worked by a leaden weight sliding down the sounding line and releasing the locking arrangement.

I think, if this thermometer were used to any extent, it would be worth while to have a "correction table" for it. Although the amount of mercury which remains, and which is measured on the capsizing of the instrument, is small, still it is affected by temperature; and if, as frequently happens, it is read at a temperature considerably over 80°, the expansion of this small amount of mercury is worth considering. I remember Mr. Buchanan telling me that he had experimented in his laboratory

Mr.
Stallibrass. and had made a correction table, but I do not think it has ever been used.

Mr.
Siemens. Mr. ALEXANDER SIEMENS : May I add one word only? We have also a temperature report (unfortunately it is not here) for a cable which is laid in the Red Sea, and, with your permission, I will add the result to my remarks in the Proceedings, so as to have it on record and see how that agrees with the measurements about which Mr. Lant Carpenter spoke.

[The temperature of the Red Sea, as measured by the resistance of the copper conductor in the Suakim-Jeddah cable, is 66° Fah.]

The PRESIDENT : I have only just had the pleasure of seeing that Captain Wharton is present. Perhaps he will favour us with some observations ?

Capt.
Wharton. Captain W. J. L. WHARTON, R.N., F.R.S.: I must say, Sir, that I have very little to remark that is strictly germane to the paper—*i.e.*, anything in connection with submarine cables, of which I know nothing ; I can only say a few words about the apparatus that is used to ascertain the temperatures. I thoroughly concur with the last speaker (Mr. Stallibrass) that the Miller-Casella instrument has disadvantages—I will not say many disadvantages—and if we could get a more perfect instrument we certainly would be glad to use it. But the Magnaghi form does not answer in deep water. Our experiments go to show that in about two out of three soundings the thermometer reverses at some point before the one at which you wish it to reverse ; it comes out and shows entirely wrong ; so that we have more or less discarded it as being unreliable. The Miller-Casella gives very good results in general. Of course, if there is a colder stratum above a hotter, no doubt the result is unreliable; but this is very rare. The indices seldom get shaken down—at least we find it so—and when serial temperatures are taken you can judge very fairly from that alone whether any particular observation is incorrect; and it is very rarely now, with the perfection with which they are made, that these errors are apparent, and for the present I do not think there is anything better, unless it be Sir William Siemens' beautiful sounding instrument. This is, however, hardly adapted, I am afraid, for use on board our vessels,

where we have no electrical apparatus and no officers sufficiently Capt.
Wharton.
trained in electricity to use it.   I may mention that I have this
last week received a letter from one of our surveying vessels in
the South Pacific, from Captain Aldrich, of the " Egeria," where
he has obtained a very deep sounding of 4,430 fathoms south of
the Friendly Islands, and was lucky enough to bring up his
thermometer from the bottom safe.   In the same depth in the
" Challenger " four were sent to the bottom, and three came up
broken; Captain Aldrich only sent one to the bottom, and he was
lucky enough to bring it up safe with the temperature of
33·7° Fah.

Mr. CHARLES BRIGHT made some remarks, which are included Mr. Bright.
in the communication from him which will be found at the
end of the discussion.

Dr. EUGEN OBACH : Mr. Lant Carpenter mentioned the great Mr. Obach.
influence which pressure exerts on thermometers, and perhaps it
will be interesting to the Society to hear a few figures.   Some
two years ago I made experiments with a number of ther-
mometers to find out how great the influence of pressure was on
the readings.   I had only low pressures at my disposal in the
laboratory, but still some idea will be given as to the magnitude
of this influence.   I find that when a delicate chemical ther-
mometer with a cylindrical bulb, and divided into tenths of
degrees, is placed in a vacuum, the mercury falls about one to one
and a half tenths of a degree, and when the pressure is increased
one atmosphere above the ordinary it rises to about the same
extent, all the thermometers behaving similarly; so that one
might say that for this class of thermometers a difference in
pressure of two atmospheres produces a difference in the reading
of from one-fourth to one-third of a degree centigrade.   This
influence is thus sufficiently great to necessitate a correction in
various experiments, such as the determination of boiling points
at different pressures, &c.   As Mr. Carpenter drew special atten-
tion to this matter, I thought a few further details might be
interesting.

Mr. W. B. REDGRAVE : I have been away on many cable Mr.
Redgrave.
expeditions, and under my care have had thermometers of a form

Mr.
Redgrave.

similar to the Miller-Casella, made by Elliott Brothers.   We have taken temperatures with these in all depths, from 500 to 2,200 fathoms, and have never broken one yet.

Captain Wharton has said that a friend of his was lucky enough to recover a thermometer from a great depth.   I think that a very apt expression, as I have seen many thermometers go down, but few come to the surface again.   In taking deep-sea temperatures with the ordinary sounding wire and machine it is, to say the least, doubtful if you will recover your thermometer. With a thermometer made fast to the moorings of a buoy at a depth of 2,200 fathoms, the temperature was 37°.   Assuming the bottom stratum to be the coldest, this agrees fairly with Mr. Lant Carpenter's figures.

As regards the low temperature of the Western Atlantic, spoken of by Mr. Lant Carpenter, I should like to mention a fact probably unknown to many.   The temperature by copper resistance, using metallic circuit, of a cable laid in that position was 29·5° in the month of August.   Many people discredited this, and the test was repeated and confirmed by an eminent electrician now dead— Mr. J. C. Laws.

Capt.
Wharton.

Captain WHARTON : Could I be allowed to say one word more ? The last speaker referred to my saying that Captain Aldrich was lucky enough to get his thermometer up again, as if I was afraid of the wire breaking.   It was not that, but I was referring to the very great depth, which produced a pressure of 5 tons on the thermometer—very different to the 2½ tons to which I understand the other thermometers he has spoken of were alone subjected. It is that which I referred to, and I think it is a great triumph that instruments can be made that will stand such an enormous pressure.   We send down our thermometers without the slightest fear on account of the wire ; and Captain Aldrich, who has taken many soundings in the Indian and Pacific Oceans, has only lost three thermometers in some 80 soundings, varying from 1,000 fathoms to 4,000 fathoms.   We use galvanised wire now entirely for soundings.

Mr.
Carpenter.

Mr. W. LANT CARPENTER, in reply, said : I have very little to say beyond an expression of my great thanks to Mr. Siemens, and

also to Mr. Redgrave, for the exceedingly interesting observations Mr. Carpenter. that they have been good enough to bring before us. The fact of being able to ascertain the temperature of the sea by observations on the electrical conditions of a cable is, I venture to think, exceedingly interesting, and a capital proof of the wonderful perfection to which electrical measurements have now attained.

I should like just to point out one matter with regard to Mr. Siemens' observations which may not be altogether understood by the meeting generally : that is, that he did not make it quite clear that the cable on which those observations were made lies in really shallow water. The depth is not greater than 100 fathoms, and of course a good deal of what I have been saying relates to water of very much greater depths than that ; and such shallow water is affected by variations in the temperature of the air above it. As far as I remember, I believe the conclusion arrived at in the " Challenger " and other observations was to the effect that what might be called the super-heating effect of the sun's rays does not reach lower than about 100 fathoms ; so that there we have in this shallow-water cable an interesting and most beautiful example of the illustration of that fact, and of the effect of the temperature of the air upon the water below it.

With regard to what Mr. Bright was so kind as to say, I will only just point out that I think I was careful, in alluding to the diagram, to say that it *did* apply to one particular cable, and I gave the name of the cable and the particulars of it. I did not mean to imply at all that that should be taken as an average of insulators, but simply as one insulator ; and I was quite aware that various insulators had their separate coefficients of change with regard to temperature. I have to thank Mr. Bright for a correction which he was good enough to give me with regard to the pressure law, with which I had associated Mr. Latimer Clark's name ; and I am glad to find that a good deal of the result was due to one whose memory we all so much respect and revere— Sir William Siemens.

I have only now to thank the members for the patient attention with which they listened to my communication, which perhaps some of them might have thought was a little outside the usual run of the Society's papers.

The PRESIDENT: Gentlemen, we have listened to a very clear and able paper, of a class rarely introduced to our notice, and yet particularly interesting to one section of the members of our Society—those engaged in submarine enterprise. I think it is well that we should at times have something specially adapted to the tastes and interests of the various classes of our members, and I thank Mr. Lant Carpenter for having introduced his paper to us. I beg, therefore, to propose that you will present him with a hearty and sincere vote of thanks for the paper which he has been kind enough to read this evening.

The motion was carried unanimously.

The meeting then adjourned.

AZORES TO MADEIRA

Madeira

Formigas

1900
2025
2840
2575
2400
1650

FROM 55° TO 60°

FROM 50° TO 55°

FROM 45° TO 50°

FROM 40° TO 45°

FROM 35° TO 40°

2000   2100   2200   2300   2400

Fig 7.

(Challenger)
SULU SEA

0
200
400
600
800
1000
1200
1400
1600
1800
2000
2200
2400
2600
2800
3000
3200
3400

From 400 fms to the bottom is of
the uniform temperature of 50°5′

TERRANEAN

rface 78°

Fath.

55°   200
      400
      600
      800
      1000
      1200
      1400
      1600
      1800
55°   2000

Fig 13.

N. Pole   N. Temperate   Equator   S. Temperate   S. Pole

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COMMUNICATION.

THE PHYSICAL AND ELECTRICAL EFFECT OF PRES-SURE AND TEMPERATURE ON A SUBMARINE CABLE CORE.

By CHARLES BRIGHT, Member.

As regards the conductivity of copper referred to by Mr. Lant Carpenter, I would remark that the 102·38 per cent. mentioned by him is at the present time quite an ordinary conductivity for copper intended for telegraphic purposes, and consequently the correct coefficient for variation of resistance by temperature is frequently somewhat higher than that given for Dr. Matthiessen's standard, pointing to the desirability of a fresh standard being adopted.

Inasmuch as the resistance of gutta-percha is affected so much more sensibly by a change of temperature than that of copper, it is sometimes, in the case of gutta-percha cores, a good plan to calculate the temperature of the dielectric by the variation of the dielectric resistance as well as by that of the conductor, merely as an occasional check on the result obtained for the dielectric resistance at standard temperature. In calculating the average temperature of the dielectric from its altered resistance, it would require to be known that no fault of any description existed inter-mittently or otherwise. The effect of pressure on its resistance would, moreover, require to be included in the calculation.

No theory has been advanced in the paper as to the different physical effect produced by a change of temperature which causes the resistance of metals, such as copper, to be decreased by a cold temperature, and that of vegetable fibrous substances like gutta-percha or india-rubber to be increased thereby, and *vice versâ*. The physical effect is presumably the same in both cases as that of pressure of the water, only materially more marked. The effect of pressure is that of condensation. In the case of a metal no sensible change is produced thereby, owing to the already highly condensed state; but with vegetable fibrous substances like gutta-percha or india-rubber the effect is highly sensible. These materials are naturally of a porous character, but when subjected to pressure become less so, and consequently tend to absorb less

water, the presence of which, especially as regards salt water, naturally lowers the resistance of such materials; moreover, the mere fact of increased homogeneity is sufficient to account for increased resistance by less loss of current laterally.

The late Sir William Siemens first demonstrated the effect of pressure on insulating substances in a paper on the subject read before the British Association in 1863, based on experiments on the gutta-percha core of the first Malta-Alexandria cable, as well as on the Carthagena-Oran G.P. core, and on certain india-rubber cores. Herein he pointed out how the resistance of gutta-percha was materially increased by pressure, and in direct proportion to the degree of the same, which was varied as desired in the pressure tanks of Reid. The formula, given in the paper before us, expressing the increase of the resistance of gutta-percha by pressure, was given by Siemens as the result based on these experiments, where the coefficient 0·00023 was the amount of increase in resistance found to occur on a unit resistance by a unit pound pressure. It will be seen, therefore, that this formula was not one of the many introductions due to Mr. Latimer Clark, as stated in the paper, but is due rather to Sir W. Siemens. Moreover, the coefficient therein contained only applies to " ordinary " gutta-percha, and does not apply to cores with other insulating mediums, such as india-rubber; indeed, hardly more than the temperature coefficient would apply to other insulators. Messrs. Bright and Clark tested the first Persian Gulf core under pressure, finding the gutta-percha to be increased in resistance 2·6 per cent. for every 100 lbs. pressure; whereas Siemens's experiments showed the dielectric resistance of the Malta-Alexandria core to be increased only 2·3 per cent. thereby. In Bright and Clark's experiments 600 lbs. was, however, the maximum pressure, being equivalent to only 224 fathoms of sea water; whereas in those of Siemens a pressure equal to about 1,700 fathoms was applied. The most useful way in practice of expressing the increase of the resistance of gutta-percha by pressure on submergence is, I think, to say that it is increased 6·2 per cent. for every 100 fathoms depth of sea water. It is not absolutely certain whether the pressure test as applied to a core

represents quite the same thing as a sheathed cable when submerged. In this test it is necessary to keep the core for some time under the required pressure before noting the effect on the dielectric resistance, as the application of pressure increases the temperature for the time being, and consequently a change in resistance at that time would be partly due to the change of temperature, just as the withdrawal of the pressure also has the effect of temporarily lowering the temperature of the water. At present the dielectric resistance of cores is only increased by pressure during the actual time of its application. It is not improbable, however, that the resistance of inferior insulating materials might, by special apparatus, be *permanently* increased by pressure. This would prove, however, an expensive operation, probably. Most forms of india-rubber cores are more absorptive than a gutta-percha core, owing largely to the fact that the rubber, as a rule, is of necessity laid on the wire in strips either spirally or longitudinally, thus involving a continuous seam instead of being applied round the wire in a homogeneous tubular form as in the case of a gutta-percha core. Siemens found that some forms of masticated india-rubber were *de*creased in resistance by pressure at a rate about one-fourth of that at which gutta-percha is increased by a corresponding amount. The formula expressing the decrease of resistance of such india-rubber by pressure would be .

$$R\,p = R\left[1 - (0{\cdot}000061 \times p\text{ lbs.})\right],$$

where R is the original resistance, $0{\cdot}000061$ the cofficient of decrease of resistance on unit resistance (1 megohm) by unit pressure (1 lb.), p the number of lbs. pressure applied, and $R\,p$ the *de*creased resistance by pressure. Fairbairn found, on the other hand, that some forms of india-rubber were very slightly *in*creased in resistance by pressure. In any case the resistance of any description of india-rubber can be but little improved by pressure due to submergence as compared with gutta-percha, owing to its higher absorptive power. Most mixtures of vulcanised india-rubber being more homogeneous and less absorptive (like gutta-percha) are not, however, affected materially by pressure in either direction.

The nature of the physical change produced by a change of

temperature on vegetable substances of the gutta-percha and india-rubber order is that of cold contracting the material and heat expanding it. Thus by contraction the pores of the material are closed by its molecules being brought closer together, and consequently it is more compact in its texture, its absorptive tendency is proportionally decreased, and as a result the electrical conductivity is decreased. The effect of heat on fibrous materials, such as gutta-percha and india-rubber, is that of expansion separating the pores and thus increasing the absorptive tendency and lowering the resistance by this and by the decreased homogeneity of the texture. The fact that temperature exerts an opposite influence on the conductivity of metals and vegetable materials may be to some extent explained by the fact that vegetable materials (such as gutta-percha) contain moisture, a good conductor in itself. By pressure or low temperature this moisture is expelled, thus decreasing the conductivity. The fact that the resistance of gutta-percha or india-rubber varies by change of temperature according to a logarithmic curve rather than by direct proportion—*i.e.*, is increased at a rate so much higher at low than at high temperatures—can be physically accounted for by the fact that it is only when low temperatures are reached that the moisture begins to be expelled. The conductivity of copper is affected by temperature according to the same rule; this is, probably, by reason that the substance is not materially condensed until low temperatures are reached. No degree of pressure expels any large amount of moisture from gutta-percha or india-rubber, and the conductivity of copper is not found to be affected at all by pressure.

In 1863 Messrs. Bright and Clark conducted a series of experiments on the gutta-percha core of the first Persian Gulf cable in order to determine precisely the nature and degree of the variation of the resistance of gutta-percha due to temperature. The results of these experiments showed that the resistance of gutta-percha was increased very considerably by a decrease of temperature, and *vice versâ*—not in simple proportion to each alteration of temperature, but by compound interest, as shown in the diagram of Mr. Carpenter's paper.

The resistance of india-rubber is somewhat similarly increased by a fall of temperature and decreased by a rise thereof, but mixtures of india-rubber when vulcanised are considerably less affected by temperature than gutta-percha. India-rubber when vulcanised being less affected by temperature and pressure than gutta-percha or unvulcanised rubber, it is much less improved as an insulator by submergence under any considerable depth of water. According to Mr. Bruce Warren's determination, the coefficient for variation of resistance by temperature of the favourite form of Hooper's vulcanised rubber was less than half that for the variation of gutta-percha resistance. The degree of variation of resistance by temperature (as also by pressure) of india-rubber of the present day, even when vulcanised, differs very much, however, for the different "mixtures." The amount of variation by pressure and temperature depends principally on the degree of specific resistance, and on the amount of moisture contained by them: thus, mixtures with a low resistance are very much more affected than those with a very high resistance (in the same way that gutta-percha, which often has a resistance at 75° Fah. about $\frac{1}{70}$th of that of Hooper's rubber, is more affected by temperature); thus the coefficient for variation of resistance by temperature of some forms of vulcanised india-rubber, with a resistance but little above that of gutta-percha, is more than double that for vulcanised rubber with the highest resistance, or nearly as much as that for gutta-percha.

The same applies, only to a less extent, as regards the different gums of gutta-percha. Gutta-percha gums with a comparatively low resistance are more improved, as a rule, by low temperature and pressure on submergence than those with a very high resistance. The explanation of this fact is that the lower resistance materials contain, very usually, more moisture, or are also more absorptive. Thus pressure or cold temperature increases their resistance more, by driving out moisture and preventing absorption.

This may apply to some extent as regards other materials with a much lower resistance (as well as to inferior mixtures of india-rubber), but unfortunately their resistances cannot, as a rule, be

relied upon to remain at their sufficient height for any considerable time; besides which their electrostatic capacity is usually too high.

For these reasons the coefficient of variation of resistance represented by the curve in the diagram attached to Mr. Lant Carpenter's paper cannot be said to apply to gutta-percha in general, but only to the particular gum on which the experiments were made, from which the coefficient corresponding to the curve was deduced. Owing to the alteration in the gums at different times it would be desirable, strictly speaking, to calculate a separate coefficient for variation of resistance by temperature for each separate core made. Considering, however, that the resistance of gutta-percha varies so much by age (owing to other circumstances besides changes of temperature), such calculations and corrections of resistance for temperature can only be correct for the time at which the degree of variation of the resistance by temperature was first noted. I do not think this fact is sufficiently appreciated.

The greater the depth to which a cable with a gutta-percha core is submerged, the greater the reduction in the difference between the share taken by temperature and pressure respectively in increasing the insulation resistance. This is owing to the fact that whereas the temperature decreases rapidly at depths near the surface, it falls only very gradually at greater depths, until often below a certain depth (usually about 2,000 fathoms) it falls no further. The pressure, however, on a submerged cable increases in direct proportion to the depth to which it is submerged in the ocean; consequently the insulation resistance is by pressure increased, accordingly, in direct proportion to the depth, whereas by temperature this is evidently not the case. Indeed (as inferred above), it will by temperature be often increased to the full extent at a depth of 2,000 fathoms; so that at a depth of 3,000 fathoms it will not be further increased by temperature, though materially further by pressure. This would still more apply in the case of vulcanised india-rubber core cables (the resistance of india-rubber when vulcanised being naturally so much less affected by temperature than that of gutta-percha), which at great depths might

be almost as much increased in insulation resistance by pressure as by the bottom temperature.

In quite shallow tropical waters, cores (especially in the case of india-rubber) with a high absorption are said sometimes to be somewhat decreased rather than increased in resistance by submergence, owing to the absorption not being overcome by any great pressure or low temperature. On the other hand, india-rubber usually stands low temperatures (such as below 32° Fah.), physically and mechanically, better than gutta-percha does.

Temperature affects the electrostatic capacity of a cable in so far as its affects the resistance of the insulating medium. That is to say, materials, as a rule, with the highest resistance usually, although not always, offer facilities for the lowest capacity, and *vice versâ*. Moreover, when the resistance of gutta-percha is increased as by a decrease of temperature, its capacity also is, as a result, at the same time decreased (owing to a specific change in the material), though in a much less degree. It would probably be decreased in a much more proportional degree to the increase of resistance but for the fact of the cold temperature having also, by contraction, the effect of bringing the two plates of the Leyden jar closer together. The capacity of india-rubber is less altered than gutta-percha by temperature, in fact, scarcely at all—some mixtures are increased and others decreased in capacity thereby, but all in a very small degree.

In his experiments with pressure Sir W. Siemens did not find that the capacity of either gutta-percha or india-rubber was affected by pressure. It is probable, however, that pressure, inasmuch as it increases the resistance of gutta-percha, will, like a low temperature, tend indirectly to be the cause of a slight decrease of capacity; but whereas pressure produces so much less effect than temperature on the resistance of gutta-percha, its effect on the capacity is imperceptible; indeed, the same counteracting tendency by condensation of the Leyden jar taking place, the capacity can, in any case, be but very slightly affected by pressure. Inasmuch as pressure tends to decrease the resistance of some forms of india-rubber, it also sometimes tends to increase the capacity thereof, in the same way that a cold temperature does.

15th November, 1888.

At a Special General Meeting of Members, held at the Institution of Civil Engineers, 25, Great George Street, on Thursday, November 22nd, 1888—Mr. E. GRAVES, President, in the Chair—

The PRESIDENT moved that the following resolution, passed at the Special General Meeting of Members on the 8th inst., be now confirmed, viz.:—" That the name of the Society be changed to " ' The Institution of Electrical Engineers.' "

The motion, having been seconded by Dr. JOHN HOPKINSON, was carried unanimously.

The PRESIDENT moved that the following resolution, passed at the Special General Meeting of Members on the 8th inst., be now confirmed, viz.:—" That the office of Honorary Secretary be " abolished, and that the Articles of Association be altered by " omitting all reference to the Honorary Secretary in Articles " 36, 38, 40, and 43."

The motion, having been seconded by Sir DAVID SALOMONS, Bart., was carried unanimously.

The One Hundred and Eighty-second Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 22nd, 1888—Mr. EDWARD GRAVES, President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The names of new candidates for election into the Society were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—
Major S. Flood-Page.

From the class of Students to that of Associates—
Ernest Matthew Lacey.

The PRESIDENT: I have now to announce that at the Special General Meetings of Members held on November 8th and 22nd the following special resolutions were respectively passed and confirmed:—

   1. " That the name of the Society be changed to ' The Insti-
        " ' tution of Electrical Engineers.' "

   2. " That the office of Honorary Secretary be abolished,
        " and that the Articles of Association be altered by
        " omitting all reference to the Honorary Secretary in
        " Articles 36, 38, 40, and 43."

But although the resolutions have been passed and confirmed, the name of the Society cannot be officially changed until the consent of the Board of Trade has been obtained and a new certificate issued by the Registrar of Joint Stock Companies. It is hoped that these formalities will be completed by the 1st January

next, and that on and from that date the title of the Society will
be " *The Institution of Electrical Engineers.*"

The following paper was then read :—

ON A SYSTEM OF ELECTRICAL DISTRIBUTION.

By HENRY EDMUNDS, Member.

It has been remarked that it is undesirable to bring a paper of
this kind before a scientific society on a subject which is only in
its initial or untried stage.   While there may be some justice in
this remark, I think that in bringing this paper before this Society
to-night I may have the opportunity of laying before you the
results of certain experiments and actual work now extending
over some ten or twelve months, and of pointing out to you what
have been the results of that work, and what the indications are
with regard to its usefulness and extension.

In designing this system of central station lighting it has
been my endeavour to develop a plan of effectively and economi-
cally distributing the electric light over a given area, which should
practically isolate each installation and make it independent and
self-contained; which should obviate the use of objectionable
machinery on the consumer's premises; which should ensure
freedom from powerful and dangerous currents of electricity;
which should provide a reliable supply of the electric current at
all hours of the day or night; and which would enable a simple
and readily understood form of meter to be used that would com-
mand the confidence of the consumer.

One of the great desiderata of a system of lighting is constant
supply.   It is not sufficient to supply the illuminant for a specified
number of hours per day; what the householder wishes to have is
a means by which he has always at hand a reliable source of light,
capable of supplying all his requirements night and day.   To
ensure this it would be necessary either to have the generating
machinery always at work during the whole twenty-four hours, or
to employ some system of storage.   Even if the machinery were
constantly kept running, however, that would not render the con-
stancy of the supply absolutely reliable, for the machinery might

break down owing to accident or wear, and the district be deprived of light until the necessary repairs were made. Therefore it is essential that a reserve of electricity, independent of the generating plant, should be at hand, and such a reserve can only be obtained by the use of storage batteries.

The subject of electric lighting in conjunction with storage batteries is one that is receiving considerable attention from a good many engineers at the present time. The desirability of working with them has, I think, been admitted by most engineers for a long period, but the difficulty in connection with their use has been great. Among other things, the durability and efficiency of the battery itself has been more or less an unknown quantity, and this has caused people to hesitate before embarking upon any large amount of work dependent on them. It is hardly my province now to go into the history of the several methods which have been tried, but we may simply look on the two broad modes of working, viz., charging batteries in multiple, or charging them in series. As we do not in this system propose to charge in multiple, we can leave that out of the question. We may therefore consider the mode that we are here adopting of charging batteries in series. It has been found to be most economical to use comparatively small conductors with suitable current, and high electromotive force enabling one to carry the current over long distances from central stations, and to supply the current at different points on the line as may be required for local work or use.

An opportunity for distributing current over a large area was presented to me some months ago, where a central station was available, suitable as to its position and size, but a long way from where it was intended to supply the bulk of the current for lighting purposes. It was necessary to consider the respective advantages of direct supply with low tension and high tension in conjunction with transformers. The former was out of the question on account of the distance, and the latter was at that time in disfavour, owing to several breakdowns and interruptions of the light to consumers having been reported in London. It was therefore deemed desirable to work in conjunction with a reserve, such as storage batteries in some form or other, and the

idea occurred to me that a system might be developed which would practically be analogous to the transformer system, but using direct in lieu of alternating currents, and charging batteries in series with a constant current on one main line, rather than in parallel, and simply increasing the E.M.F. as the work increased and as the length of the line extended. This might be done in several ways—by dividing the line into sections, charging each portion for a good number of hours according to the probable demand, or by an arrangement for supplying current from the main to a portion of any group of cells less than the whole; but here a difficulty occurs, for it is most undesirable to be charging batteries as a whole or in part at the same time that they might be wanted for discharging. To obviate this, the batteries might be duplicated, so that one portion might be in the charging main and the other in the local. But this, again, is limited by the number charged in series at the same time. Besides this, the cells that have been charged have a large difference of potential when brought into the local, which causes a fluctuation of lights when such changes occur.

This brings us to the system which I have devised. Sets of accumulators are placed in groups at various points or sub-stations throughout the district to be lighted, and charged in sections from the central station. The secondary batteries at each sub-station are divided into four equal groups, with any convenient number in each group. For instance, we may have four sets of eight cells each, of which three sets, or 24 cells, will always be in connection with the lamp circuit, and supplying current locally to those houses which are in connection with the sub-station; while the fourth section will be in the main circuit, in series with the other sub-stations, being charged by the current from the central station. At short intervals the charged group is switched out of the main circuit into the supply, its place being taken by one of the discharged groups. In order that the three groups in the supply circuit shall be as far as possible in an equal state of charge, it is essential that the changes should take place at frequent intervals, otherwise one group would become highly charged while the other three are being nearly exhausted.

Therefore I have designed an apparatus by which each group is brought in succession into the charging circuit, charged for a short period—two minutes in actual practice—and then removed again into the supply circuit, in order that it may discharge itself. This apparatus, which works automatically, I call a " distributor."

The operation of the distributor may be understood from this diagram. You will see that the four groups of cells are lettered A, B, C, and D. H H is the main charging conductor, H² H² the branch conductor, and L the local installations. Y is a resistance.
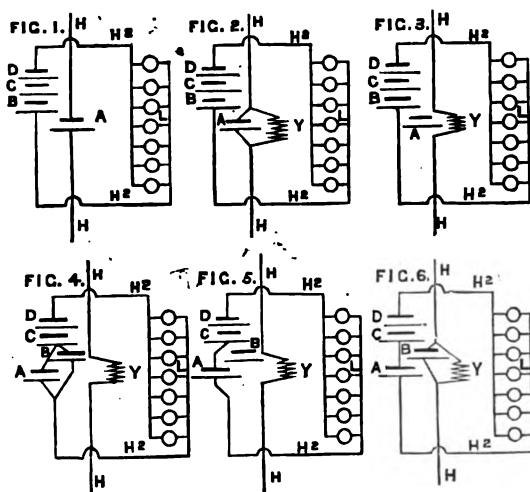
DIAGRAM OF CYCLES.

In Fig. 1 the battery A is shown in the main conductor being charged, while batteries B, C, and D are connected together in series, discharging locally. In Fig. 2 the resistance Y is connected in parallel with the battery A, thus giving a by-path for the main current, which now divides itself between the two circuits, viz., the resistance and the battery. In Fig. 3 the battery A is now disconnected from the charging main, the resistance Y carrying the whole of the current, and maintaining the continuity of the charging circuit. You will observe that battery A is now free, and is to be removed to the local circuit. In Fig. 4 this is effected by coupling it in parallel with battery B. You will observe here that the two negative poles of

the batteries A and B are coupled together to the local negative, while their positives are coupled together to the negative of local C. We have thus been enabled to introduce A into the local circuit without causing any alteration in the E.M.F, of the series, or consequent fluctuations in the local circuit.

We can now, as in Fig. 5, isolate the battery B from the local battery, A having taken place, maintaining the continuity of the local. Battery B is now connected with the main in parallel with resistance Y, as in Fig. 6. Resistance Y is then cut out, leaving battery B to receive the full charging current of the main H H, it practically being the substitute for battery A, as in Fig. 1.

These cycles of changes are essential to the efficient carrying out of this system, for we must guard against interrupting the main charging circuit, because of other work going on in the circuit at the same time in series with the battery under consideration. We must also be careful at no time to alter or fluctuate the pressure of the local batteries. We must also at no time have the charging main in contact with the local batteries, as this might cause a variation in their pressure, or in an extended system have a tendency to leaking through the local to earth, with its attendant dangers. You will see, therefore, that by carrying out this cycle we ensure all the requisite conditions.

In practice this cycle of changes takes place at intervals of about every two minutes, the batteries A, B, C, and D receiving their increments of charge, A for two minutes, B for two minutes, C the same, and D the same; so that in eight minutes each of the cells will have received two minutes' charge. The charge now proceeds in the reverse direction from D to A; thus the cycle is A, B, C, D, D, C, B, A, &c.

Although the process of describing in detail the changes which are shown in the diagrams, and which complete one cycle, have been somewhat lengthy, it must not be supposed that the actual change in the mechanical-electrical actions of the distributor which bring about this result are of any protracted period of time. If so, the value of the system would be neutralised. The actual time occupied is less than one second, so that in twenty-four hours the distributor instrument is only actually under move-

ment for twelve minutes. This practically consumes very little
time, and prevents any appreciable wear of the moving parts.
The time mechanism runs on jewelled bearings, and has therefore
little wear.
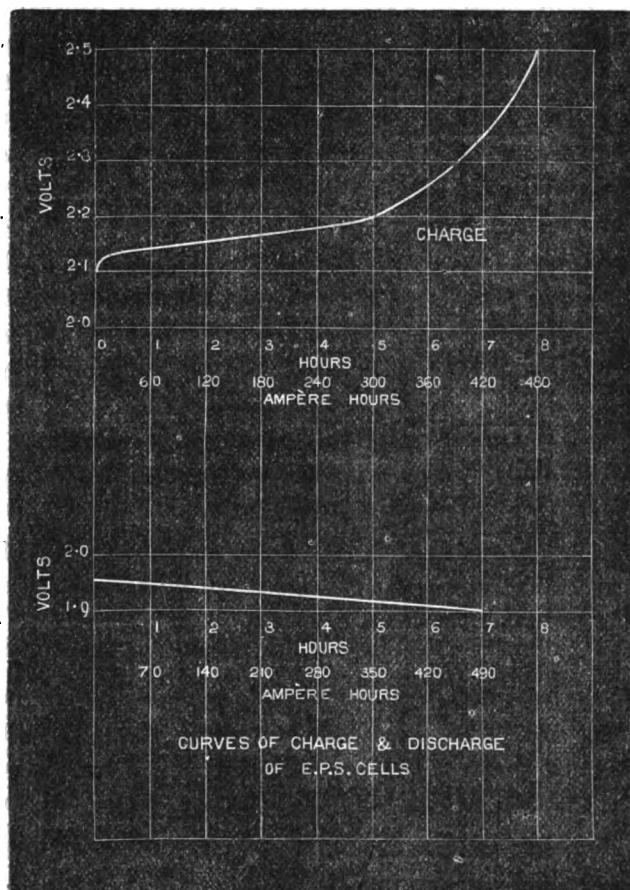
We have here a probably well-known diagram (Fig. 7), supplied
to me by the Electrical Power Storage Company, which shows the
rise of potential when charging with a 60-ampere current in the
same size of cells that we are using in our system ; and here you
will observe that when the battery is being discharged, and when
at the end of its discharge the voltage has fallen, it stands at 1·9, as

against 2 volts when fully charged, after a period of 490 ampere-hours' discharge. The battery is now put in to be charged, and the result is that the counter-electro-motive force required to charge a cell with a 60-ampere current would be 2·1. Now, as the charging is continued for a period of time, it gradually rises up to 2·2—that is, after five hours. Then we have a very sudden rise in the next three hours, bringing up the charging potential to 2·5. But in practice (I am speaking now of where batteries are charged either separately, in the ordinary method, or charging and discharging at the same time) this is rather a serious drawback, because if you have got that increase of voltage per cell, and you have 50 cells, you have a considerable difference of potential at the time of charging as compared with when the batteries are at work and not being charged. Many devices and arrangements have had to be made, such as cutting out cells when charging, putting in resistance, or other things, in order to approximately balance the local potential.

I find that by the method of charging we have here we are not troubled in that way. In the first place, a cell receiving a 60 or a 70-ampere current, such as we use, for a period of two minutes, shows no rise in E.M.F. You will observe by the table a rise of one-tenth occurs in five hours, but in two minutes there is hardly any rise at all; then it comes into the discharging circuit. So the cycles continue, and it will be observed that in twenty-four hours we only charge a group of cells, as a whole, with a six hours' current. On the other hand, we only want one-third of the charging potential in the main. This enables us to do a given amount of work with one-third of the plant, and one-third of the pressure at the charging station, compared with what would be required if we were doing the same amount of work by a direct system. This is very important. The cells are kept in much better condition by continually receiving fresh increments of charge, and having periods of rest from discharge. Also, the loss due to difference of potential between the charging and discharging current is minimised, it being found in practice that we can continue charging the cell intermittently to its maximum capacity with a mean voltage of 2·25, instead of 2·5 per cell, as is required where cells

are charged continuously. This, on a large scale, is a considerable element of economy when there are a large number of cells in the charging main. Besides this, we find that an approximately constant potential on the local circuit enables us to run lamps at a higher efficiency without the risk of rupture due to sudden rise of pressure, and, so far as we can judge, the life of the batteries is considerably lengthened by this mode of intermittent charge and rest. We can keep up the voltage at 48 or 50 without varying, and can run lamps at a higher efficiency without fear of over-stepping the mark, being sure that we shall not go above a certain amount—*i.e.*, that with lamps sent out by the makers as requiring 46 volts we can put them on a 48-volt circuit. This gives greater efficiency, and we are not sacrificing the life of the lamp, because the perfectly steady current that we get prolongs very much the life of the lamps, which averages fully 1,000 hours at the high efficiency of about 2½ watts per candle.

It is obvious that running a smaller station during the whole twenty-four hours is much more economical than running a large station only for a few hours during such time as the current is con-sumed. The current being always constant in volume enables a single main of one size to be used, preventing complications, and only requiring an increase of work as the pressure extends, which pressure, however, as has been pointed out, is only one-third of the sum of the pressures of the work done on the local circuits. For example, suppose batteries were used in 100 centres, with an average of 50 volts each, giving the sum of 5,000 volts altogether, it would require at a central station, to charge these all together in series, a potential of 6,250 volts, exclusive of the line ; while by this mode of charging we can effect the same results with a charging potential not exceeding 2,000 volts, exclusive of the line. The safety ensured through this is of the highest impor-tance, both in removing high pressures at the central station and through the differences of potential between the earth and the line at various points, considerably cheapening the cost of insulating the main conductors, and freeing the system from danger of leakage to earth, with its attendant dangers, enabling a lower grade of insulation to be used for the interior wiring, and

getting rid of the vexed question which all contractors are feeling with regard to complying with the requirements of the insurance offices. This is a considerable factor of economy in extended work, and is much more important than at first sight appears. Many people are objecting to the introduction of electric lighting more out of consideration to the first cost of installation—which is now enhanced by the many difficulties experienced in complying with the conditions of safety, where extremely high tension currents are used—than on account of the cost of the current as a matter of consumption.

I think that is something to be appreciated. We find that from time to time we have had to increase the insulation of the conductors that are used; and as these very high tension currents are being introduced in London, and just now there are proposals for something very high indeed, I suppose greater care than ever will have to be taken. In this system I think we can effect all that can be done with the very high tension current transformer systems without having that at all, because the local circuits have only their local voltage, and have nothing to do with the high potential at the central station used for charging them as a whole. The current supplied from the dynamos at the central station is 70 amperes, this being the amount by which the large-sized E.P.S. cells are best charged. The voltage of the dynamo varies according to the work at any one time on the line, and the current is maintained constant at all times. A simple mode of ensuring these results has been obtained by using an ordinary series dynamo whose field is excited from a separate source—by batteries, for example, incidentally charged from the main—and the speed of the dynamo is varied in accordance with the variations of the work or external resistance of the circuit.

The accompanying diagram (Fig. 8) represents a steam engine, boiler, and dynamo to illustrate one or two things that are important. The current that we find best in practice is 70 amperes; the E.M.F. varies according to the work at any one time on the line; and the current is maintained constant at all times. You will observe that the engine has got no governor to it at all. That is not an accident on the part of the draughtsman, but it is a very important

feature in what I am going to
describe. It is found that by
proportioning the size of the
engine—that is, the area of
the piston—to the current
which it is desired to pro-
duce, 70 pounds of steam can
be taken as the boiler pressure
for 70 amperes on the circuit.
Now, if I simply open my
steam valve so that I ensure
getting 70 pounds direct on
to the piston, and if I short-
circuit the dynamo by a short
loop, and then start the engine,
the engine cannot run away.
The dynamo being short-
circuited acts like a brake,
and the engine is held back,
rotating very slowly, and at
the same time generating a
70-ampere current in that
short loop. Now, if I put
in work—that is to say, if I
put in resistance in the shape
of one group of batteries—the
result is that the resistance
causes the current to drop,
causes the brake action of the
dynamo to be released, and the
engine immediately speeds
itself, until it again is brought
up by the 70 amperes. And
as the ohmic resistance in-
creases as we put in more
work the engine goes ahead
and runs up to meet the
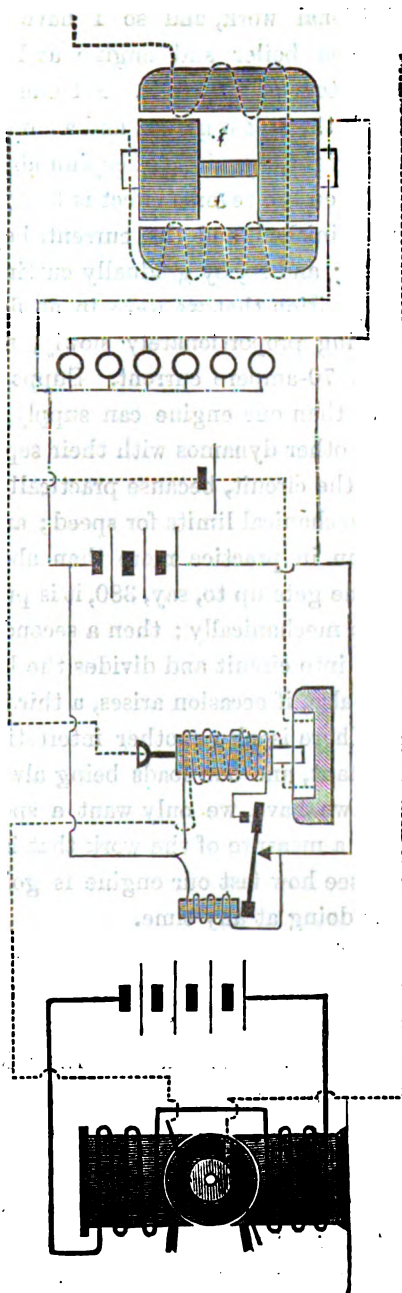
FIG. 8.

additional work, and so I have only to put a steam regulator between boiler and engine and I can do away with electric regulators of all kinds. All one need do is to tell the man to watch the steam gauge, and as long as he keeps that at 70 pounds all is right, the circuit keeping also at 70 amperes.

Then the reverse effect is that if work is cut out the resistance is diminished, and the current, beginning to increase, acts like a brake; and so, by gradually cutting out our work, coming back to the position that we were in at first, we have our engine simply running proportionately slowly, meanwhile grinding out a constant 70-ampere current. Supposing, however, we want more work than one engine can supply, then all that we need do is to have other dynamos with their separate engines ready to be put into the circuit, because practically this engine would get beyond its mechanical limits for speed; and as we do not want the engine to run in practice more than about 350 revolutions, when this engine gets up to, say, 380, it is probably going as fast as it ought to do mechanically ; then a second engine throws itself automatically into circuit and divides the load, and the two take the work. And also, if occasion arises, a third one comes in ; and so on.

There is also another interesting feature. Our current being constant, and our loads being always proportional to the voltage that we have, we only want a speed counter to give us at any time a measure of the work that is going on in the circuit. We can see how fast our engine is going, and know exactly the work it is doing at any time.

Of course it may be said that a system of this kind is essentially dependent on the mechanical details used for carrying the same into effect, and here actual practice has been of very great use to me. The Cadogan Electric Light Company, of Chelsea, having adopted this system, has enabled me to see actually what such shortcomings were. The apparatus I have here to-night is of the kind that has been in use there, and, like all first forms, has not performed as well in practice as it did with the care and attention it received in the laboratory.

The apparatus here (Fig. 9) consists of a powerful electro-magnet driving a set of cams, which operate four levers, each lever

carrying a full set of combination of contacts for each of the groups of batteries in use at one time.   This reciprocating lever arrange-
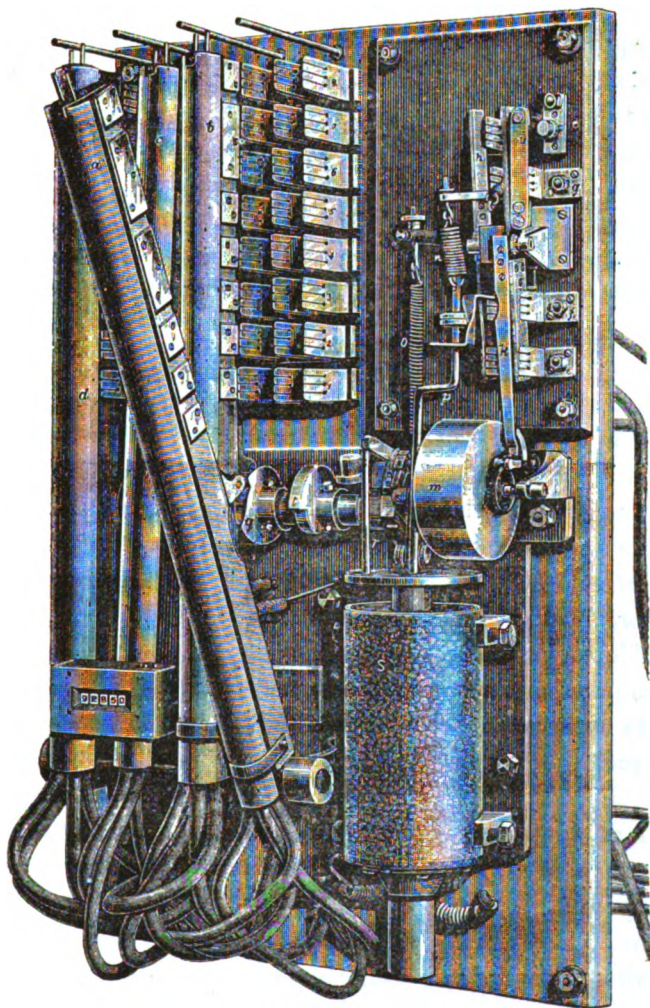
FIG. 9.—Distributor.

ment, however, has shown a serious difficulty, for the levers have occasionally wedged themselves so tightly that the electro-magnet could not draw them out, or, on the other hand, have proved too slack, so that insufficient contact was made, and sparking and heating caused in the apparatus.   All this, I am glad to say, has

been got over by the substitution of a rotary for a reciprocal motion, and now I can show you an improved form of apparatus, which is doing the work here this evening, which carries out all the requirements of this system in a much simpler manner. When any group of cells are sufficiently charged so that they ought to be disconnected from the charging main, an arrangement called a "voltage regulator" is brought into use. This causes the batteries to be disconnected from the main, and to remain disconnected until such time as they are discharged sufficiently to reduce their E.M.F. to a lower level, when the apparatus automatically causes the battery to be again put into the charging main. It will be obvious that such a system as this is sufficiently elastic by varying the number of cells to give practically any potential that may be demanded, and can therefore comply with all kinds of requirements for lighting, arc or incandescent, or supplying current for other purposes. It is very desirable not to have too great a variety of pressures, and therefore in houses where but a small amount is demanded it is best to put down a plant of sufficient size to supply several together from one distributor, thus forming a small station; it being, however, always desirable to have the same as near as possible to the point of consumption, and much cheaper to carry our charging main there than to have the local discharge mains. It is convenient not to have such stations too large, or at too great a distance from their work. For suburban or scattered lighting this system is very useful, as only one main is required of, say, three-eighths of an inch cross section, which can be conveniently carried overhead or underground.

The apparatus here (Fig. 10) is the time mechanism arranged for giving the periods of change and of charge, and in conjunction with it is an arrangement which acts like a meter. We have seen that we have here a constant current, and we also know that the movement of this magnet takes place every two minutes; therefore it is only necessary during such time as the charging current may be on to put a small solenoid which we have here, carrying a worm and gear, in connection with the counter, which counts the number of revolutions. As a revolution is made once every 16

minutes, we take 70 amperes × 16 minutes, and in that way, as the apparatus counts automatically for the whole time, we, know-
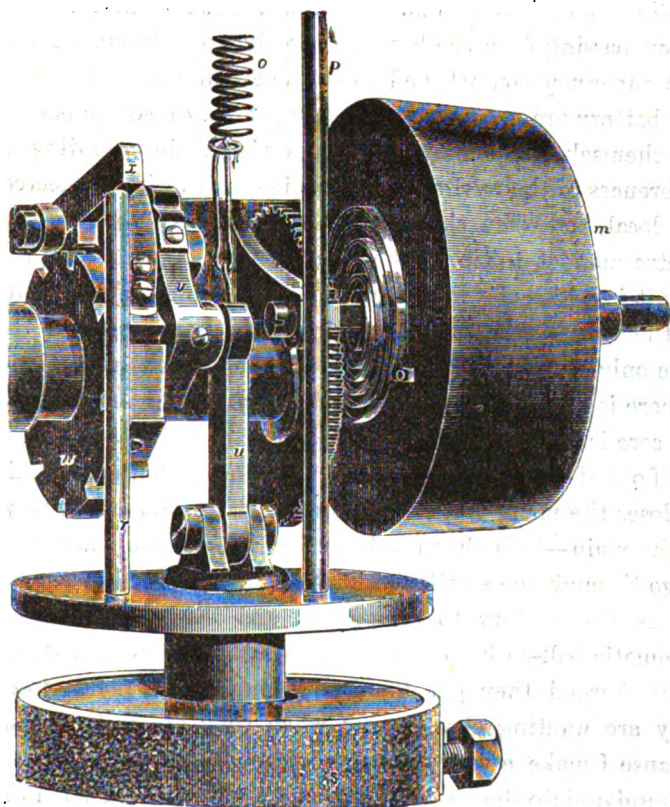
Fig. 10.—Time Mechanism.

ing the voltage, have an exact measure of the current that is going into the house. That does away with all the difficulty of differentiating the meter, and of explaining that which is a very difficult thing to explain, viz., where the current goes to that goes out of the batteries, and it removes a good deal of trouble to the contractors in explaining about the loss in the consumer's batteries due to storage. By this method we simply charge for what goes in, and leave the consumer to take care of what goes out.

If our system be very much extended I find that we can, by changing the direction of the current from the dynamo—which is

done by simply changing the connections so that the fields are reversed—then bring into operation another arrangement, which I call a "polarised switch." You will observe that the current is now passing from the brush of the dynamo through a solenoid into a mercury contact, and on to the circuit; *i.e.*, this distributor and battery are not now being charged. I cause the batteries to put themselves in and out of the charging main according to the differences of their voltage. There is a small wire connected with the local, and when the voltage is sufficiently high to cause an electro-magnet to tip up the lever, contact is made with a small circuit in which the wire is wound in a contrary direction to that of the solenoid in the main wire : the consequence is that we have only to bring in a small wire along which a fraction of an ampere is passing and there is no sparking, and the result is that the core is immediately drawn up. In the latter operation it cuts itself out of the local battery that would have been in the main, and it closes the main ; but when the voltage in the local battery—not on the main—falls, then the lever does not act and the current has to go through the smaller wire, which neutralises the current and causes the battery to be put in, and the whole acts like an automatic ball-cock to a water cistern. When the batteries are fully charged they cut themselves out of the main, and when they are wanting charge—not when they are fully discharged, because I make a very great distinction there—then they throw themselves into the main. Now, supposing that we have a theatre, or anything of that kind, to light, where it would not be economical to put batteries down which would supply the maximum number of lights required, but where it would do, say, to put batteries down sufficient for their supplying an average number of lights, and which would prevent the theatre from being in the dark and causing panic ; suppose that a greater demand on these lamps comes than the batteries could supply, then we have an arrangement included in the circuit which I call a "motor-"generator." This is practically a motor and dynamo combined in one. When the charge from these batteries exceeds a certain point, then this motor-generator is thrown into circuit, and it is operated by the current from the main circuit, generating

current in the local, reinforcing the batteries, and keeping a steady current by working through them on to the larger number of lights. Thus the motor-generator practically takes the place of a secondary generator in an alternating-current system. This, I think, is a field which may be very fairly developed in the future. Or suppose that we have more houses on a circuit than our engines can charge in series at one time, we can have these polarised switches so arranged that when the current goes in one direction it would charge a set of houses in the daytime, for example, and when reversed cause the day set to leave the main and another, or night, set to come into the main. That is, of course, where twelve hours' charge is enough—say when four hours' discharge is wanted per day. We will thereby be enabled to supply a circuit twice the length that our plant could do normally; *i.e.*, we can run two sets of houses on the same circuit, and by reversing the current cause half the houses to go in at one period and the other half to go out, and *vice versâ*, and in that way we can extend very largely the operations from one station.

In this paper I have not gone into the question of cost, but, from the experience we have had, think that such a system compares most favourably in this respect with any of the direct- or alternating-current methods, and I hope it will prove a means of supplying that great demand which all engineers feel is now upon us.

The PRESIDENT: I think the discussion on the paper we have The President just heard is sure to be interesting : perhaps it will be opened by some member who is not on the Council.

Mr. A. RECKENZAUN: I can only congratulate Mr. Edmunds Mr. Reckenzaun. for the ingenious manner in which he has carried out this problem. I have had the privilege of seeing the apparatus at work a few weeks ago at his place at Hatton Garden, and I have been very much surprised at its absolutely accurate working. The only objection that I found in the apparatus was that it appeared too delicate. When it is placed in a station where the accumulators might be in close proximity to the apparatus there is the possibility of the fumes which always exist tarnishing

Mr. Becken-saun. the metal.   In regard to the movement, I anticipated that there would be considerable sparking, because Mr. Edmunds told me that his charging current was 70 amperes; and I was very much surprised to find that the sparking is almost invisible when the levers are moving from one position to the next. That is done by a resistance which is automatically shunted in during the operation.   Mr. Edmunds has mentioned that the batteries are charged with a constant potential of 2·25 volts, whereas ordinarily the charging would have to be done with a potential of 2·5 volts.   Now I think that is rather erroneous.   The electro-motive force, as the charging proceeds, as it has been described on the diagram, rises from 2·1 to 2·5 or 2·6; but I believe that it would be difficult to maintain the charging electro-motive force at 2·25 volts, because the counter-electro-motive force of the battery is practically that, and sometimes a little more, owing to the hydrogen gas which is always, in a nearly fully charged cell, on the surface of the negative plates, and which produces a fictitious electro-motive force.   I have no other remarks to make, and can only repeat that I felt very much interested in the subject.

Mr. Preece.   Mr. W. H. PREECE: I am sorry to say, Sir, that I have kept no particular notes of the points that Mr. Edmunds has raised in this paper.   A similar paper was read by him at the Bath meeting of the British Association, and he described there very fully his system.   It was very well discussed, and I took that opportunity to express my admiration at the way in which the details had been carried out.

There is something remarkably enticing in the system. Broadly, the principle has about it the merit, not only of novelty, but of success, and it also has been worked out in a manner that reflects the very highest credit on Mr. Edmunds. But we always find in every new system that if you attempt at once to attain completion and perfection you are sure to introduce complication and expense.   The first thing in a novelty is to make it perfect.   When you have made it perfect, and you find it expensive, the next thing is to try and lop off the excrescences, and bring it down to within the range of practice.   I think this

plan of Mr. Edmunds, excellent as it is, is still in the experimental Mr. Preece. stage, where it is laden rather with excrescences that will have in practice to be cut off; and I think that he deserves a considerable amount of thanks from us for bringing a new thing like this before us in order that we may think it out for ourselves and probably assist him, as we all ought to do, in perfecting an advanced thing.

Mr. Reckenzaun has remarked upon one point that I must say I feared very greatly, and that was that the constant removal of a battery and its replacement in a circuit would produce rather intense sparks, because the current of 70 amperes is rather a severe one, and we always find, when we attempt to change the direction of a current in a circuit, either by a switch or any other contrivance, that invariably there is considerable sparking; but there are no sparks here—at least, they are not perceptible—and I think that that is probably due to the fact that in the changes that are made by removing one cell or one battery out something is done that is not affected by that pest of all electro-magnetic apparatus, self-induction.

Another practical defect is the tendency that there is to make things too delicate. In carrying out changes of this kind you want something extremely substantial, very well made, and very well worked out; and I can see by the apparatus before me that Mr. Edmunds is gradually working it up into a more practical stage than the first form of apparatus I saw in Hatton Garden was in. The next point that occurred to me, and about which I confess I should like to have heard a little further explanation from Mr. Edmunds, is the means by which, without governor, without compound winding, without any contrivance of any kind, he succeeds in maintaining a constant current in his circuit. As regards the automatic switches that are used, the object is undoubtedly extremely valuable: we would like to have seen an apparatus of this kind used over and over again for different purposes; but I must say, as a practical man, that I dislike the introduction of any complicated instrument to add to the complication of an already complicated apparatus, and I think the process itself should be simplified as much as possible;

Mr. Preece. and though it is a very nice thing to use plant over and over again, it would be better without that complicated motor.

With regard to those curves and to the counter-electro-motive force, or, rather, to the voltage of batteries. There is, I cannot help thinking, a misconception existing about the behaviour of batteries when they are charged and when they are discharged. Those curves are evidently curves that have been drawn from actual experiments. The form of the curve showing the rise of volt-age hour by hour, as the battery is charged, is a curve that is per-fectly familiar to me, and is one that I have reproduced over and over again; but there is another curve wanting that is of more consequence than even the curve of voltage, and that is the curve of current. If you take a record of the variation of voltage, and also of the variation of current, during the time a battery is charging, you will find that while the voltage is increasing the current is diminishing, and the result is that the energy per unit of time at any particular interval is very nearly the same. Now, when you discharge, you discharge a battery at a very much lower voltage; and the general impression is that when you discharge a battery at a very much lower voltage than that battery takes when it is charged, you get a great loss of energy. Well, as a matter of fact you do not, for a secondary battery has one remarkable point connected with it that has not received the attention it deserves. It is this—that a battery is a kind of auto-matic governor: that the internal resistance of a battery that controls the external work done by the battery varies with the current that goes through it, so that if you discharge a battery with a small current you get one internal resistance; if you discharge it with a larger current you get very much less internal resistance. The number of watts taken out at a given time are practically the same, and the result is that you might get out of a battery very nearly the whole of the energy put into it. I have succeeded in extracting from a battery, using very small currents, energy equal to 98 per cent. of that put in. I do not think, Sir, there are any other points upon which I care just now about making any remarks.

There are several practical suggestions that Mr. Edmunds has

raised. One appeared to me to be rather serious: I did not quite Mr. Preece.
catch what he said, but the serious point is really the difficulties
that are placed, not only in the way of contractors in carrying
out work, but in the way of consulting engineers who have to
scheme out that work. We find that restrictions are increasing
day by day in respect of the character and quality of wire that
seem to be quite unjustifiable. For instance, I have only recently
heard that somebody has planted his foot down upon the use of
pure india-rubber. Well, now, it is rather a curious thing that we
in the Post Office, who have had some experience in the use of
insulating materials of different kinds, specify only pure india-
rubber for our aerial cables that have to carry currents; whereas
we are now told that pure india-rubber is entirely tabooed, it must
not be used, and that nothing but vulcanised india-rubber must be
employed. That may be all very right, it may be very true and
very just; but at the same time we practical men who have had
thirty years' or more experience of pure india-rubber would like to
know why that substance that we have used so much turns out
now, in the year 1888, to be bad. Last year, when I spoke
here, I referred to the merits of a new class of wire that was being
introduced as a mode of maintaining the insulation by coating
the insulating compound with lead. I referred more particularly
to the remarkable insulating and non-inflammable properties
that the refuse of petroleum possessed, and we know that it is
only possible to maintain the perfection of this by a lead coating.
We have had some experience of lead coating, but it has been
tabooed also; and I must own that I should like to learn from
some of these attempts to use high-tension currents, especially
converters and transformers, the results from some practical
men who have really tested the merits of lead-covered wire and
found out if it possesses defects or not.

Mr. GISBERT KAPP: Mr. Preece has spoken about the system Mr. Kapp.
of working a steam engine without a governor as if it were some-
thing doubtful or dangerous. I may say that some years ago
Mr. Bernstein has already done it, and there is really no difficulty
about it; it is a perfectly easily understood thing. There is a
delightful simplicity about the rule that for each pound of

Mr. Kapp. steam in the boiler the dynamo gives 1 ampere. The pressure against the piston being constant, the torque on the armature is constant; and as the field is also constant (being always excited from the same number of cells), the current must be constant, and on this account the engine will give no trouble at all. But I think it will give trouble in another way, viz., in the coal bill. Engines can be worked economically within certain limits of power, degree of expansion, and speed; but if the limit, especially as regards speed, be widely departed from, and if an engine is worked just crawling round, you will find that the weight of water used per horse-power-hour will enormously increase. The reason is that the steam has time to condense in the cylinder. The great advantage of high-speed engines, such as the Willans, is that the steam has no time to condense, or, rather, that very little of it is condensed in comparison with the amount utilised. Although the cylinder may be of a metal which conducts heat very quickly, yet the steam has not time enough to give away many heat units, and at these fast speeds the engine works almost as if it were made of a non-conducting material. At slow speeds, on the other hand, the proportional loss through condensation is very great; and although if the engine runs very slowly it is a comfortable thing for the man in charge, it is not so comfortable for the man who has to pay the coal bill. It is a questionable advantage to work in this manner, though the working is perfectly feasible on scientific grounds.

While I am on this subject I should like to put a question to Mr. Edmunds. If I understand him rightly, he says that the generating plant—by which he means the boiler, engine, and dynamo—need only be a third of what would be required in a battery system arranged on what is generally known as the Colchester plan, where you have two batteries, one being charged while the other is working the local circuit. On first looking at Mr. Edmunds's plan it seems that his argument is right; but on further consideration I am not quite sure that it is so, and I should be glad if Mr. Edmunds could, in his reply, dispel my doubts. One of the conditions of working these batteries is that they should receive only small increments of charge for two

minutes, and during the next six minutes discharge these incre- Mr. Kapp.
ments again, in order that they may not accumulate a charge or
become overcharged, and that the counter-electro-motive force of
the battery may not rise up to the high point of the curve. If
that be só—if the batteries are not to be used as accumulators of
large quantities of electricity—then I cannot see how Mr.
Edmunds can save anything in the size of the generating
plant. This plant would have to be exactly as powerful as if
it were used for direct lighting. If, on the other hand, you allow
an accumulation of the charge, then the argument that the
batteries are used in a better way in this case falls to the
ground; and, in fact, the circumstance that Mr. Edmunds puts
a regulating magnet in, which can only come into play upon a
rise in the E.M.F., shows that he anticipates that the batteries
will accumulate a large charge. In this case each cell will give a
greater electro-motive force than 2·2 volts, and by the rise of
pressure consequent upon an accumulation of charge the cells will
cut themselves out. The question is, How great is the rise in
pressure? and I should like to ask the author whether the magnet
of his " voltage regulator " can be so delicately adjusted that it will
come into play upon a pressure of, say, 2·25 volts, corresponding
to the knee of the curve, before the rapid rise of E.M.F. begins.
If that is so, I can quite understand that the lamps can be
worked at a higher voltage than that at which they are
labelled, and that the light will be exceedingly steady. But
in that case we come upon the other difficulty, viz., that we
cannot charge up our batteries during the daytime and draw
on the charge during the night; *i.e.*, we live from hand to
mouth, so to speak, and as much as the engine puts out
the consumer takes in the evening, and there is practically
very little storage. I should like to draw the attention of
the meeting to the very beautiful magnet adopted by Mr.
Edmunds in the voltage regulator. I am under the impression
that I have seen this type of electro-magnet before; but, whether
it be original or not, it has a feature that is very well worth our
attention, and may be useful for many purposes. The author has
in this magnet got over what Mr. Preece calls the pest of

Mr. Kapp. magneto-electric apparatus, viz., self-induction. Here is a mag-
net with one circuit round it always closed, therefore in that
circuit there can be no sparking ; you have also another circuit of
high resistance, in which there is a contact or switch. When the
current flows through both circuits they act differentially and
there is no magnetism in the iron core, and therefore on
breaking contact the spark is quite insignificant, the shunt
circuit at this moment acting as if it had only an inert resistance.
When the shunt current is broken there is a good deal of mag-
netism in the core and self-induction in the shunt coil, but then
there is no current flowing through this coil. To start the
current we must close the contact, but on closing again we
can, of course, have no spark ; so that we have here an electro-
magnetic instrument which can be put in and out of action
and has all the advantages of such instruments without their
disadvantage of self-induction. I am rather sorry that Mr.
Edmunds has put a motor-generator into the diagram. The
great object of his system is, apparently, to do away with the
necessity of relying upon the continuous working of the
machinery at the station. Apart from this, we do not want
moving machinery in the houses, not even in a theatre, if we can
help it ; and whatever arguments might be used in favour of this
motor-generator in one particular installation will hold good all
over the line, and then you would have no batteries at all, and
would work by motor-generators only. One very great advantage
of the author's system is that he can have perfect insulation
between the local circuit and the high-tension main circuit, but
with a motor-generator it is impossible to get this high insulation.
Mr. Edmunds has alluded to transformers and the thin insulating
partition between the two circuits. The difficulty of obtaining
perfect insulation in transformers is considerable, but can be over-
come. With a motor-generator having a revolving armature sub-
jected to high mechanical strains the difficulty is immeasurably
greater, and I doubt that it would be safe to use a motor-
generator on a circuit which may at times carry a pressure of 2,000
volts.

Mr. W. P. GRANVILLE : I should like to ask how many addi- tional contacts this system would introduce into each sub-section, and also how many houses, of average size, such sub-section could conveniently supply.

Mr. W. L. MADGEN : I do not wish to say anything tending to revive the discussion between the adherents of the alternating and battery transformer systems, as both parties seem to have buried the hatchet, determined to work their respective methods and leave time to show which is best. But a marked change has taken place in the aspect of the B.T. system since the discussion on Mr. Crompton's and Mr. Kapp's papers. It was then implied that, as the function of the cells would be principally that of " potential reducers," the entire loss by conversion would not be incurred; but that position has now been abandoned, and I understand it to have been made a rule that house circuits are not to be connected with the battery at the same time as the primary or high-tension main. Mr. Edmunds seemed to justify the use of house conductors of a lower grade insulation with his system than are now being generally used in connection with the alternating-current service; but I do not suppose that the extra price for equally well insulated conductors will make very much difference, and the tendency to use less reliable is at least one which should not be encouraged.

Mr. D. C. BATE : I should like to have one point cleared up. It seems that not only will this system of secondary battery distribution not reduce the cost, but that it is liable to increase it, not only over transformer, but also over other battery systems. Here, if the batteries are discharged at more than an average rate of one-third of the maximum charging current, we shall not only have no accumulation, but we shall have a loss, and therefore, taking 70 amperes as being put in, the battery must not discharge at more than 20 odd amperes. The cells would therefore be very much larger than if, as is done in the Colchester system, the batteries were allowed to discharge at their full rate and then replaced by others of equal capacity; by which means it seems that we could get twelve hours' charge and discharge (Mr. Preece, however, says we can get 98 per cent. efficiency at a low

r. Bate,   discharge), whereas here we could only get six hours' charge out of twenty-four. There is one other point, and that is the solenoid which has to cut out half the houses, roughly, by the current in the dynamo being reversed. It would seem that if Mr. Edmunds anticipates running his station for twenty-four hours there would be no necessity for reversing the current, or anything of that sort, because as each battery is charged the solenoid would cut it out; he would therefore be able to put on the same number of batteries as if he charged for twelve hours on one set and twelve hours on another. I must frankly admit that I still do not understand his engine running without a governor, and I hope Mr. Edmunds will give some of us who do not understand it further information on the point.

Mr.
Trotter.   Mr. ALEX. P. TROTTER : I can corroborate from practical experience what Mr. Kapp has said about constant current. Some time ago I took great pains to make an accurate constant-current regulator for Mr. Bernstein, but found that it was much better to throw off the governor and let the engine go ahead with an open throttle-valve. How far that would answer on a large scale I cannot say. This is an extremely ingenious system, and one would like to look into the figures. Great stress has been laid upon the insulation, and that the main is quite apart and separate from the batteries ; but I understand that 6,000 volts go "fooling round" somewhere inside this beautiful arrangement on the table, and I should like to know the actual distance of the insulation in inches. The main, I apprehend, gets inside the box with the other contacts. I do not wish to be flippant, as I cannot but admire the extreme ingenuity which has been brought to bear on the invention.

Professor
Forbes.   Professor G. FORBES : I am glad to take this opportunity of saying that I think we are all agreed that the subject which has been introduced to us this evening is one which is absorbing attention more now than ever, and one to which, we look forward to such fertile inventors as Mr. Edmunds, to bring us a great deal of new materials in the future. Having now become fairly satisfied with the performance of one system of distribution, we naturally look forward to the time when we shall be able to

distribute on an alternative plan at some future date. It seems Professor
Forbes.
but a very short time since the question of distribution was being
discussed and seemed quite hopeless, and I think I may safely
say that all of us will feel a great relief when the time does arrive
that a system of distribution—Mr. Edmunds's or any other which
is combined with storage batteries—gives us a more efficient and
a more economical system than the system of distribution which
is alone at our disposal at present. The chief difference which I
notice, in the general principle, between Mr. Edmunds's system
and other systems which have been proposed for using secondary
batteries, is that by this ingenious plan of charging and discharg-
ing cells alternately at very short intervals of time, the cutting
out of cells is, as he tells us, altogether avoided. In every other
system of using accumulators which I am acquainted with, that is
the trouble which has to be met—that when you first put the fully
charged cells into circuit they have a higher voltage than they
have after a short time, and that a certain proportion—one or two
cells—have to be cut out ; and the complicated devices which have
been attempted to be used to accomplish this end have, so far as
I have seen them at present, not been successful. There are
some, I believe, which have lately been brought out which I have
not yet investigated, but Mr. Edmunds's system is one which is
free from that defect. There are a great many points which it is
almost impossible to discuss without having considered the paper
far more fully in detail than one can by simply hearing it read ;
but a question has been raised by several speakers which I also
should like some information about, and that is the advantage of
using the cell, both in charging and discharging, at about a half-
charge, and never over that. It seems to me that the batteries are
never used at a higher charge than one-half their charge ; and it
also seems to me that we are depending in this system upon the
capacity of the battery, and therefore we are depending upon
only half the charge which they are capable of using. I noticed
Mr. Edmunds making some expression of dissent when a similar
remark was made by another speaker, but I fail to see the way
out of the difficulty, and I should be very glad to hear his answer
to that. Incidentally I was extremely pleased to hear Mr.

Professor
Forbes.

Edmunds make himself responsible for the statement that he has lamps which are working with 2¼ watts per candle-power, and whose life is considerably over 1,000 hours. This is news to me, and I would be glad to know something more about the lamps. The question as to the *modus operandi* of Mr. Edmunds's self-regulation has been answered by Mr. Kapp. When he was describing this it was new to me—I did not know of Mr. Bernstein's experiment—and what occurred to me at the moment was that this is exactly the converse of the result in motor work, which to the best of my recollection was first announced by Marcel Deprez. When you have a series motor, or a motor which is excited by a constant current, you may vary the load as much as you please and the volts will go up, but the current will remain perfectly constant; the pressure may be made to vary as much as you please and the current will remain perfectly constant. This is exactly the converse thing—*i.e.*, applied to a dynamo. That was the case of a motor working against constant pressure; this is the case of a dynamo being driven by the constant pressure of a steam engine. I should like, if Mr. Edmunds would give it, a little detail information about the method which he mentioned of automatically introducing a second and third engine to assist the work. It seemed to me that he passed it over a little lightly, and it does not appear quite such a simple affair as he would lead us to believe. The weakest point in the necessary apparatus of the system seemed to me to be the make-and-break which I think Mr. Kapp has approved of so much. It does seem to me that in an apparatus which is to be used day and night from month to month, to put anything in the consumer's house in the shape of a make-and-break is to be condemned at once. That it is objectionable has been accepted generally in the case of meters, and I should say also in apparatus of this sort, which is the very basis of the success of the system. I agree also with the remarks of Mr. Kapp about the motor-generator, but I look upon that as being a subsidiary thing, and not essential to the main feature of the system. I think it is quite right, as Mr. Preece has said, that every detail should be suggested, and it is quite easy to lop them off as time goes on and we

find that they are not of any use. In conclusion, I think that the first general feeling one has about this system is that there is a little too much complexity in the apparatus which is to be put into every individual house; and it would have been well, I think, if Mr. Edmunds could have given us some approximate information of the cost of this apparatus which is to be added to all the ordinary apparatus with which a house is installed. It is impossible to give any offhand opinion as to the relative cost of this system which Mr. Edmunds proposes, and of other systems—the cost of installing and the cost of working—it is not the duty of those who are discussing the paper to give detail figures on that point; but I think it would have been a good thing if we had had two approximate estimates, even though they are merely of a theoretical nature, to give us some basis on which to judge about the relative cost.   But the cost of the plant and the cost of working it are things which will have to be worked out afterwards, and if these be satisfactory the complexity of the apparatus is not very serious.   I think that the system in its general features is one of very great beauty indeed. · By the way, it nearly escaped me about the motor-generator.   I would ask Mr. Edmunds if he does not think that having those machines on such a very high potential circuit, it would be extremely difficult to get perfect insulation for them.   I fancy it would be.   If I have been a little critical about some points of this paper, it was not my intention to be so; I am extremely delighted with the general scheme which has been put before us; and I am sure everyone else here will look forward with great interest to see the development of Mr. Edmunds's system, and how it works out in commercial practice.

The PRESIDENT: Are you prepared to reply to the discussion, Mr. Edmunds?

Mr. H. EDMUNDS: Through the kindness of the Electrical Power Storage Company I have brought some cells here to-night, but they cannot remain after this evening; and as I believe several gentlemen wish to look more closely into the apparatus while it is in operation, and as there are many questions raised in the discussion to answer, I should prefer, if agreeable to yourself and the meeting, to postpone my reply to the next meeting.

The PRESIDENT: I presume there will be no objection on the part of the meeting to Mr. Edmunds taking that course?

The meeting signified assent.

A ballot took place, at which the following were elected :—

Associates:

Alfred Inigo Baron.	John Melville Smith.
E. T. Mackrill.	Douglas Lewis Wells.

Students:

William Brew.	Harris Henry Eley.
George Bliss Winter.	

The meeting then adjourned.

ABSTRACTS.

HERTZ'S RESEARCHES ON ELECTRICAL OSCILLATIONS.*

By G. W. de Tunzelmann, B.Sc.

H. Hertz has been engaged for some time past in a series of researches on electrical oscillations, which have led to results of very exceptional interest, as was pointed out by Professor Fitzgerald in his inaugural address to Section A of the British Association last week; and as these results throw considerable light on the nature of electrical action, it will be of interest to readers of *The Electrician* to have a connected account of the investigations, to which I therefore propose to devote a short series of papers.

In Hertz's first paper on the subject, viz., "On Very Rapid Electrical "Oscillations" (Wied. *Ann.*, vol. xxxi., page 421, 1887), he refers to a paper by Colley, "On some New Methods for Observing Electrical Oscillations, "with Applications" (*ibid.*, vol. xxvi., page 432), who calls attention to the fact that Sir William Thomson in 1853 first showed the possibility of producing electrical oscillations by the discharge of a charged conductor, and gives references to all the investigations in the same direction which were known to him.

For the benefit of readers who may wish to pursue the subject further the list is reproduced below :—

Sir W. Thomson, "Mathematical and Physical Papers," vol. i., page 540.

Feddersen, Poggendorff's *Annalen*, vol. ciii., page 69, 1858; vol. cviii., page 497, 1859; vol. cxii., page 452, 1861; vol. cxiii., page 437, 1861; vol. cxv., page 336, 1862; vol. cxvi., page 132, 1862.

Kirchhoff, "Gesammelte Abhandlungen," page 168, containing remarks on, and corrections of some of Feddersen's results.

Von Helmholtz, "Gesammelte Abhandlungen," vol. i., page 531.

Von Oettingen, Poggendorff's *Annalen*, vol. cxv., page 115, 1862; and Jubelband, page 269, 1874.

Bernstein, Poggendorff's *Annalen*, vol. xiii., page 142, 1828.

Schiller, Poggendorff's *Annalen*, vol. clii., page 535, 1872.

L. Lorenz, Wiedemann's *Annalen*, vol. vii., page 161, 1879.

Mouton, Thèse, Paris, 1876. *Journal de Physique*, vol. vi., pages 5 and 46, 1876.

Kolacek, Beiblätter zu Wiedemann's *Annalen*, vol. vii., page 541, 1883 (abstract of a paper published in the reports of the Bohemian Scientific Society in 1882).

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* Having regard to the great interest and importance of Dr. Hertz's experiments, the Editing Committee have considered it desirable to reprint this excellent *resumé* by Mr. de Tunzelmann.—Ed.

Olearsky, *Verhandlungen der Academie von Krakau,* vol. vii., page 141, 1882.

Oberbeck, Wiedemann's *Annalen,* vol. xvii., pages 816 and 1040, 1882; vol. xix., pages 213 and 265, 1883.

Bichat et Blondlot, *Comptes Rendus,* vol. xciv., page 1590, 1882.

According to these investigations, the electrical oscillations produced in an open circuit by means of an induction coil are measured by ten-thousandths of a second; while in the case of the oscillatory discharge of a Leyden jar they are about a hundred times as rapid, as was shown by Feddersen.

According to theory, still more rapid oscillations should be possible in an open circuit of wire of good conducting material, provided its ends are not connected with conductors of any considerable capacity; but it is not possible to determine from theory whether measurable oscillations are actually produced. Some observations of Hertz's led him to believe that under certain circumstances oscillations of this kind were produced, and his researches show that this is so, and that the oscillations are about a hundred times as rapid as those observed by Feddersen; so that their periods are measured by hundred-millionths of a second, and, therefore, they occupy a position intermediate between acoustic and luminous vibrations.

Preliminary Experiments.—It is known that if in the secondary circuit of an induction coil there be inserted, in addition to the ordinary air space, across which sparks pass, a Riess spark micrometer, with its poles joined by a long wire, the discharge will pass across the air space of the micrometer in preference to following the path of least resistance through the wire, provided this air space does not exceed a certain limit; and it is upon this principle that lightning protectors for telegraph lines are constructed. It might be expected that the sparks could be made to disappear by diminishing the length and resistance of the connecting wire; but Hertz finds that though the length of the sparks can be diminished in this way, it is almost impossible
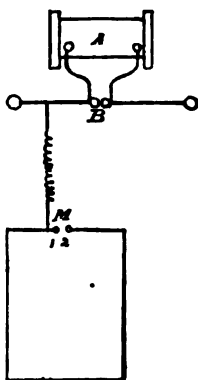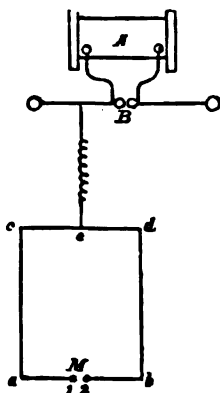
Fig. 1.   Fig. 2.

to get rid of them entirely, and they can still be observed when the balls of the micrometer are connected by a thick copper wire only a few centimetres in length.

This shows that there must be variations in the potential measurable in hundredths of a volt in a portion of the circuit only a few centimetres in length, and it also gives an indirect proof of the enormous rapidity of the discharge, for the difference of potential between the micrometer knobs can only be due to self-induction in the connecting wire. Now the time occupied by variations in the potential of one of the knobs must be of the same order as that in which these variations can be transmitted through a short length of a good conductor to the second knob. The resistance of the wire connecting the knobs is found to be without sensible effect on the results.

In Fig. 1, A is an induction coil and B a discharger. The wire connecting the knobs 1 and 2 of the spark micrometer M consists of a rectangle, half a metre in length, of copper wire 2 millimetres in diameter. This rectangle is connected with the secondary circuit of the coil in the manner shown in the diagram, and when the coil is in action, sparks, sometimes several millimetres in length, are seen to pass between the knobs 1 and 2, showing that there are violent electrical oscillations, not only in the secondary circuit itself, but in any conductor in contact with it. This experiment shows even more clearly than the previous one that the rapidity of the oscillations is comparable with the velocity of transmission of electrical disturbances through the copper wire, which, according to all the evidence at our disposal, is nearly equal to the velocity of light.

In order to obtain micrometer sparks some millimetres in length, a powerful induction coil is required, and the one used by Hertz was 52 centimetres in length and 20 centimetres in diameter, provided with a mercury contact-breaker, and excited by six large Bunsen cells. The discharger terminals consisted of brass knobs 3 centimetres in diameter. The experiments showed that the phenomenon depends to a very great extent on the nature of the sparks at the discharger, the micrometer sparks being found to be much weaker when the discharge in the secondary circuit took place between two points, or between a point and a plate, than when knobs were used. The micrometer sparks were also found to be greatly enfeebled when the secondary discharge took place in a rarefied gas, and also when the sparks in the secondary were less than half a centimetre in length; while, on the other hand, if they exceeded 1½ centimetres, the sparks could no longer be observed between the micrometer knobs. The length of secondary spark which was found to give the best results, and which was, therefore, employed in the further observations, was about three quarters of a centimetre.

Very slight differences in the nature of the secondary sparks were found to have great effect on those at the micrometer, and Hertz states that after some practice he was able to determine at once from the sound and appearance of the secondary spark whether it was of a kind to give the most powerful effects at the micrometer. The sparks which gave the best results were of a brilliant white colour, only slightly jagged, and accompanied by a sharp crack.

The influence of the spark is readily shown by increasing the distance

between the discharger knobs beyond the striking distance, when the micrometer sparks disappear entirely, although the variations of potential are now greater than before. The length of the micrometer circuit has naturally an important influence on the length of the spark, as the greater its length the greater will be the retardation of the electrical wave in its passage through it from one knob of the micrometer to the other.

The material, the resistance, and the diameter of the wire of which the micrometer circuit is formed have very little influence on the spark. The potential variations cannot, therefore, be due to the resistance; and this was to be expected, for the rate of propagation of an electrical disturbance along a conductor depends mainly on its capacity and coefficient of self-induction, and only to a very small extent on its resistance. The length of the wire connecting the micrometer circuit with the secondary circuit of the coil is also found to have very little influence, provided it does not exceed a few metres in length. The electrical disturbances must therefore traverse it without undergoing any appreciable change. The position of the point of the micrometer circuit which is joined to the secondary circuit is, on the other hand, of the greatest importance, as would be expected, for if the point is placed symmetrically with respect to the two micrometer knobs, the variations of potential will reach the latter in the same phase, and there will be no effect, as is verified by observation. If the two branches of the micrometer circuit on each side of the point of contact of the connection with the secondary are not symmetrical, the spark cannot be made to disappear entirely; but a minimum effect is obtained when the point of contact is about half-way between the micrometer knobs. This point may be called the null point.

Fig. 2 shows the arrangement employed, e being the null point of the rectangular circuit, which is 125 centimetres long by 80 centimetres broad. When the point of contact is at a or b, sparks of from 3 to 4 millimetres in length are observed; when it is at e no sparks are seen ; but they can be made to reappear by shifting the point of contact a few centimetres to the right or left of the null point. It should be noted that sparks only a few hundredths of a millimetre in length can be observed. If, when the point of contact is at e, another conductor is placed in contact with one of the micrometer knobs, the sparks reappear.

Now the addition of this conductor cannot produce any alteration in the time taken by the disturbances proceeding from e to reach the knobs, and therefore the phenomenon cannot be due simply to single waves in the directions $e\,a$ and $d\,b$ respectively, but must be due to repeated reflection of the waves until a condition of stationary vibration is attained, and the addition of the conductor to one of the knobs must diminish or prevent the reflection of the waves from that terminal. It must be assumed, then, that definite oscillations are set up in the micrometer circuit just as an elastic bar is thrown into definite vibrations by blows from a hammer. If this assumption is correct, the condition for the disappearance of the sparks at M will be that the vibration periods of the two branches $e\,1$ and $e\,2$ shall be

equal. These periods are determined by the products of the coefficients of self-induction of these conductors into the capacities of their terminals, and are practically independent of their resistances.

In confirmation of this, it is found that if when the point of contact is at *e*, and the sparks have been made to reappear by connecting a conductor with one of the knobs, this conductor is replaced by one of greater capacity, the sparking is greatly increased. If a conductor of equal capacity is connected with the other micrometer knob, the sparks disappear again; the effect of the first conductor can also be counteracted by shifting the point of contact towards it, thereby diminishing the self-induction in that branch. The conclusions were further confirmed by the results obtained when coils of copper wire were inserted into one or other, and then into both of the branches of the micrometer circuit.

Hertz supposed that as the self-induction of iron wires is, for slow alternations, from eight to ten times that of copper wires, therefore a short iron wire would balance a long copper one; but this was not found to be the case, and he concludes that, owing to the great rapidity of the alternations, the magnetism of the iron is unable to follow them, and therefore has no effect on the self-induction.

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In a note in Wiedemann's *Annalen*, vol. xxxi., page 543, Dr. Hertz states that since the publication of his paper in the same volume, he had found that von Bezold had published a paper in 1870 (Poggendorff's *Annalen*, vol. cxl., page 541), in which he had arrived by a different method of experimenting, at similar results and conclusions as those given by him under the head of Preliminary Experiments.

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Induction Phenomena in Open Circuits.—In order to test more fully his conclusion that the sparks obtained in the experiments described in my last paper were due to self-induction, Dr. Hertz placed a rectangle of copper wire with sides 10 and 20 centimetres in length respectively, broken by a short air space, with one of its sides parallel and close to various portions of the secondary circuit of the coil, and of the micrometer circuit, with solid dielectrics interposed, to obviate the possibility of sparking across, and he found that sparking in this rectangle invariably accompanied the discharges of the induction coil, the longest sparks being obtained when a side of the rectangle was close to the discharger.

A copper wire, *i g h* (Fig. 3), was next attached to the discharger, and a side of the micrometer circuit, which was supported on an insulating stand, was placed parallel to a portion of this wire, as shown in the diagram. The sparks at M were then found to be extremely feeble until a conductor, C, was attached to the free end, *h*, of the copper wire, when they increased to one or two millimetres in length. That the action of C was not an electrostatic one was shown by its producing no effect when attached at *g* instead of at *h*. When the knobs of the discharger B were so far separated that no sparking took place there, the sparks at M were also found to disappear, showing that

these were due to the sudden discharges, and not to the charging current. The sparks at the discharger which produced the most effect at the micrometer were of the same character as those described in my last paper. Sparks were also found to occur between the micrometer circuit and insulated conductors in its vicinity. The sparks became much shorter when conductors of larger capacity were attached to the micrometer knobs, or when these were touched by the hand, showing that the quantity of electricity in motion was too small to charge these conductors to a similarly high potential. Joining the micrometer knobs by a wet thread did not perceptibly diminish the strength of the sparks. The effects in the micrometer circuit were not of sufficient strength to produce any sensation when it was touched or the circuit completed through the body.

In order to obtain further confirmation of the oscillatory nature of the current in the circuit $k i h g$ (Fig. 3), the conductor O was again attached to h, and the micrometer knobs drawn apart until sparks only passed singly. A second conductor, O', as nearly as possible similar to O, was then attached to k, when a stream of sparks was immediately observed, and it continued when the
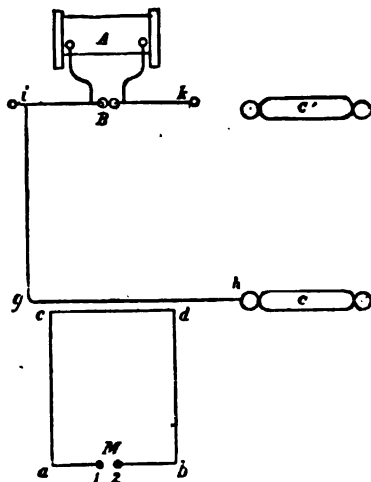
Fig. 3.

knobs were drawn still further apart. This effect could not be ascribed to a direct action of the portion of circuit $i k$, for in this case the action of the portion of circuit $g h$ would be weakened, and it must therefore have consisted in C' acting on the discharging current of C—a result which would be quite incomprehensible unless the current in $g h$ were of an oscillatory character.

Since an oscillatory motion between C and C' is essential for the production of powerful inductive effects, it will not be sufficient for the spark to occur in an exceedingly short time, but the resistance must at the same time not exceed certain limits. The inductive effects will therefore be excessively small if the induction coil included in the circuit C C' is replaced by an electrical machine

alternately charging and discharging itself, or if too small an induction coil is used, or, again, if the air space between the discharger knobs is too great, as in all these cases the motion ceases to be oscillatory.

The reason that the discharge of a powerful induction coil gives rise to oscillatory motion is that, firstly, it charges the terminals C and C′ to a high potential; secondly, it produces a sudden spark in the intervening circuit; and, thirdly, as soon as the discharge begins, the resistance of the air space is so much reduced as to allow of oscillatory motion being set up. If the terminal conductors are of very large capacity, for example, if the terminals are in connection with a battery, the current of discharge may indefinitely reduce the resistance of the air space, but when the terminal conductors are of small capacity this must be done by a separate discharge, and therefore, under the conditions of the author's experiments, an induction coil was absolutely essential for the production of the oscillations.

As the induced sparks in the experiment last described were several millimetres in length, the author modified it by using the arrangement shown in Fig. 4, and greatly increasing the distance between the micrometer circuit and the secondary circuit of the induction coil. The terminal conductors C and C′ were three metres apart, and the wire between them was of copper, 2 millimetres in diameter, with the discharger B at its centre.

The micrometer circuit consisted, as in the preceding experiments, of a rectangle 80 centimetres broad by 120 centimetres long. With the nearest side of the micrometer circuit at a distance of half a millimetre from C B C′, sparks two millimetres in length were obtained at M, and though the length of the sparks decreased rapidly as the distance of the micrometer circuit was increased, a continuous stream of sparks was still obtained at a distance of one
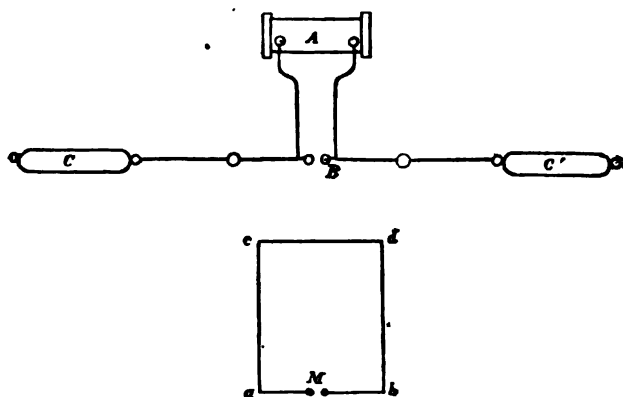
Fig. 4.

and a half metres. The intervention of the observer's body between the micrometer circuit and the wire C B C′ produced no visible effect on the stream of sparks at M. That the effect was really due to the rectilinear conductor C B C′ was proved by the fact that when one or other, or both halves of this conductor were removed, the sparks at M ceased. The same effect was

produced by drawing the knobs of the discharger B apart until sparks ceased
to pass, showing that the effect was not due to the electrostatic potential
difference of C and C', as this would be increased by separating the discharger
knobs beyond sparking distance.

The closed micrometer circuit was then replaced by a straight copper wire,
slightly shorter than the distance C C', placed parallel to C B C', and at a
distance of 60 centimetres from it. This wire terminated in knobs, 10
centimetres in diameter, attached to insulating supports, and the spark
micrometer divided it into two equal parts. Under these circumstances sparks
were obtained at the micrometer, as before.

With the rectilinear open micrometer circuit, sparks were still observed at
the micrometer when the discharger knobs of the secondary coil circuit were
separated beyond sparking distance. This was, of course, due simply to
electrostatic induction, and shows that the oscillatory current in C C' was
superposed upon the ordinary discharges. The electrostatic action could be got
rid of by joining the micrometer knobs by means of a damp thread. The con-
ductivity of this thread was therefore sufficient to afford a passage to the
comparatively slow alternations of the coil discharge, but was not sufficient to
provide a passage for the immeasurably more rapid alternations of the
oscillatory current. Considerable sparking took place at the micrometer when
its distance from C B C' was 1·2 metres, and faint sparks were distinguishable
up to 3 metres. At these distances it was not necessary to use the damp thread
to get rid of the electrostatic action, as, owing to its diminishing more rapidly
with increase of distance than the effect of the current induction, it was no
longer able to produce sparks in the micrometer, as was proved by separating
the discharger knobs beyond sparking distance, when sparks could no longer be
perceived at the micrometer.

Resonance Phenomena.—In order to determine whether, as some minor
phenomena had led the author to suppose, the oscillations were of the nature
of a regular vibration, he availed himself of the principle of resonance.
According to this principle, an oscillatory current of definite period would,
other conditions being the same, exert a much greater inductive effect upon
one of equal period than upon one differing even slightly from it.[*]

If, then, two circuits are taken having as nearly as possible equal vibration
periods, the effect of one upon the other will be diminished by altering either
the capacity or the coefficient of self-induction of one of them, as a change in
either of them would alter the period of vibration of the circuit.

This was carried out by means of an arrangement very similar to that of
Fig. 4. The conductor C C' was replaced by a straight copper wire 2·6 metres
in length and 5 millimetres in diameter, divided into two equal parts as before
by a discharger. The discharger knobs were attached directly to the secondary
terminals of the induction coil. Two hollow zinc spheres, 30 centimetres in
diameter, were made to slide on the wire, one on each side of the discharger,
and since, electrically speaking, these formed the terminals of the conductor,
its length could be varied by altering their position. The micrometer circuit

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[*] See Oberbeck, Wiedemann's *Annalen*, vol. xxvi., p. 245, 1885.

was chosen of such dimensions as to have, if the author's hypothesis were correct, a slightly shorter vibration period than that of C C'. It was formed of a square, with sides 75 centimetres in length, of copper wire 2 millimetres in diameter, and it was placed with its nearest side parallel to C B C', and at a distance of 30 centimetres from it. The sparking distance at the micrometer was then found to be 0·9 millimetre. When the terminals of the micrometer circuit were placed in contact with two metal spheres 8 centimetres in diameter, supported on insulating stands, the sparking distance could be increased up to 2·5 millimetres. When these were replaced by much larger spheres the sparking distance was diminished to a small fraction of a millimetre. Similar results were obtained on connecting the micrometer terminals with the plates of a Kohlrausch condenser. When the plates were far apart the increase of capacity increased the sparking distance, but when the plates were brought close together the sparking distances again fell to a very small value.

The simplest method of adjusting the capacity of the micrometer circuit is to suspend to its ends two parallel wires the distance and lengths of which are capable of variation. By this means the author succeeded in increasing the sparking distance up to three millimetres, after which it diminished when the wires were either lengthened or shortened. The decrease of the sparking distance on increasing the capacity was naturally to be expected; but it would be difficult to understand, except on the principle of resonance, why a decrease of the capacity should have the same effect.

The experiments were then varied by diminishing the capacity of the circuit C B C' so as to shorten its period of oscillation, and the results confirmed those previously obtained; and a series of experiments in which the lengths and capacities of the circuits were varied in different ways, showed conclusively that the maximum effect does not depend on the conditions of either one of the two circuits, but on the existence of the proper relation between them.

When the two circuits were brought very close together, and the discharger knobs separated by an interval of 7 millimetres, sparks were obtained at the micrometer, which were also 7 millimetres in length, when the two circuits had been carefully adjusted to have the same period. The induced E.M.F.'s must in this case have attained nearly as high a value as the inducing ones.

To show the effect of varying the coefficient of self-induction, a series of rectangles, a b c d (Fig. 4), were taken, having a constant breadth, a b, but a length, a c, continually increasing from 10 centimetres up to 250 centimetres: it was found that the maximum effect was obtained with a length of 1·8 metres. The quantitative results of these experiments are shown in Fig. 5, in which the abscissæ of the curve are the double lengths of the rectangles, and the ordinates represent the corresponding maximum sparking distances. The sparking distances could not be determined with great exactness, but the errors were not sufficient to mask the general nature of the result.

In a second series of experiments the sides a c and b d were formed of loose coils of wire which were gradually pulled out, and the result is shown in Fig. 6. It will be seen that the maximum sparking distance was attained for a somewhat greater length of side, which is explained by the fact that in the latter

experiments the self-induction only was increased by increase of length, while in the former series the capacity was increased as well. Varying the resistance
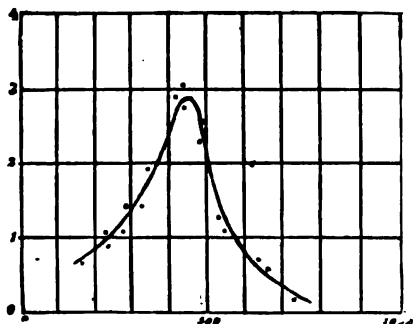
FIG. 5.

Curve showing relation between length of side of rectangle (taken as abscissa) and maximum sparking distance (taken as ordinate), the sides consisting of straight wires of varying lengths.

of the micrometer circuit by using copper and German silver wires of various diameters was found to have no effect on the period of oscillation, and extremely little on the sparking distance.
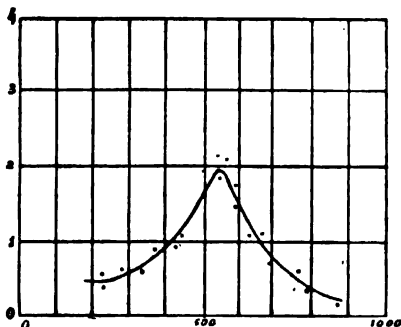
FIG. 6.

Curve showing relation between length of side of rectangle (taken as abscissa) and maximum sparking distance (taken as ordinate), the sides consisting of spirals gradually drawn out.

When the wire $c\,d$ was surrounded by an iron tube, or when it was replaced by an iron wire, no perceptible effect was obtained, confirming the conclusion previously arrived at that the magnetism of the iron is unable to follow such rapid oscillations, and therefore exerts no appreciable effect.

Nodes.—The vibrations in the micrometer circuit which have been considered are the simplest ones possible, but not the only ones. While the potential at the ends alternates between two fixed limits, that at the central portion of the circuit retains a constant mean value. The electrical vibration, therefore, has a node at the centre, and this will be the only nodal point. Its existence may be proved by placing a small insulated sphere close to various portions of the micrometer circuit while sparks are passing at the discharger of the coil, when it will be found that if the sphere is placed close to the centre of the circuit the sparking will be very slight, increasing as the sphere is moved further away. The sparking cannot, however, be entirely got rid of, and there is a better way of determining the existence and position of the node. After adjusting the two circuits to unison, and drawing the micrometer terminals so far apart that sparks can only be made to pass by means of resonant action, let different parts

of the circuit be touched by a conductor of some capacity, when it will be found that the sparks disappear, owing to interference with the resonant action, except when the point of contact is at the centre of the circuit. The author then endeavoured to produce a vibration with two nodes, and for this purpose he modified the apparatus previously used by adding to the micrometer circuit a second rectangle, $e f g h$, exactly similar to the first (as shown in Fig. 7), and joining the points of the circuit near the terminals by wires 13 and 24, as shown in the diagram.

The whole system then formed a closed metallic circuit, the fundamental vibration of which would have two nodes. Since the period of this vibration would necessarily agree closely with that of each half of the circuit, and, therefore, with that of the circuit C C', it was to be expected that the vibration would have a pair of loops at the junctions 13 and 24, and a pair of nodes at the middle points of $c d$ and $g h$. The vibrations were determined by measuring the sparking distance between the micrometer terminals 1 and 2. It was found that, contrary to what was expected, the addition of the second rectangle diminished this sparking distance from about 3 millimetres to about 1 millimetre. The existence of resonant action between the circuit C C' and the micrometer circuit was, however, fully demonstrated, for any alteration in the circuit $e f g h$, whether it consisted in increasing or in decreasing its length, diminished the sparking distance. It was also found that much weaker sparking took place between $c d$ or $g h$ and an insulated sphere, than between $a e$ or $b f$ and the same sphere, showing that the nodes were in $c d$ and $g h$, as expected. Further, when the sphere was made to touch $c d$ or $g h$ it had no effect on the sparking distance of 1 and 2; but when the point of contact was
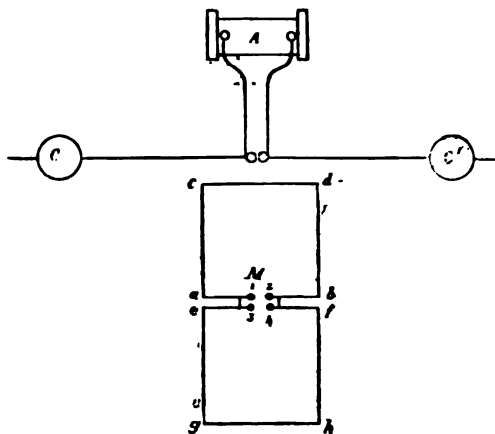
FIG. 7.

at any other portion of the circuit the sparking distance was diminished, showing that these nodes did really belong to the vibration, the resonant action of which increased this sparking distance.

The wire joining the points 2 and 4 was then removed. As the strength of

the induced oscillatory current should be zero at these points, the removal ought not to disturb the vibrations, and this was shown experimentally to be the case, the resonant effects and the position of the nodes remaining unchanged. The vibration with two nodal points was, of course, not the fundamental vibration of the circuit, which consisted of a vibration with a node between a and e, and for which the highest values of the potential were at the points 2 and 4.

When the spheres forming the terminals at these points were brought close together slight sparking was found to take place between them, which was attributed to the excitation, though only to a small extent, of the fundamental vibration. This explanation was confirmed in the following manner :—The sparks between 1 and 2 were broken off, leaving only the sparks between 2 and 4, which measured the intensity of the fundamental vibration. The period of vibration of the circuit C C' was then increased by drawing it out to its full length, and thereby increasing its capacity, when it was observed that the sparking gradually increased to a maximum, and then began to diminish again. The maximum value must evidently occur when the period of vibration of the circuit C C' is the same as that of the fundamental vibration of the micrometer circuit, and it was shown that when the sparking distance between 2 and 4 had its maximum value the sparks corresponded to a vibration with only one nodal point, for the sparks ceased when the previously existing nodes were touched by a conductor, and the only point where contact could take place without effect on the sparking was between a and e. These results show that it is possible to excite at will in the same conductor, either the fundamental vibration or its first overtone, to use the language of acoustics.

Hertz appears to consider it very doubtful whether it will be possible to get higher overtones of electrical vibration, the difficulty of obtaining such lying not only in the method of observation, but also in the nature of the oscillations themselves. The intensity of these is found to vary considerably during a series of discharges from the coil even when all the circumstances are maintained as constant as possible, and the comparative feebleness of the resonant effects shows that there must be a considerable amount of damping. There are, moreover, many secondary phenomena which seem to indicate that irregular vibrations are superposed upon the regular ones, as would be expected in complex systems of conductors. If, therefore, we wish to compare electrical oscillations, from a mathematical point of view, with those of acoustics, we must seek our analogy in the high notes intermixed with irregular vibrations, obtained, say, by striking a wooden rod with a hammer, rather than in the comparatively slow harmonic vibration of tuning forks, or strings; and in the case of vibrations of the former class we have to be contented, even in the study of acoustics, with little more than indications of such phenomena as resonance and nodal points.

Referring to the conditions to be fulfilled in order to obtain the best results, should other physicists desire to repeat the experiments, Dr. Hertz notes a fact of very considerable interest and novelty, namely, that the spark from the discharger should always be visible from the micrometer, as when

this was not the case, though the phenomena observed were of the same character, the sparking distance was invariably diminished. This effect of the light from the spark of an induction coil in increasing the sparking distance in the secondary circuit of another coil, excited great interest when referred to by Professor Lodge in the course of the recent discussion on Lightning Conductors at the British Association, and he pointed out that the same effect was produced by light from burning magnesium wire, or other sources rich in the ultra-violet rays.

Theory of the Experiments.—The theories of electrical oscillations which have been developed by Sir William Thomson, von Helmholtz, and Kirchhoff have been shown* to hold good for the open circuit oscillations of induction apparatus, as well as for the oscillatory Leyden jar discharge; and, although Dr. Hertz has not succeeded in obtaining definite quantitative results to compare with theory, it is of interest to inquire whether the observed results are of the same order as those indicated by theory.

Hertz considers, in the first place, the vibration period. Let T be the period of a single or half vibration proper to the conductor exciting the micrometer circuit; P its coefficient of self-induction in absolute electro-magnetic measure, expressed, therefore, in centimetres; C the capacity of one of its terminals in electrostatic measure, and, therefore, also expressed in centimetres; and v the velocity of light in centimetre-seconds.

Then, if the resistance of the conductor is small,

$$T = \frac{\pi \sqrt{P C}}{v}.$$

In the case of the resonance experiments, the capacity C was approximately the radius of the sphere forming the terminal, so that C = 15 centimetres. The coefficient of self-induction was that of a wire of length $l = 150$ centimetres, and diameter $d = 1/2$ centimetre.

According to Neumann's formula,

$$P = \iint \frac{\cos \epsilon}{r} d s \, d s',$$

which gives in the case considered

$$P = 2 l \left(\log. \frac{4 l}{d} - 0.75 \right) = 1,902 \text{ cm.}$$

As, however, it is not quite certain that Neumann's formula is applicable to an open circuit, it is better to use von Helmholtz's more general formula, containing an undetermined constant k, according to which

$$P = 2 l \left(\log. \frac{4 l}{d} - 0.75 + \frac{1 - k}{2} \right).$$

Putting $k = 1$ this reduces to Neumann's formula, for $k = 0$ it reduces to that of Maxwell, and for $k = -1$ to Weber's. The greatest difference in the values of P obtained by giving these different values to k would not exceed a sixth of its mean value, and, therefore, for the purposes of the present approximation it is enough to assume that k is not a large positive or negative number;

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* Lorentz, Wiedemann's *Annalen*, vol. vii., p. 161, 1879.

for if the number 1,902 does not give the correct value of the coefficient for the wire 150 cm. in length, it will give the value corresponding to a conductor not differing greatly from it in length.

Taking $P = 1,902$ cm. we have $\pi\sqrt{UP} = 531$ cm., which represents the distance traversed by light during the oscillation, or, according to Maxwell's theory, the length of an electro-magnetic ether wave. The value of T is then found to be 1·77 hundred-millionths of a second, which is of the same order as the observed results.

The ratio of damping is then considered. In order that oscillations may be possible the resistance of the open circuit must be less than $2\,v\sqrt{P/U}$. For the exciting circuit used this gives 676 ohms as the upper limit of resistance. If the actual resistance, r, is sensibly below this limit, the ratio of damping will be $e^{\frac{r\,T}{2\,P}}$. The amplitude will therefore be reduced in the ratio 1 : 2·71 in

$$\frac{2\,P}{r\,T} = \frac{2\,v}{\pi\,r}\sqrt{\frac{P}{U}} = \frac{676}{\pi\,r} = \frac{215}{r}$$

oscillations. We have, unfortunately, no means of determining the resistance of the air space traversed by the spark, but as the resistance of a strong electric arc is never less than a few ohms we shall be justified in assuming this as the minimum limit. From this it would follow that the number of oscillations due to a single impulse must be reckoned in tens, and not in hundreds or thousands, which is in accordance with the character of the experimental results, and agrees with the results observed in the case of the oscillatory Leyden jar discharge. In the case of closed metallic circuits, on the other hand, theory indicates that the number of oscillations before equilibrium is attained must be reckoned by thousands.

Hertz compares, lastly, the order of the inductive actions of these oscillations, according to theory, with that of the effects actually observed. To do this it must be noted that the maximum E.M.F. induced by the oscillation in its own circuit is approximately equal to the maximum potential difference at its extremities; for if there were no damping, these quantities would be identical, since at any moment the potential difference at the extremities and the E.M.F. of induction would be in equilibrium. In the experiments under consideration the potential difference at the extremities was such as to give a spark 7 to 8 mm. in length, which must therefore represent the maximum inductive action excited in its own circuit by the oscillation. Again, at any instant the induced E.M.F. in the micrometer circuit must be to that in the exciting conductor in the same ratio as that of the coefficient of mutual induction p of the two circuits to the coefficient of self-induction P of the exciting circuit. The value of p for the case considered is easily calculated from the ordinary formulæ, and it is found to lie between one-ninth and one-twelfth of P. This would only give sparks of from $\frac{1}{2}$ to $\frac{2}{3}$ mm. in length, so that according to theory visible sparks ought in any case to be obtained; but, on the other hand, sparks several millimetres in length, as were obtained in the experiments previously described, can only be explained on the assumption that the

successive inductive actions produce an accumulative effect; so that theory indicates the necessity of the existence of the resonant effects actually observed.

Dr. Hertz was at first inclined to suppose that as the micrometer circuit was only broken by the extremely short air space limited by the maximum sparking distance under the conditions of the experiment, it might therefore be treated as a closed circuit, and only the total induction considered. The ordinary methods of electro-dynamics give the means of completely determining the total inductive effect of a current element on a closed circuit, and would, therefore, in this case have sufficed for the investigation of the phenomena observed. He found, however, that the treatment of the micrometer circuit as a closed circuit led to incorrect results, so that it, as well as the primary, had to be treated as an open circuit, and therefore a knowledge of the total induction was insufficient, and it became necessary to consider the value both of the E.M.F. induction and of the electrostatic E.M.F. due to the charged extremities of the exciting circuit at each point of the micrometer circuit.

The investigations to which these considerations led are described by Dr Hertz in a paper, "On the Action of a Rectilinear Electrical Oscillation upon "a Circuit in its Vicinity," published in Wiedemann's *Annalen*, vol. xxxiv., p. 155, 1888.

In what follows, the exciting circuit will be spoken of as the primary, and the micrometer circuit as the secondary. Hertz points out that the reason that the electrostatic effect cannot be neglected is to be found in the extreme rapidity with which the electrostatic forces change their sign. If the electrostatic alternations in the primary were comparatively slow they might attain a very high intensity without giving rise to a spark in the secondary, since the electrostatic distribution on the secondary would vary so as to remain in equilibrium with the external E.M.F. This, however, is impossible, because the variations in direction follow each other too rapidly for the distribution to follow them.

In the present investigations the primary circuit consisted of a straight copper wire 5 millimetres in diameter, carrying at its extremities hollow zinc spheres 30 centimetres in diameter. The centres of the spheres were one metre apart, and at the middle of the wire was an air space $\frac{3}{4}$ centimetre in length. The wire was placed in a horizontal position, and the observations were all made at points near to the horizontal plane through it, which, however, did not, of course, affect their generality, as the same effects would necessarily be produced in any plane through the horizontal wire. The secondary circuit consisted of a circle of 35 centimetres radius, of copper wire 2 millimetres in diameter, the circle being broken by an air space capable of variation by means of a micrometer screw.

The circular form was selected for the secondary circuit because the former investigations had shown that the sparking distance was not the same at all points of the secondary, even when the conductor as a whole remained unchanged in position, and with a circular circuit it was easier to bring the air space to any part than if any other form had been used. To attain this

object the circle was made movable about an axis passing through its centre perpendicular to its plane.

The circuits of the dimensions stated were very nearly in unison, and they were further adjusted by means of little strips of metal soldered to the extremities, and varied in length until the maximum sparking distance was obtained.

We shall follow Dr. Hertz in first considering the subject theoretically, and then examining how far the experimental results are in accordance with the theoretical conclusions. It will be assumed that the E.M.F. at every point is a simple harmonic function of the time, but that it does not undergo reversal in direction, and it will further be assumed that the oscillations are at any given moment everywhere in the same phase. This will certainly be the case in the immediate neighbourhood of the primary, and for the present we shall confine our attention to such points. Let s be the distance of a point measured along the circuit from the air space of the secondary, and F the component E.M.F. at that point along the circular arc $d\,s$. Then F is a function of s, which assumes its original value after passing once round the circle of circumference S. It may, therefore, be expanded in the form

$$F = A + B\cos.\frac{2\pi s}{S} + \ldots\ldots + B'\sin.\frac{2\pi s}{S} + \ldots\ldots$$

The higher terms of the series may be neglected, as the only result of so doing will be that the approximate theory will give an absolute disappearance of sparks where really the disappearance is not quite complete, and indeed the experiments are not delicate enough to enable us to compare their results with theory beyond a first approximation.

The force A acts in the same direction, and is of constant amount at all points of the circle, and therefore it must be independent of the electrostatic E.M.F., as the integral of the latter round the circle is zero. A, then, represents the total E.M.F. of induction, which is measured by the rate of variation of the number of magnetic lines of force which pass through the circle. If the electro-magnetic field containing the circle is assumed to be uniform, A will therefore be proportional to the component of the magnetic induction perpendicular to the plane of the secondary. It will therefore vanish when the direction of the magnetic induction lies in the plane of the secondary. A will consist of an oscillation, the intensity of which is independent of the position of the air space in the circle, and the corresponding sparking distance will be called a.

The term $B'\sin.\dfrac{2\pi s}{S}$ can have no effect in exciting the fundamental vibration of the secondary, since it is symmetrical on opposite sides of the air space.

The term $B\cos.\dfrac{2\pi s}{S}$ will give a force acting in the same direction in the two quadrants opposed to the air space, and will excite the fundamental vibration. In the two quadrants adjacent to the air space it will give a force in the opposite direction, but its effect will be less than that of the former one. For the current is zero at the extremities of the circuit, and therefore the electricity cannot move so freely as near the centre. This corresponds to the

fact that if a string fastened at each end has its central portion and ends acted on respectively by oppositely directed forces, its motion will be that due to the force at the central portion, which will excite the fundamental vibration if its oscillations are in unison with the latter. The intensity of the vibration will be proportional to B. Let E be the total E.M.F. in the uniform field of the secondary, ϕ the angle between its direction and the plane of the latter, and θ the angle which its projection on this plane makes with the radius drawn to the air space. Then we shall have, approximately,

$$F = E \cos. \phi \sin. \left(\frac{2 \pi s}{S} - \theta \right),$$

and, therefore, $B = - E \cos. \phi \sin. \theta.$

B, therefore, is a function simply of the total E.M.F. due both to the electrostatic and electro-dynamic actions. It will vanish when $\phi = 90°$—that is to say, when the total E.M.F. is perpendicular to the plane of the circle, whatever be the position of the air space on the circle. B will also vanish when $\theta = 0$—that is to say, when the projection of the E.M.F. on the plane of the circle coincides with the radius through the air space. If the position of the air space on the circle is varied, the angle θ will vary, and, therefore, also the intensity of the vibration and the sparking distance. The sparking distance corresponding to the second term of the expansion for F can therefore be represented approximately by a formula of the form $\beta \sin. \theta$.

Now the oscillations giving rise to sparks of lengths a and $\beta \sin. \theta$ respectively are in the same phase. The resulting oscillations will therefore be in the same phase, and their amplitudes must be added together. The sparking distance being approximately proportional to the maximum total amplitude may therefore also be obtained by adding the sparking distances due to the two oscillations respectively. The sparking distance will therefore be given as a function of the position of the air space on the secondary circuit by the expression $a + \beta \sin. \theta$. Since the direction of the oscillation in the air space does not come into consideration, we are concerned only with the absolute value of this expression, and not with its sign. The determination of the absolute values of the quantities a and β would involve elaborate theoretical investigations, and is, moreover, unnecessary for the explanation of the experimental results.

Experiments with the Secondary Circuit in a Vertical Plane.—When the circle forming the secondary circuit was placed with its plane vertical, anywhere in the neighbourhood of the primary, the following results were obtained:—

The sparks disappeared for two positions of the air space, separated by 180°, namely, those in which it lay in the horizontal plane through the primary; but in every other position sparks of greater or less length were observed.

From this it followed that the value of a must have been constantly zero, and that θ was zero when the air space was in the horizontal plane through the primary.

The electro-magnetic lines of force must therefore have been perpendicular to this horizontal plane, and therefore consisted of circles with their centres on the primary; while the electrostatic lines of force must have been entirely in

the horizontal plane, and therefore this system of lines of force consisted of curves lying in planes passing through the primary. Both of these results are in agreement with theory.

When the air space was at its greatest distance from the plane the sparking distance attained a maximum value of from 2 to 3 millimetres. The sparks were shown to be due to the fundamental vibration, by slightly varying the secondary, so as to throw it out of unison with the primary, when the sparking distance was diminished, which would not have been the case if the sparks had been due to overtones. Moreover, the sparks disappeared when the secondary was cut at its points of intersection with the horizontal plane through the primary, though these would be nodal points for the first overtone.

When the air space was kept at its greatest possible distance from the horizontal plane through the primary, and turned about a vertical axis, the sparking distance attained two maxima at the points for which $\phi = 0$, and almost disappeared at the points for which $\phi = 90°$.

The lower half of Fig. 8 shows the different positions of minimum sparking. A A' is the primary conductor, and the lines $m\,n$ represent the projections of the secondary circuit on the horizontal plane. The arrows perpendicular to these give the direction of the resultant lines of force. As this did not anywhere vanish in passing from the sphere A to the sphere A', it could not change its sign.

The diagram brings out the two following points:—

1. The distribution of the resultant E.M.F. in the vicinity of the rectilinear vibration is very similar to that of the electrostatic E.M.F. due to the action of its two extremities. It should be specially noted that near the centre of the primary the direction is that of the electrostatic E.M.F., showing that it is more powerful than the electro-dynamic, as required by theory.

2. The lines of force deviate more rapidly from the line A A' than the
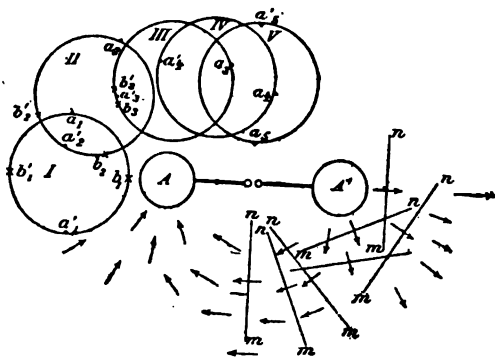
Fig. 8.

electrostatic lines, though this is not so evident on the reduced scale of the diagram as in the author's original drawings on a much larger scale.

It is due to the components of the electrostatic E.M.F. parallel to A A'

being weakened by the E.M.F. of induction, while the perpendicular components remain unaffected.

Experiments with the Secondary Circuit in a Horizontal Plane.—The results obtained when the plane of the secondary was horizontal can best be explained by reference to the upper half of the diagram in Fig. 8.

In the position I., with the centre of the circle in the line AA' produced, the sparks disappeared when the air space occupied either of the positions b_1 or b'_1, while two equal maxima of the sparking distance were obtained at a_1 and a'_1, the length of the spark in these positions being 2·5 millimetres. Both these results are in accordance with theory.

In the position II. the circle is cut by the electro-magnetic lines of force, and, therefore, a does not vanish. It will, however, be small, and we should expect that the expression $a + \beta \sin \theta$ would have two unequal maxima $\beta + a$ and $\beta - a$, both for $\theta = 90°$, and having the line joining them perpendicular to the resultant E.M.F., and between these two maxima we should expect two points of no sparking near to the smaller maximum. This was confirmed by the observations.

The maximum sparking distances were 3·5 millimetres at a_2, and 2 millimetres at a'_2. Now, with the air space at a_2, the sphere A being positive, the resultant E.M.F. in the opposite portion of the circle will repel positive electricity from A, and therefore tend to make it flow round the circle clockwise. Between the two spheres the electrostatic E.M.F. acts from A towards A', and the opposite E.M.F. of induction in the neighbourhood of the primary acts from A' to A, parallel to the former, and, acting more strongly on the nearer than on the further portion of the secondary, tends to cause a current in the same direction as that due to the former, namely, in a clockwise direction. Thus the resultant E.M.F. is the sum of the two as required by theory, and in the same way it is easily seen that when the air space is at a'_2 the resultant E.M.F. is equal to their difference.

As the position III. is gradually approached the maximum disappears, and the single maximum sparking distance a_3 was found to be 4 millimetres in length, having opposite to it a point of disappearance a'_3. In this case clearly $a = \beta$, and the sparking distance is given by the expression $a (1 + \sin \theta)$. The line $a_3 a'_3$ is again perpendicular to the resultant E.M.F.

As the circle approaches further towards the centre of AA' a will become greater than β, and the expression $a + \beta \sin \theta$ will not vanish for any value of θ, but will have a maximum $a + \beta$ and a minimum $a - \beta$; and in the experiments it was found that the sparks never entirely disappeared, but varied between a maximum and a minimum, as indicated by theory.

In the position IV. a maximum sparking distance of 5·5 millimetres was observed at a_4, and a minimum of 1·5 millimetre at a'_4.

In the position V. there was a maximum sparking distance of 6 millimetres at a_5, and a minimum of 2·5 millimetres at a'_5. In these experiments the air space should be screened off from the primary in the latter positions as well as in the earlier ones, in which it is unavoidable, as otherwise the results would not be comparable.

In passing from the position III. to the position V. the line $a\,a'$ rapidly turned from its position of parallelism to the primary circuit into a position perpendicular to it. In the latter positions the sparking was essentially due to the inductive action, and therefore the author was justified, in the experiments described in my previous papers, in assuming the effect in these positions to be due to induction.

Even in these positions, however, the sparking is not totally independent of electrostatic action, except when the air space is half-way between the maximum and minimum positions, and, therefore, $\beta \sin. \theta = 0$.

Other Positions of the Secondary Circuit.—Dr. Hertz made numerous observations with the secondary circuit in other positions, but in no case were any phenomena observed which were not completely in accordance with theory. As an example of these consider the following experiment :—

The secondary was first placed in the horizontal plane in the position V. (Fig. 8), and the air space was in the position a_5 relatively to the primary. The circle was then turned about a horizontal axis through its centre and parallel to the primary, so as to raise the air space above the horizontal plane. During this rotation θ remained equal to 90°, and the value of β remained nearly constant, but a varied approximately in the same ratio as cos. Ψ, Ψ being the angle between the plane of the circle and the horizontal, for a is proportional to the number of magnetic lines of force passing through the circle. Let a_0 be the value of a in the initial position, then in the other positions its value would be a_0 cos. Ψ, and therefore the sparking distance should be given by the expression a_0 cos. $\Psi + \beta$, in which a_0 was known to be greater than β. This was confirmed by observation, for it was found that as the air space increased its height above the horizontal plane the sparking distance diminished from 6 millimetres down to 2 millimetres, its value when the air space was at its greatest distance above the horizontal plane. During the rotation through the next quadrant the sparking distance diminished almost to zero, and then increased to the smaller maximum of 2·5 millimetres, which it attained when the circle had turned through 180° and was therefore again horizontal. Similar results were obtained in the opposite order, as the circle was rotated from 180° to 360°. When the circle was kept with the air space at its maximum height above the horizontal plane, and then raised or lowered bodily without rotation, the sparking distance was found to diminish in the former case and to increase in the latter—results completely in accordance with theory.

Forces at Greater Distances.—Experiments with the secondary at greater distances from the primary are of great importance, as the distribution of E.M.F. in the field of an open circuit is very different according to different theories of electro-dynamic action, and the results may, therefore, serve to eliminate some of them as untenable. In making these experiments, however, an unexpected difficulty was encountered, as it was found that at distances of from 1 to 1·5 metres from the primary, the maximum and minimum, except in certain positions, became indistinctly defined; but when the distance was increased to upwards of 2 metres, though the sparks were then very small,

the maximum and minimum were found to be very sharply marked when the sparks were observed in the dark. The positions of maximum and minimum were found to occur with the circle in planes at right angles to each other. At considerable distances the sparking diminished very slowly as the distance was increased. Dr. Hertz was not able to determine an upper limit to the distance at which sensible effects took place, but in a room 14 metres by 12, sparks were distinctly observed when the primary was placed in one corner of the room, wherever the secondary was placed. When, however, the primary was slightly displaced, no effects could be observed, even when the secondary was brought considerably nearer. The interposition of solid screens between the two circuits greatly diminished the effect.

Dr. Hertz mapped out the distribution of force throughout the room by means of chalk lines on the floor, putting stars at the points where the direction of the E.M.F. became indeterminate. A portion of the diagram obtained in this manner is shown on a reduced scale in Fig. 9, with respect to which the following points are noteworthy :—

Fig. 9.

1. At distances beyond three metres the E.M.F. is everywhere parallel to the primary oscillation. Within this region, therefore, the electrostatic E.M.F. is negligible in comparison with the E.M.F. of induction. Now all the theories of the mutual action of current elements agree in giving an E.M. of induction inversely proportional to the distance ; while the electrostatic E.M.F., being due to the differential action of the two extremities of the primary, is approximately inversely proportional to the cube of the distance. Some of these theories, however, are not in accordance with the experimental result that the effect diminishes much more rapidly in the direction of the primary oscillation than in a direction at right angles to it, induced sparks being observed at a distance exceeding 12 metres in the latter direction, while they disappeared at a distance of about four metres in the former direction.
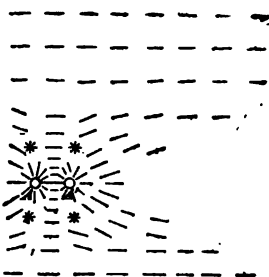
2. That, as already proved, for distances less than one metre the distribution of E.M.F. is practically that of the electrostatic E.M.F.

3. There are two straight lines, at all points of which the direction of the E.M.F. is determinate, namely, the line in which the primary oscillation takes place, and the perpendicular to the primary through its middle point. Along the latter the E.M.F. does not vanish at any point; the sparking diminishes gradually as the distance is increased. This, again, is inconsistent with some of the theories of mutual action of current elements, according to which it should vanish at a certain definite distance. A very important result of the investigation is the demonstration of the existence of regions within which the direction of the E.M.F. becomes indeterminate. These regions form two rings encircling the primary circuit. Since the E.M.F. within them acts very nearly equally in every direction, it must assume different directions in succession, for, of course, it cannot act in different directions simultaneously.

The observations, therefore, lead to the conclusion that within these regions the magnitude of the E.M.F. remains very nearly constant, while its direction varies through all the points of the compass at each oscillation. Dr. Hertz states that he has been unable to explain this result, as also the existence of overtones, by means of the simplified theory in which the higher terms of the expansion of F are neglected, and he considers that no theory of simple action at a distance is capable of explaining it. If, however, the electrostatic E.M.F. and the E.M.F of induction are propagated through space with unequal velocities, it admits of very simple explanation. For within these annular regions the two E.M.F.s are at right angles and of the same order of magnitude; they will therefore, in consequence of the distance traversed, differ in phase, and the direction of the resultant will turn through all the points of the compass at each oscillation.

This phenomenon appears to him to be the first indication which has been observed of a finite rate of propagation through space, of electrical actions, for if there is a difference in the rate of propagation of the electrostatic and electro-dynamic E.M.F., one at least of them must be finite.

At the end of the paper in which the preceding experiments are described, Dr. Hertz describes some observations which he has made on the conditions at the primary sparking point which affect the production of sparks in the secondary circuit. He finds that illuminating the primary spark diminishes its power of exciting rapid oscillations, the sparks in the secondary being observed to cease when a piece of magnesium wire was burnt, or an arc lamp lighted, near the primary sparking point. The observed effect on the primary sparks is that they are no longer accompanied by a sharp crackling sound as before. The effect of a second discharge is especially noteworthy, and it was found that the secondary sparks could be made to disappear by bringing an insulated conductor close to the opposed surfaces of the spheres forming the terminals at the primary air space, even when no visible sparking took place between the latter and the insulated conductor. The secondary sparking could also be stopped by placing a fine point close to the primary air space, or by touching one of the opposed surfaces of the terminals with a piece of sealing-wax, glass, or mica. Dr. Hertz states that further experiments have led him to conclude that, even in these cases, the effect is due to light too feeble to be perceived by the eye, arising from a side discharge. He points out that these effects afford another example of the effects of light on electric discharges, which have been observed by E. Wiedemann, H. Ebert, and W. Hallwachs.

Dr. Hertz's next paper in order of publication in Wiedemann's *Annalen*, " On some Induction Phenomena arising from Electrical Actions in Dielec-" trics" (Wiedemann's *Annalen*, vol. xxxiv., p. 273), contains an account of some researches undertaken with a view of obtaining direct experimental confirmation of the assumption involved in the most suggestive theory of electrical actions, viz., that of Faraday and Maxwell, that the well-known electrostatic phenomena observed in dielectrics are accompanied by corresponding electro-dynamic actions. The method of observation consisted in placing a secondary conductor adjusted to unison, as regards electrical oscilla-

tions, with the primary, as near as possible to the former, and in such a relative position that the sparks in the primary produced no sparking in the secondary. As the equilibrium could be disturbed and sparking induced in the secondary by the approach of conductors, it formed a kind of induction balance; but the point of special interest in connection with it was that a similar effect was produced when the conductors were replaced by insulators, provided the latter were of comparatively large size. The observed rapidity of the oscillations induced in the dielectrics showed that the quantities of
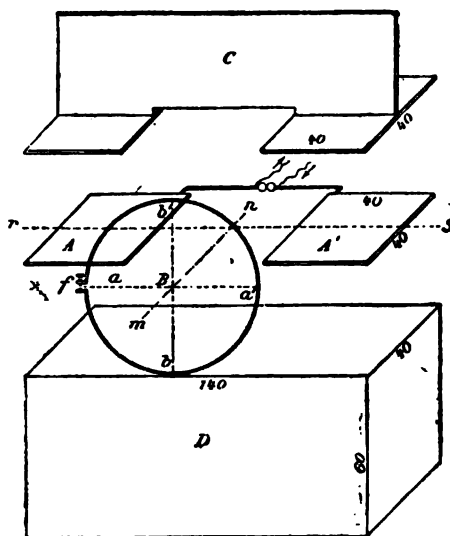
FIG. 10.

electricity in motion under the influence of dielectric polarisation were of the same order of magnitude as in the case of metallic conductors.

The apparatus employed is shown diagrammatically in Fig. 10, and was supported on a light wooden framework, not shown in the illustration. The primary conductor consisted of two brass plates, A A', with sides 40 centimetres in length, joined by a copper wire 70 centimetres long and half a centimetre in diameter, containing an air space of three-quarters of a centimetre, with terminals formed of polished brass spheres. When placed in connection with a powerful induction coil, oscillations are set up, the period of which, determined by the dimensions of the primary, can be determined to within a hundred-millionth of a second. The secondary conductor consisted of a circle, 35 centimetres in radius, of copper wire two millimetres in diameter, containing an air space, the length of which could be varied by means of a screw from a few hundredths of a millimetre up to several millimetres. The dimensions stated were such as to bring the two conductors into unison, and secondary sparks up to six or seven millimetres in length could be obtained.

The circle was movable about an axis through its centre perpendicular to its plane, to enable the position of the air space to be varied. The axis was fixed in the position $m\ n$ in the plane of A and A′, and half-way between them. The centre of the circle was at a distance of 12 centimetres from the nearest points of A and A′.

When f was in either of the positions a or a' lying in the plane of A A′ no sparking occurred in the secondary, while maximum sparking took place at b and b' 90° from the former positions. The E.M.F. giving rise to the secondary sparks is, as in previous experiments, partly electrostatic and partly electro-magnetic, and the former being the greater will determine the sign of the resultant E.M.F. The oscillations must, for the reason previously explained, be considered as produced in the part of the secondary most remote from the air space. Assuming the E.M.F. and the amplitude of the resulting oscillation to be positive when f is in the position b', they will both be negative when f is at b.

When the circle was slightly lowered in its own plane the sparking distance was increased at b' and diminished at b, and the null points lay at a certain distance below a and a'. The electrostatic E.M.F. is scarcely affected by such a displacement, but the integral of the E.M.F. of induction taken round the circle is no longer zero, and therefore gives rise to an oscillation which will be of positive sign whatever be the position of f, for the direction of the resultant E.M.F. of induction is opposite to that of the electrostatic E.M.F. in the upper half of the circle, and coincides with it in the lower half, where the electrostatic E.M.F. has been assumed to be positive. Since the new oscillation so produced is in the same phase as the previously existing one, their amplitudes must be added to give the resultant amplitude, which explains the phenomena.

Effects of the Approach of Conductors.—In making these observations it was found necessary to remove all conductors to a considerable distance from the apparatus, in order to obtain a complete disappearance of sparking at the points a and a'. Even the neighbourhood of the observer was sufficient to set up sparking when the air space f was in either of these positions, and the sparks had therefore to be observed from a distance. The conductors used for the experiments were of the form shown at C (Fig. 10), and consisted of thin metal foil. The objects kept in view in selecting the material and dimensions, were to obtain a conductor which would give a moderately large effect, and having an oscillation period less than that of the primary.

When the conductor C was brought near to A A′, it was found that the sparking distance decreased at b and increased at b', and the null points were displaced upwards—that is, in the direction of C.

From the results of experiments already described it is evident that the effect of displacing A A′ upwards would be the same, qualitatively, as that of a current in the same direction as that in A A′ directly above it. The effect produced by the approach of C was the reverse of this, and could be explained by an inductive action, supposing there were a current in C in the opposite direction to that in A A′, which is exactly what must occur; for the electrostatic

E.M.F. would give rise to such a current, and since the oscillations in C are more rapid than those of this E.M.F., the current must be in the same phase as the inducing E.M.F. The truth of this explanation was confirmed by the following experiments. The horizontal plates of the conductor C being left in the same position as before, the vertical plate was removed, and successively replaced by wires of increasing length and fineness, in order to lengthen the oscillation period of C. The effect of this was to displace the null points more and more in an upward direction, while at the same time they became less sharply defined, a minimum sparking taking the place of the previous absolute disappearance. The sparking distance at the highest point had previously been much less than at the lowest point, but after the disappearance of the null points it began to increase. At a certain stage the sparking distance at the two positions became equal, and then no definite minimum points could be found, but sparking took place freely at all positions of f. Beyond this stage the sparking distance at the lowest point diminished, and very soon two minimum points made their appearance close to it, not clearly defined at first, but gradually becoming more distinct, and at the same time approaching the points a a', with which they ultimately coincided, when the minimum points again became absolute null points. These results are in agreement with the conclusion drawn from the former observations, for as the oscillation period of C approaches that of A A' the intensity of the current in the former increases, but a difference of phase arises between it and the exciting E.M.F. When the two are in unison the current in C attains its maximum, and, as in other cases of resonance, the difference of phase gives rise to a slightly damped oscillation, having a period of about a quarter that of the original one, which makes any interference between the oscillations excited in the circle B by A A' and C respectively impossible. These conditions clearly correspond to the stage at which the sparking distances at b and b' were equal. When the oscillation period of C becomes decidedly greater than that of A A', the amplitude of the oscillation in the former will again diminish, so that the difference in phase between it and the exciting E.M.F. will approach half of the original period. The current in C will therefore always be in the same direction as that in A A', so that interference between the two oscillations excited in B will again become possible, and the effect of C will then be opposite to its original effect. When the conductor C was made to approach A A' the sparks in B became much smaller, which is explained by the fact that its effect will be to increase the oscillation period of A A', and therefore to throw it out of unison with B.

Effects of the Approach of Dielectrics.—A very rough estimate shows that when a dielectric of large mass is brought near to the apparatus, the quantities of electricity set in motion by dielectric polarisation are at least as large as in metallic wires or thin rods. If, therefore, the action of the apparatus were unaffected by the approach of such masses, it would show that, in contradiction to the theories of Faraday and Maxwell, no electro-dynamic actions are called into play by means of dielectric polarisation, or, as Maxwell calls it, electric displacement. The experiments, however, showed an effect similar to

that which would be produced if the dielectric were replaced by a conductor with a very small oscillation period. In the first experiment made, the mass of dielectric consisted of a pile of books, 1·5 metre long, 0·5 metre broad, and 1 metre high, placed under the plates A A'. Its effect was to displace the null points through about 10° towards the pile. A block of asphalte (D, in Fig. 10), weighing 800 kilogrammes, and measuring 1·4 metre in length, 0·4 metre in breadth, and 0·6 metre in height, was then used in place of the books, the plates being allowed to rest upon it.

The following results were then obtained :—

1. The spark at the highest point of the circle was now decidedly stronger than that at the lowest point, which was nearer to the asphalte.

2. The null points were displaced through about 23° downwards—that is, in the direction of the block—and at the same time were transformed into mere points of minimum sparking, a complete disappearance being no longer obtainable.

3. When the plates A A' rested on the asphalte block the oscillation period of the primary was increased, as shown by the fact that the period of B had to be slightly increased in order to obtain the maximum sparking distance.

4. When the apparatus was moved gradually away from the block its action steadily diminished without changing its character.

5. The action of the block could be compensated by bringing the conductor C over the plates A A', while they rested on the block, the null points being brought back to a and a' when C was at a height of 11 centimetres above the plates. When the upper surface of the asphalte was 5 centimetres below the plates, compensation was obtained when C was placed at a height of 17 centimetres above them, showing that the action of the dielectric was of the order of magnitude which had been anticipated.

The asphalte.contained about 5 per cent. of aluminium and iron compounds, 40 per cent. of calcium compounds, and 17 per cent. of quartz sand. In order to make sure that the observed effects were not due to the conductivity of some of these substances, a number of further experiments were made.

In the first place, the asphalte was replaced by a mass of the same dimensions of the so-called artificial pitch prepared from coal, and effects of a similar kind were observed, but slightly weaker, the greatest displacement of the null points amounting to 19°. Unfortunately this pitch contains free carbon, the amount of which it is difficult to determine, and this would have some conductivity.

The experiments were then repeated with a conductor, C, of half the linear dimensions of the former one, and smaller blocks of various substances, on account of the great cost of obtaining large blocks of pure materials. The substances used were asphalte, coal-pitch, paper, wood, sandstone, sulphur, paraffin, and also a fluid dielectric, namely, petroleum. With the smaller apparatus it was not possible to obtain quantitative results of the same accuracy as before, but the effects were of an exactly similar character, and left little room for doubt of the reality of the action of the dielectric.

The results might possibly be supposed to be due to a change in the distribution of the electrostatic E.M.F. in the neighbourhood of the dielectric, but in the first place Dr. Hertz states that he has been unable to explain the details of the observations on this hypothesis, and in the second place it is disproved by the following experiment:—

The smaller apparatus was placed with the line *r s* on the upper near corner of one of the large blocks, in which position the dielectric was bounded by the plane of the plates A A' and the perpendicular plane through *r s*, both of which are equipotential surfaces, so that if the action were electrostatic no effect should be produced by the dielectric. It was found, however, to produce the same effect as in other positions. It might also be supposed that the effects were due to a slight conductivity, but this could hardly be the case with such good insulators as sulphur and paraffin. Suppose, moreover, that the conductivity of the dielectric is sufficient to discharge the plate A in the ten-thousandth of a second, but not much more rapidly. Then, during one oscillation, the plates would lose only the ten-thousandth part of their charge, and the conduction current in the substance experimented on would not exceed the ten-thousandth part of the primary current in A A', so that the effect would be quite insensible.

It was shown in the experiments described in my last Paper that when variable electrical forces act in the interior of dielectrics of specific inductive capacity not equal to unity, the corresponding electric displacements produce electro-dynamic effects. In a paper, "On the Velocity of Propagation of "Electro-dynamic Actions," in Wiedemann's *Annalen*, vol. xxxiv., p. 551, Dr. Hertz shows that similar actions take place in the air, which proves, as was previously pointed out, that electro-dynamic action must be propagated with a finite velocity.

The method of investigation was to excite electrical oscillations in a rectilinear conductor in the same manner as in former experiments, and then to produce effects in a secondary conductor by exciting electrical oscillations in it by means of those in the rectilinear conductor, and at the same time by the primary conductor acting through the intervening space. This distance was gradually increased, when it was found that the phase of the vibrations at a distance from the primary lagged behind those in its immediate neighbourhood, showing that the action is propagated with a finite velocity, which was found to be greater than the velocity of propagation of electrical waves in wires in the ratio of about 45 to 28, so that the former is of the same order as the velocity of light. Dr. Hertz was unable to obtain any evidence with respect to the velocity of propagation of electrostatic actions.

The primary conductor A A' (Fig. 11) consisted of a pair of square brass plates with sides 40 centimetres in length, connected by a copper wire 60 centimetres in length, at the middle point of which was an air space, across which sparks were made to pass by means of powerful discharges from the induction coil J. The conductor was fixed at a height of 1·5 metre above the base-plate of the coil, with its plates vertical, and the connecting wire

horizontal. A straight line, r s, drawn horizontally through the air space
of the primary, and perpendicular to the direction of the primary oscillation,
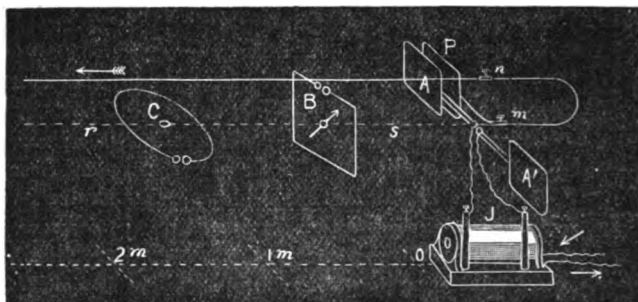
FIG. 11.

will be called " the base-line ; " and a point in this, situated at a distance of 45
centimetres from the air space, will be referred to as " the null point."

The experiments were made in a large lecture room, with nothing near
the base-line for a distance of 12 metres from the primary conductor. The
room was darkened during the experiments.

The secondary conductor consisted either of a circular wire, C, of 35
centimetres radius, or of a square of wire, B, with sides 60 centimetres long.
The primary and secondary air spaces were both capable of adjustment by
means of micrometer screws. Both the secondary conductors were in unison
with the primary, the (half) vibration period of each being $1·4$ hundred-
millionth of a second, as calculated from the capacity and coefficient of
self-induction. It is doubtful whether the ordinary theory of electrical
oscillations would lead to accurate results under the conditions of these
experiments, but as it gives correct numerical results in the case of Leyden
jar discharges, it may be expected to be correct as far as the order of the
results is concerned. When the centre of the secondary lies in the base-line,
and its plane coincides with the vertical plane through the base-line, no
sparks are observed in the secondary, the E.M.F. being everywhere perpen-
dicular to the direction of the secondary. This will be referred to as "the
" first principal position " of the secondary. When the plane of the secondary
is vertical and perpendicular to the base-line, the centre still lying in the
base line, the secondary will be said to be in its " second principal position."
Sparking then occurs in the secondary when its air space is either above or
below the horizontal plane through the base-line, but not when it is in this
plane. As the distance from the primary was increased, the sparking
distance was observed to decrease, rapidly at first, but ultimately very slowly.
Sparks were observed throughout the whole distance of 12 metres available
for the experiments. The sparking in this position is due essentially to
the E.M.F. produced in the portion of the secondary remote from the air
space. The total E.M.F. is partly electrostatic and partly electro-dynamic,
and the experiments show beyond the possibility of doubt that the former

is greater, and therefore determines the direction of the total E.M.F. close to the primary, while at greater distances it is the electro-dynamic E.M.F. which is the greater.

The plane of the secondary was then turned into the horizontal, its centre still lying in the base-line. This may be called "the third principal position." When the centre of the circular secondary conductor was kept fixed at the null point, and the air space was made to travel round the circle, vigorous sparking was observed in all positions. The sparking distance attained its maximum length of about six millimetres when its air space was nearest to that of the primary, and its minimum length of about three millimetres when the distance between the two air spaces was greatest. If the secondary had been influenced by the electrostatic force, sparking would only be expected when the air space was close to the base-line, and a cessation of sparks in the intermediate positions. The direction of the oscillation would, moreover, be determined by the direction of the E.M.F. in the portion of the secondary furthest from the air space. There is, however, superposed upon the electrostatically excited oscillation, a second oscillation, due to the E.M.F. of induction, which produces a considerable effect, since its integral round the circle (considered as a closed circuit) does not vanish; and the direction of this integral E.M.F. is independent of the position of the air space, opposing the electrostatic E.M.F. in the portion of the secondary next to A A', and assisting it in the portion furthest from A A', as explained in a previous Paper.

The electrostatic and electro-dynamic E.M.F.s therefore act in the same direction when the air space is turned towards the primary conductors, and in opposite directions when the air space is turned away from the primary. In the latter position it is the E.M.F. of induction which is the more powerful, as is shown by the fact that there is no disappearance of sparking in any position of the air space, for when this is 90 degrees to the right or left of the base-line it coincides with a node with respect to the electrostatic E.M.F. In these positions the inductive action in the neighbourhood of the primary can be observed, independently of the electrostatic action.

Waves in Rectilinear Wires.—In order to produce in a wire by means of the primary oscillations a series of advancing waves of the character required for these experiments, the following arrangements were made :—Behind the plate A was placed a plate, P, of equal size. A copper wire one millimetre in diameter connected P to the point M of the base-line. From M the wire was continued in a curve about a metre in length to the point N, situated about 30 centimetres above the air space, and was then further continued in a straight line parallel to the base-line for such a distance as to obviate all danger of disturbance from reflected waves. In the present series of experiments the wire passed through a window, and after being carried to a distance of about 60 metres, was put to earth, and a special series of experiments showed that this length was sufficient. When a wire, bent so as to form a nearly closed circuit with a small air space, was brought near to this straight wire, a series of fine sparks was seen to accompany the

discharges of the induction coil. Their intensity could be varied by varying the distance between the plates P and A. The waves in the rectilinear wire were of the same period as that of the primary oscillations, as was proved by their being shown to be in unison with each of the two secondary conductors previously described. The existence of stationary waves showed that the waves in the rectilinear wire were of a steady character in space as well as in time. The nodal points were determined in the following manner :—The further end of the wire was left free, and the secondary conductor was brought near to it in such a position that the wire lay in its plane, and had the air space turned towards it. As the secondary was moved along the wire, points of no sparking were observed to recur periodically. The distance from the point n to the first of these was measured, and the length of the wire made equal to a multiple of this distance. The experiments were then repeated, and it was found that the nodal points occurred at approximately equal intervals along the wire.

The nodes could also be distinguished from the loops in other ways. The secondary conductor was brought near to the wire, with its plane perpendicular to it, and with its air space neither directed completely towards the wire nor completely away from it, but in an intermediate position, so as to produce E.M.F.s perpendicular to the wire. Sparks were then observed at the nodes, while they disappeared at the loops. When sparks were taken from the rectilinear wire by means of an insulated conductor, they were found to be stronger at the nodes than at the loops; the difference, however, was small, and was, indeed, scarcely distinguishable unless the position of the nodes and loops was previously known. The reason that this and other similar methods do not give a well-defined result lies in the fact that irregular oscillations are superposed upon the waves considered; the regular waves, however, can be picked out by means of the secondary, just as definite notes are picked out by means of a Helmholtz resonator. If the wire is severed at a node, no effect is produced upon the waves in the portion of wire next to the origin; but if the severed portion of wire is left in its place, the waves continue to be propagated through it, though with somewhat diminished strength.

The possibility of measuring the wave-lengths leads to various applications. If the copper wire hitherto used is replaced by one of different diameter, or by a wire of some other metal, the nodal points retain their position unchanged. It follows from this that the velocity of propagation in a wire has a definite value independent of its dimensions and material. Even iron wires offer no exception to this, showing that the magnetic susceptibility of iron does not play any part in the case of such rapid motions. It would be interesting to investigate the behaviour of electrolytes in this respect. In their case we should expect a smaller velocity of propagation, because the electrical motions are accompanied by motions of the molecules carrying the electric charges. It was found that no propagation of the waves took place through a tube 10 millimetres in diameter, filled with a solution of sulphate of copper; but this may have been due to the

resistance being too high. By the measurement of wave-lengths the relative vibration periods of different primary conductors can be determined, and it therefore becomes possible to compare in this manner the vibration periods of plates, spheres, ellipsoids, &c.

In the experiments made by Dr Hertz, nodes were very distinctly produced when the wire was severed at a distance of either 8 metres or 5·5 metres from the null point of the base-line. In the first case the nodes occurred at distances from the null point of −0·2 metre, 2·3 metres, 5·1 metres, and 8 metres, and in the latter case at distances of −0·1 metre, 2·8 metres, and 5·5 metres. It appears, therefore, that the (half) wave-length in a free wire cannot differ much from 2·8 metres. The fact that the wave-lengths nearest to P were somewhat smaller, was to be expected from the influence of the plates and of the curvature of the wire. This wave-length, with a period of 1·4 hundred-millionth of a second, gives 200,000 kilometres per second for the velocity of propagation of electrical waves in wires. Fizeau and Gounelle (Poggendorff's *Annalen*, vol. lxxx., p. 158, 1850) obtained for the velocity in iron wires 100,000 kilometres per second, and 180,000 in copper wires. W. Siemens (Poggendorff's *Annalen*, vol. clvii., p. 309, 1876), by the aid of Leyden jar discharges, obtained a velocity of from 200,000 to 260,000 kilometres per second in iron wires. Dr. Hertz's result is very nearly the mean of these, from which we may conclude that the order, at any rate, of the vibration period as calculated by him is correct. The value obtained cannot be regarded, independently of its agreement with experimental results otherwise obtained, as a fresh determination of the velocity, since it rests upon a theory which is open to doubt.

Interference of the Direct Actions with those Transmitted through the Wire.—If the square circuit B is placed at the null point in the second principal position, with the air space at its highest point, it will be unaffected by the waves in the wire, but the direct action when in this position was found to produce sparks 2 millimetres in length. B was then turned about a vertical axis into the first principal position, in which there would be no direct action of the primary oscillation, but the waves in the wire gave rise to sparks, and by bringing P near enough to A, a sparking distance of 2 millimetres could be obtained. In the intermediate positions sparks were produced in both these ways, and it would therefore be possible to get a difference of phase, such that one should either increase or diminish the effect of the other. Phenomena of this nature were, indeed, observed. When the plane of B was in such a position that the normal drawn towards A A' was directed away from that side of the primary conductor on which P was placed, there was more sparking than even in the principal position; but if the normal were directed towards P the sparks disappeared, and only reappeared when the air space was made smaller. When the air space was at the lowest point of B, the other conditions remaining the same, the sparks disappeared when the normal was turned away from P. Further variations of the experiment gave results in accordance with these.

It is easily seen that these phenomena were exactly what were to be expected. To fix the ideas, suppose the air space to be at the highest point, and

the normal directed towards P, as in Fig. 11. Consider what happens at the moment that the plate A has its greatest positive charge. The electrostatic, and therefore the total E.M.F., is directed from A towards A'. The oscillation to which this gives rise in B is determined by the direction of the E.M.F. in the lower portion of B. Therefore positive electricity will flow towards A' in the lower portion, and away from A' in the upper portion.

Consider next the action of the waves. As long as A is positively charged, positive electricity will flow from the plate P. This current is, at the moment considered, at its maximum value at the middle point of the first half wave-length. A quarter of a wave-length further from the origin—that is to say, in the neighbourhood of the null point—it first changes its direction. The E.M.F. of induction will here, therefore, impel positive electricity towards the origin. A current will therefore flow round B towards A' in the upper portion and away from A' in the lower portion. The electrostatic and electro-dynamic E.M.F.s are therefore in opposite phases and oppose each other's action. If the secondary circuit is rotated through 90 deg., through the first principal position, the direct action changes its sign, but not so the action of the waves, so that they now tend to strengthen each other. The same reasoning holds when the air space is at the lowest point of B.

Greater lengths of wire were then included between m and n, and it was found that the interference became gradually less marked, until with a length of 2·5 metres it disappeared entirely, the sparks being of equal length whether the normal were directed towards or away from P. When the length of wire between m and n was further increased, the distinction between the different quadrants reappeared, and with a length of 4 metres the disappearance of the sparks was fairly sharp. The disappearance, however, then took place (with the air space at the highest point) when the normal was directed away from P, the opposite direction to that in which the disappearance previously took place. With a still further increase in the length of the wire the interference reappeared, and returned to its original direction with a length of 6 metres. These phenomena are clearly to be explained by the retardation of the waves in the wire, and show that here again the direction of motion in the advancing waves changes its sign at intervals of about 2·8 metres.

To obtain interference phenomena with the secondary circuit C in the third principal position, the rectilinear wire must be removed from its original position and placed in the horizontal plane through C either on the side of the plate A or of the plate A'. Practically it is sufficient to stretch the wire loosely and to fix it by means of an insulated clamp on each side of C alternately. It was found that when the wire was on the same side as the plate P the waves in it diminished the previous sparking, and when on the opposite side the sparking was increased, both results being unaffected by the position of the air space in the secondary circuit. Now it has been already pointed out that at the moment when the plate A has its maximum positive charge, and at which, therefore, the primary current begins to flow from A, the current at the first node of the rectilinear wire begins to flow away from the origin. The two currents therefore flow round C in the same direction when

C lies between the rectilinear wire and A, and in opposite directions when the wire and A are on the same side of C. The fact that the position of the air space is indifferent confirms the conclusion formerly arrived at—that the direction of oscillation is that due to the electro-dynamic E.M.F. These interferences are also changed in direction when the wire *m n*, 1 metre in length, is replaced by a wire 4 metres in length.

Dr. Hertz also succeeded in obtaining interference phenomena when the centre of the secondary circuit was not in the base-line, but these results were of no special importance, except that they confirmed the previous conclusions.

Interference Phenomena at Various Distances.—Interference may be produced with the secondary at greater distances than that of the null point, but care must then be taken that the action of the waves in the wire is of about the same magnitude as the direct action of the primary circuit through the air. This can be effected by increasing the distance between P and A.

Now, if the velocity of propagation of the electro-dynamic disturbances through the air is infinite, the interference will change its sign at every half-wave length in the wire—that is to say, at intervals of about 2·8 metres. If the velocities of propagation through the air and through the wire are equal, the interference will be in the same direction at all distances. Finally, if the velocity of propagation through the air is finite, but different from the velocity in the wire, the interference will change in sign at intervals greater than 2·8 metres.

The interferences first investigated were those which occurred when the secondary circuit was rotated from the first into the second principal position, the air space being at the highest point. The distance of the secondary from the null point was increased by half-metre stages from 0 up to 8 metres, and at each of these positions an observation was made of the effects of directing the normal towards and away from P respectively. The points at which no difference in the sparking was observed in the two positions of the normal are marked 0 in the table below. Those in which the sparking was least,

Table I.

	0		1		2		3		4		5		6		7		8
100	+	+	0	−	−	−	−	−	0	0	0	0	0	+	+	+	+
150	+	0	−	−	−	−	0	0	0	0	0	+	+	+	+	+	0
200	0	−	−	−	−	−	0	+	+	+	+	+	0	0	0	0	0
250	0	−	−	−	−	0	0	+	+	+	+	0	0	0	0	0	0
300	−	−	−	−	0	+	+	+	+	+	0	0	0	0	−	−	−
350	−	−	0	+	+	+	+	+	+	0	0	0	0	−	−	−	−
400	−	−	0	+	+	+	+	0	0	0	0	−	−	−	−	−	−
450	−	0	+	+	+	+	+	0	0	0	−	−	−	−	−	−	0
500	−	0	+	+	+	+	0	−	−	−	−	0	0	0	0	0	+
550	0	+	+	+	+	0	0	−	−	−	−	0	0	0	0	0	+
600	+	+	+	+	0	0	−	−	−	−	−	0	0	+	+	+	+

showing the existence of interference, when the normal was directed towards P are marked +, and those in which the sparking was least, when the normal

was directed away from P are marked —.   The experiments were repeated
with different lengths of wire *m n*, varying by steps of half a metre from 1
metre up to 6 metres.   The first horizontal line in the table gives the distance,
in metres of the centre of the secondary circuit from the null point, while the
first vertical line gives the lengths of the wire *m n*, also in metres.

An inspection of this table shows, in the first place, that the changes of
sign take place at longer intervals than 2·8 metres; and, in the second place,
that the change of phase is more rapid in the neighbourhood of the origin
than at a distance from it.   As a variation in the velocity of propagation is
very unlikely, this is probably due to the fact indicated by theory that the
electrostatic E.M.F., which is more powerful than the electro-dynamic E.M.F.
in the neighbourhood of the primary oscillation, has a greater velocity of
propagation than the latter.

In order to obtain a definite proof of the existence of similar phenomena
at greater distances Dr. Hertz continued the observations, in the case of three
of the lengths *m n*, up to a distance of 12 metres, and the result is given in
the table below:—

Table II.

	0	1	2	3	4	5	6	7	8	9	10	11	12
100	+	0	—	—	0	0	0	+	+	+	+	+	0
250	0	—	—	0	+	+	0	0	0	0	—	—	—
400	—	0	+	+	0	0	—	—	—	—	0	0	0

If we make the assumption that at the greater distance it is only the
E.M.F. of induction which produces any effect, the experiments would show
that the interference of the waves excited by the E.M.F. of induction with
the original waves in the wire, changes its sign only at intervals of about
7 metres.

In order to investigate the E.M.F. of induction close to the primary oscilla-
tion, where the results are of special importance, Dr. Hertz made use of the
interferences which were obtained when the secondary circuit was in the third
principal position, and the air space was rotated through 90 degs. from the
base-line.   The direction of the interference at the null point, which has
already been considered, was taken as negative, the interference being con-
sidered positive when it was produced by the passage of waves on the side of
C remote from P, which makes the signs correspond with those of the previous
experiments.   It must be borne in mind that the direction of the resultant
E.M.F. at the null point is opposed to that of the E.M.F. of induction, and there-
fore the first table would have begun with a negative sign if the electrostatic
E.M.F. could have been eliminated.   The present experiments showed that up
to a distance of 3 metres, interference continued to occur, and always of the same
sign as at the null point.   It was unfortunately impossible to extend these
observations to a greater distance than 4 metres, on account of the feebleness

of the sparks, but the results obtained were sufficient to give distinct evidence of a finite velocity of propagation of the E.M.F. of induction. These observations; like the former ones, were repeated with various lengths of the wire *m n* in order to exhibit the variation in phase, and the results obtained are given in the table below :—

Table III.

	0	1	2	3	4
100	—	—	—	—	0
150	—	—	0	(0	0
200	0	0	0	+	+
250	0	+	+	+	+
300	+	+	+	+	+
350	+	+	+	+	0
400	+	+	+	+	0
450	+	+	+	0	0
500	+	+	0	0	0
550	+	0	0	0	—
600	0	—	—	—	—

which shows that as the distance increases, the phase of the interference changes in such a manner that a reversal of sign takes place at intervals of from 7 to 8 metres. This result is further confirmed by comparing the results of Table III. with the results for greater distances given in Table II., for in the former series the effect of the electrostatic E.M.F. is eliminated owing to the special position of the secondary circuit, while in the former it becomes insensible at the greater distances owing to its rapid decrease with increasing distance. We should therefore expect the results given in the first table for distances beyond 4 metres to follow without a break the results given in Table III. for distances up to 4 metres. This was found to be the case, as is evident from inspection of Tables II. and III.

To show this more clearly the signs of the interference of the waves, due to the electro-dynamic E.M.F., with the waves in the wire, are collected together in Table IV., the first four columns of which are taken from Table III., and the remaining columns from Table II.

Table IV.

	0	1	2	3	4	5	6	7	8	9	10	11	12
100	—	—	—	—	0	0	0	+	+	+	+	+	0
250	0	+	+	+	+	+	0	0	0	0	—	—	—
400	+	+	+	+	0	0	—	—	—	—	0	0	0

From the results given in this table the author draws the following conclusions:—

1. The interference does not change its sign at intervals of 2·8 metres. The electro-dynamic actions are therefore not propagated with an infinite velocity.

2. The interference is not in the same phase at all points. Therefore the electro-dynamic actions are not propagated through air with the same velocity as electric waves in wires.

3. A gradual retardation of the waves in the wire has the effect of displacing a given phase of the interference towards the origin of the waves. The velocity of propagation through the air is therefore greater than through a wire.

4. The sign of the interference is reversed at intervals of 7·5 metres, and therefore in traversing this distance an electro-dynamic wave gains one length of the waves in the wire.

Thus, while the former travels 7·5 metres, the latter travels 7·5 − 2·8 = 4·7 metres, and therefore the ratio of the velocities is 75 : 47, which gives for the half wave-length of the electro-dynamic action 2·8 × 75/47 = 4·5 metres. Since this distance is traversed in 1·4 hundred-millionth of a second, the absolute velocity of propagation through the air must be 320,000 kilometres per second. This result can only be considered reliable as far as its order is concerned; but its true value can hardly exceed half as much again, or be less than two-thirds of this amount. In order to obtain a more accurate determination of the true value it will be necessary to determine the velocity of electric waves in wires with greater exactness.

It does not necessarily follow from the fact that in the immediate neighbourhood of the primary oscillation the interference changes its sign after an interval of 2·8 metres, that the velocity of propagation of the electrostatic action is infinite, for such a conclusion would rest upon a single change of sign, which might, moreover, be explained, independently of any change of phase, by a change in the sign of the amplitude of the resultant force at a certain distance from the primary oscillation. Quite independently, however, of any knowledge of the velocity of propagation of electrostatic actions, there exist definite proofs that the rates of propagation of electrostatic and electro-dynamic E.M.F.s are unequal.

In the first place, the total force does not vanish at any point on the base-line. Now near the primary the electrostatic E.M.F. is the greater, while the electro-dynamic E.M.F. is the greater at greater distances. There must, therefore, be some point at which they are equal, and since they do not balance they must take different times to reach this point.

In the second place, the existence of points at which the direction of the resultant E.M.F. becomes indeterminate does not seem capable of explanation, except on the supposition that the electrostatic and electro-dynamic components perpendicular to each other are in appreciably different phases, and, therefore, do not compound into a rectilinear oscillation in a fixed direction. The fact that the two components of the resultant are propagated with

different velocities is of considerable importance, in that it gives an independent proof that one of them at any rate must have a finite velocity of propagation.

The latest researches of Dr. Hertz on electrical oscillations of which accounts have been published at present are described in a paper, "On Electro-"dynamic Waves in Air, and their Reflection," in Wiedemann's *Annalen*, vol. xxxiv., p. 609. The author had been endeavouring to find a more striking and direct proof of the finite velocity of propagation of electro-dynamic waves than those which he had hitherto given, for, though these are quite sufficient to establish the fact, they can only be properly appreciated by one who has obtained a grasp of the results of the entire series of researches.

In many of the experiments which have been described, Dr. Hertz had noticed the appearance of sparks at points in the secondary conductor where it was clear from geometrical considerations that they could not be due to direct action, and it was observed that this occurred chiefly in the neighbourhood of solid obstacles. It was found, moreover, that in most positions of the secondary conductor the feeble sparks produced at a great distance from the primary became considerably stronger in the vicinity of a solid wall, but disappeared with considerable suddenness quite close to the wall. The most obvious explanation of these experiments was that the waves of inductive action were reflected from the wall and interfered with the direct waves, especially as it was found that the phenomena became more distinct when the circumstances were such as to favour reflection to the greatest possible extent. Dr. Hertz therefore determined upon a thorough investigation of the phenomena.

The experiments were made in the Physical Lecture Theatre, which is 15 metres in length, 14 metres in width, and 6 metres in height. Two rows of iron columns, running parallel to the sides of the room, would collectively act almost like a solid wall towards electro-dynamic action, so that the available width of the room was only 8·5 metres. All pendant gas fittings were removed, and the room left empty, with the exception of wooden tables and forms, which would not exert any appreciable disturbing effect. The end wall, from which the waves were to be reflected, was of solid sandstone, with two doors in it, and the numerous gas pipes attached to it gave it, to a certain extent, the character of a conducting surface, and this was increased by fastening to it a sheet of zinc 4 metres high and 2 metres broad, connected by wires to the gas pipes and a neighbouring water pipe. Special care was taken to provide an escape for the electricity at the upper and lower extremities of the zinc plate, where a certain accumulation of electricity was to be expected.

The primary conductor was the same that was employed in the experiments described in my last paper, and was placed at a distance of 13 metres from the zinc plate, and, therefore, two metres from the wall at the other end of the room. The conducting wire was placed vertically, so that the E.M.F.s to be considered, increased and diminished in a vertical direction. The centre of the primary conductor was 2·5 metres above the floor of the room, which left a clear space for the observations above the tables and benches. The point

of intersection of the reflecting surface with the perpendicular from the centre of the primary conductor will be called "the point of incidence," and the experiments were limited to the neighbourhood of this point, as the investigation of waves striking the wall at a considerable angle would be complicated by the differences in their polarisation. The plane of vibration was therefore parallel to the reflecting surface, and the plane of the waves was perpendicular to it, and passed through the point of incidence.

The secondary conductor consisted of the circle of 35 centimetres radius, which has been already described. It was movable about an axis through its centre perpendicular to its plane, and the axis itself was movable in a horizontal plane about a vertical axis. In most of the experiments the
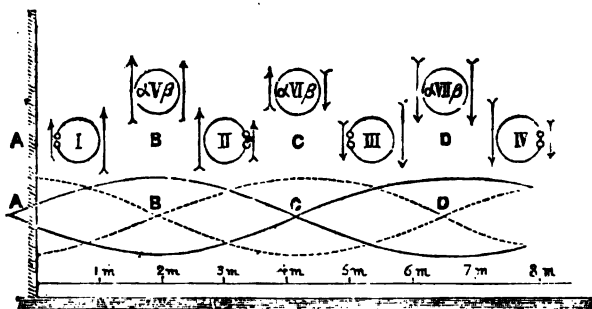
FIG. 12.

secondary conductor was held in the hand by its insulating wooden support, as this was the most convenient way of bringing it into the various positions required. The results of these experiments, however, had to be checked by observations made with the observer at a greater distance from the secondary, as the neighbourhood of his body exerted a slight influence upon the phenomena. The sparks were distinct enough to be observed at a distance of several metres when the room was darkened, but when the room remained light they were practically invisible even when the observer was quite close to the secondary.

When the centre of the secondary was placed in the line of incidence and with its plane in the plane of vibration, and the air space was turned first towards the reflecting wall and then away from it, a considerable difference was generally observed in the strength of the sparks in the two positions. At a distance of about 0·8 metre from the wall the sparks were much stronger when the air space was directed towards the wall, and its length could be adjusted so that while there was a steady stream of sparks when in this position, they disappeared entirely when the air space was directed away from the wall. These phenomena were reversed at a distance of 3 metres, and recurred, as in the first case, at a distance of 5·5 metres. At a distance of 8 metres the sparks were stronger when the air space was turned away from the wall, as at the distance of 3 metres, but the difference was not so well marked. When the distance was increased beyond 8 metres no further reversal took

place, owing to the increase in the direct effect of the primary oscillation and the complicated distribution of the E.M.F. in its neighbourhood.

The positions I., II., III., and IV. (Fig. 12) of the secondary circle are those in which the sparks were strongest, the distance from the wall being shown by the horizontal scale at the foot. When the secondary circle was in the positions V., VI., and VII., the sparks were equally strong in both positions of the air space, and quite close to the wall the difference between the sparking in the two positions again diminished. Therefore the points A, B, C, D in the diagram may in a certain sense be regarded as nodes. The distance between two of these points must not, however, be taken as the half wave-length, for if all the electrical motions changed their directions on passing through one of these points, the phenomena observed in the secondary circuit would be repeated without variation, since the direction of oscillation in the air space is indifferent.

The conclusion to be drawn from the experiments is that in passing any one of these points, part of the action is reversed, while another part is not. The experimental results, however, warrant the assumption that twice the distance between two of these points is equal to the half wave-length, and when this assumption is made the phenomena can be fully explained.

For suppose a wave of E.M.F., with oscillations in a vertical direction, to impinge upon the wall, and to be reflected with only slightly diminished intensity, thus giving rise to stationary waves. If the wall were a perfect conductor, a node would necessarily be formed in its surface, for at the boundary and in the interior of a perfect conductor the E.M.F. must be infinitely small. The wall cannot, however, be considered as a perfect conductor, for it was not metallic throughout, and the portion which was metallic was not of any great extent. The E.M.F. would therefore have a finite value at its surface, and would be in the direction of the impinging waves. The node, which in the case of perfect conductivity would occur at the surface of the wall, would, therefore, actually be situated a little behind it, as shown at A in the diagram. If, then, twice the distance A B—that is to say, the distance A C—is half the wave-length, the steady waves will be as represented by the continuous lines in Fig. 12. The E.M.F.s acting on each side of the circles, in the positions I., II., III., and IV., will therefore at a given moment be represented in magnitude and direction by the arrows on each side of them in the diagram. If, therefore, in the neighbourhood of a node, the air space is turned towards the node, the strongest E.M.F. in the circle will act under more favourable conditions against a weaker one under less favourable conditions. If, however, the air space is turned away from the node, the stronger E.M.F. acts under less favourable conditions against a weaker one under more favourable conditions. In the latter case the resultant action must be less than in the former, whichever of the two E.M.F.s has the greater effect, which explains the change of sign of the phenomenon at each quarter wave-length.

This explanation is further confirmed by the consideration that if it is the true one, the change of sign at the points B and D must take place in quite a different manner from that of the point C. The E.M.F.s acting on the

secondary circle, in the positions V., VI., and VII., are shown by the corresponding arrows, and it is clear that in the positions B and D, if the air space is turned from one side to the other, the vibration will change its direction round the circle, and therefore the sparking must, during the rotation, vanish either once or an uneven number of times. In the position C, however, the direction of vibration remains unaltered, and therefore the sparks must disappear an even number of times, or not at all.

The experiments showed that at B and D the sparking diminished as the air space receded from a, vanished at the highest point, and again attained its original value at the point β. At C, on the other hand, the sparking continued throughout the rotation, being a little stronger at the highest and lowest points. If, then, there is any change of sign in the position C, it must occur with very much smaller displacements than in the other positions, so that in any case there is a distinction such as required between this and the other two cases.

Another very direct proof of the truth of Dr. Hertz's representation of the nature of the waves was obtained. If the secondary circle lies in the plane of the waves instead of in the plane of vibration, the E.M.F. must be equal at all points of the circle, and for a given position of the air space the sparking must be directly proportional to its intensity. When the experiment was made, it was found, as expected, that at all distances the sparking vanished at the highest and lowest points of the circle, and attained a maximum value at the points in the horizontal plane through the point of incidence.

The air space was then placed at such a point and close to the wall, and was then moved slowly away from the wall, when it was found that, while there was no sparking quite close to the metal plate, it began at a very small distance from it, rapidly increased, reached a maximum at the point B, and then diminished again. At C the sparking again became excessively feeble, and increased as the circle was moved still further away. The sparking continued steadily to increase after this, as the motion of the circle was continued in the same direction, owing, as before, to the direct action of the primary oscillation.

The curves shown by the continuous lines in Fig. 12 were obtained from the results of these experiments, the ordinates representing the intensity of the sparks at the distances represented by the corresponding abscissæ.

The existence in the electrical waves of nodes at A and C, and of loops at B and D, is fully established by the experiments which have been described; but in another sense the points B and D may be regarded as nodes, for they are the nodal points of a stationary wave of magnetic induction which, according to theory, accompanies the electrical wave and lags a quarter wave-length behind it.

This can easily be shown to follow from the experiments, for when the secondary circle is placed in the plane of vibration with the air space at its highest point, there will be no sparking if the E M.F. is uniform throughout the space occupied by the secondary. This can only take place if the E.M.F. varies from point to point of the circle, and if its integral round the circle

differs from zero   This integral is proportional to the number of magnetic lines of force passing backwards and forwards across the circle, and the intensity of the sparks may be considered as giving a measure of the magnetic induction, which is perpendicular to the plane of the circle.   Now in this position vigorous sparking was observed close to the wall, diminishing rapidly to zero as the point B was approached, then increasing to a maximum at C, falling to a well-marked minimum at D, and finally increasing continuously as the secondary approached still nearer to the primary.   If the intensities of these sparks are taken as ordinates, positive and negative, and the distances from the wall as abscissæ, the curve shown by the dotted lines in Fig. 12 is obtained, which therefore represents the magnetic waves.

The phenomena observed in the first series of experiments described in this paper may therefore be regarded as due to the resultant electric and magnetic actions.   The former changes sign at A and C, the latter at B and D, so that at each of these points one part of the action changes sign, while the other does not, and therefore the resultant action which is their product must change sign at each of these points, as was found to be the case.

When the secondary circle was in the plane of vibration the sparking in the vicinity of the wall was observed to be a maximum on the side towards the wall, and a minimum at the opposite side, and as the circle was turned from one position to the other there was found to be no point at which the sparks disappeared.   As the distance from the wall was increased, the sparks on the remote side gradually became weaker, and vanished at a distance of 1·08 metre from the wall.   When the circle was carried further in the same direction the sparks appeared again on the side remote from the wall, but were always weaker than on the side next to it; the sparking, however, no longer passed from a maximum to a minimum merely, but vanished during the rotation once in the upper and once in the lower half of the circle.   The two null points gradually receded from their original coincident positions, until at the point B they occurred at the highest and lowest points of the circle.   As the circle was moved further in the same direction the null points passed over to the side next to the wall, and approached each other again, until, when the centre was at a distance of 2·35 metres from the wall, the two null points were again coincident.   B must be exactly half-way between this point and the similar point previously observed, which gives 1·72 metre as the distance of B from the wall—a result which agrees, within a few centimetres, with that obtained by direct observation.   Moving further in the direction of C, the sparking at different points of the circle became more nearly equal, until at C it was exactly so.   In this position there was no null point, and as the distance was further increased the phenomena recurred in the same order as before.

Dr. Hertz found that the position of O could be determined within a few centimetres, the determinations of its distance from the wall varying from 4·10 to 4·15 metres; he gives its most probable value as 4·12 metres.   The point B could not be observed with any exactness, the direct determinations varying from 6 to 7·5 metres as its distance from the wall.   It could, however, be determined indirectly, for the distance between B and C being found to be

2·4 metres, taking this as the true value, A must have been 0·68 metre behind the surface of the wall, and 6·52 metres in front of it. The half wave-length would be 4·8 metres, and by an indirect method it was found to be 4·5 metres, so that the two results agree fairly well. Taking the mean of these as the true value, and the velocity of light as the velocity of propagation, gives as the vibration period of the apparatus 1·55 hundred-millionth of a second, instead of 1·4 hundred-millionth, which was the theoretically calculated value.

A second series of experiments was made with a smaller apparatus, and though the measurements could not be made with as much exactness as those already described, the results showed clearly that the position of the nodes depends only on the dimensions of the conductors, and not on the material of the wall.

Dr. Hertz states that after some practice he succeeded in obtaining indications of reflection from each of the walls. He was also able to obtain distinct evidence of reflection from one of the iron columns in the room, and of the existence of electro-dynamic shadows on the side of the column remote from the primary.

In the preceding experiments the secondary conductor was always placed between the wall and the primary conductor—that is to say, in a space in which the direct and reflected rays were travelling in opposite directions, and gave rise to stationary waves by their interference.

He next placed the primary conductor between the wall and the secondary, so that the latter was in a space in which the direct and reflected waves were travelling in the same direction. This would necessarily give rise to a resultant wave, the intensity of which would depend on the difference in phase of the two interfering waves. In order to obtain distinct results it was necessary that the two waves should be of approximately equal intensities, and therefore the distance of the primary from the wall had to be small in comparison with the extent of the latter, and also in comparison with its distance from the secondary.

To fulfil these conditions the secondary was placed at a distance of 14 metres from the reflecting wall, and, therefore, about 1 metre from the opposite one, with its plane in the plane of vibration, and its air space directed towards the nearest wall, in order to make the conditions as favourable as possible for the production of sparks. The primary was placed parallel to its former position, and at a perpendicular distance of about 30 centimetres from the centre of the reflecting metallic plate. The sparks observed in the secondary were then very feeble, and the air space was increased until they disappeared. The primary conductor was then gradually moved away from the wall, when isolated sparks were soon observed in the secondary, passing into a continuous stream when the primary was between 1·5 and 2 metres from the wall—that is, at the point B. This might have been supposed to be due to the decrease in the distance between the two conductors, except that as the primary conductor was moved still further from the wall the sparking again diminished, and disappeared when the primary was at the point C. After passing this point the sparking continually increased as the primary approached nearer to the

secondary. These experiments were found to be easy to repeat with smaller apparatus, and the results obtained confirmed the former conclusion—that the position of the nodes depends only on the dimensions of the conductor, and not on the material of the reflecting wall.

Dr. Hertz points out that these phenomena are exactly analogous to the acoustical experiment of approaching a vibrating tuning-fork to a wall, when the sound is weakened in certain positions and strengthened in others, and also to the optical phenomena illustrated in Lloyd's form of Fresnel's mirror experiments; and as these are accepted as arguments tending to prove that sound and light are due to vibration, his investigations give a strong support to the theory that the propagation of electro-magnetic induction also takes place by means of waves. They therefore afford a confirmation of the Faraday-Maxwell theory of electrical action. He points out, however, that Maxwell's, in common with other electrical theories, leads to the conclusion that electricity travels through wires with the velocity of light—a conclusion which his experiments show to be untrue. He states that he intends to make this contradiction between theory and experiment the subject of further investigation.

The Seventeenth Annual General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 13th, 1888—Mr. EDWARD GRAVES, President, in the Chair.

The minutes of the Ordinary General Meeting of November 22nd were read and approved.

The names of new candidates were announced and ordered to be suspended.

The SECRETARY : I have with great regret to announce the death of a member whose name must be well known to all of you, viz., M. Gaulard, who died on the 26th of last month.

The PRESIDENT : I am sure that all the members present will join in an expression of most sincere regret for the death of M. Gaulard—a death untimely and unfortunate. For many years M. Gaulard was the especial apostle of a new system applicable to electric lighting : he was the apostle of the introduction of "transformers." He lived to see many people give their adhesion to what he had proposed, but unfortunately he did not live to see its success.

The PRESIDENT announced that the ballot boxes would remain open till 8.30 p.m.

Mr. J. Hookey, Mr. G. Driver, Mr. J. Bolton, and Mr. F. H. Nalder were appointed Scrutineers.

The SECRETARY then read the following Report of the Council :—

REPORT OF THE COUNCIL.

The Council have satisfaction in reporting that the number of new members elected into the Society during the present year considerably exceeds the average, including 19 Foreign Members, 9 Members, 104 Associates, and 36 Students, or a total of 168 ; and 35 candidates have been approved for ballot at the first meeting next month.

16 Associates have been transferred to the class of Members, and 15 Students to the class of Associates.

The number of deaths, although not quite so great as that of last year, has been considerable. We have been thus deprived of 5 Foreign Members—Señor Ugarte, Inspector-General of Telegraphs at Buenos Ayres ; Mons. Jules Raynaud, of the French Telegraph Administration, whose death took place under lamentable circumstances ; Mr. L. Bigelow Jones, of Cordaville, Mass., U.S.A. ; Mons. Gustave Cabenellas, the well-known French writer on electrical subjects ; and Don José Rodriguez Vera, of the Spanish Government Telegraphs ;—7 Members, including our late President, Sir Charles Bright, whose services in nearly every branch of electrical engineering, but more especially in connection with land and submarine telegraphs, are well known ; Mr. Thomas Russell Crampton, who was intimately connected with the first successful submarine line, and was eminent also as a civil and mechanical engineer ; and Mons. Lucien Gaulard, whose name must always remain identified with the transformer system of electric distribution ;—8 Associates, amongst whom are Mr. George Mason, Manager of the Telephone Construction and Maintenance Company, and Mr. George Henley, of the Henley Telegraph Works Company.

4 Foreign Members, 9 Members, and 14 Associates have retired from the Society during the year.

The general meetings of the Society, which by the liberality

of the Institution of Civil Engineers continue to be held in the lecture hall of the Institution, have been 17 in number, inclusive of 4 extra meetings; and the Council have arranged, by the kind permission of the Institution, to hold next year 16 meetings on fixed dates, and, should occasion arise, 1 extra meeting.

The average attendance is increasing considerably, and the papers read during the session have, as usual, ranged over a variety of subjects, as will be seen by the subjoined list, and have in most cases given rise to important discussions :—

LIST OF PAPERS READ BEFORE THE SOCIETY DURING THE YEAR 1888.

DATE.	TITLE.	AUTHOR.
Feb. 9.—	On Alternate-Current Transformers, with Special Reference to the Best Proportion between Iron and Copper	G. KAPP, Member.
„ 9.—	The Distribution of Electricity by means of Secondary Generators or Transformers	J. K. D. MACKENZIE, Member.
„ 9.—	Formulæ for Converters	Prof. G. FORBES, Member.
Mar. 8.—	The Present State of Fire-Telegraphy	R. VON F. TREUENFELD, Member.
„ 22.—	On the Optical Demonstration of Electrical Stress	Prof. A. W. RÜCKER, Member, and C. V. BOYS.
April 12.—	Central Station Lighting : Transformers v. Accumulators	R. E. CROMPTON, Member.
May 10.—	Fire Risks and Fire Office Rules ...	W. H. PREECE, Past-President.
„ 24.—	New Standard and Inspectional Electrical Measuring Instruments ...	Sir W. THOMSON, Past-President.
„ 31.—	The Influence Machine from 1788 to 1888	S. P. THOMPSON, Member.
Nov. 8.—	On Ocean Temperatures in relation to Submarine Cables...	W. LANT CARPENTER, Member.
„ 22.—	On a System of Electrical Distribution	H. EDMUNDS, Member.

The Council have awarded the Society's annual premiums in respect of papers read during the twelve months ending the 31st May last as follows :—

The Society's Premium, value £10, to Professor Silvanus P.

Thompson, Member, for his paper entitled "The Influence Machine " from 1788 to 1888."

The Paris Electrical Exhibition Premium, value £5, to Mr. A. C. Cockburn, Member,* for his paper, " On Safety Fuses for " Electric Light Circuits, and on the Behaviour of various Metals " usually employed in their Construction."

The Fahie Premium, value £5, to Mr. Edward Stallibrass, Member, for his paper on " Deep-Sea Sounding in connection " with Submarine Telegraphy."

The Council desire further to highly commend the original communication by Mr. E. O. Walker, " On Earth Currents in " India," published in Part 71 of the *Journal*, and which is practically a continuation of previous valuable communications on the same subject contributed by that gentleman.

With the view of inducing those who take part in discussions at the Society's meetings to condense their remarks as far as practicable, and with the further object of affording more opportunities to the younger members of joining in the discussions, the Council have introduced the following rule, which, so far, appears to have met with the approval of the members generally, viz. :— " That when a speaker shall have occupied ten minutes, the Chair- " man shall sound a bell; and if the speaker do not close his " remarks within the next two minutes, the question shall be " put to the meeting whether or not he shall be permitted to " continue them; but the Chairman shall have authority to " suspend the rule on exceptional occasions."

The Committee on Electrical Nomenclature and Notation have prepared a draft report, which has been carefully considered, but it is not yet sufficiently advanced to be submitted to the full Committee for final approval.

The Committee appointed to revise the rules and regulations issued by the Society in 1883 for the prevention of risks of fire arising from electric lighting hoped to have been instrumental in bringing about the general adoption and recognition by all the fire insurance offices of one set of rules, to be modified from time

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* Associate when the paper was read.

to time as occasion required, by which much inconvenience both
to the offices and to electrical engineers and contractors would, in
the opinion of your Council, be avoided.   In their endeavours to
effect that desirable object they were warmly seconded by the
Fire Offices Committee; but, owing to the attitude taken up by
at least one of the leading offices, the Committee of the Society
found themselves compelled to limit their recommendations to a
revision and amplification of the general rules of 1883, leaving
the insurance offices to issue such other rules of a more detailed
nature as they might think fit.

Her Majesty's Government having decided to take no official
part in the forthcoming Universal Exhibition at Paris, the Society
was requested by Mons. Berger, the Director-General, to place
itself at the head of the movement being made to induce the
representatives of electrical science and industry in this country
to take part in the Exhibition, and practically to take charge of
that section.

Your Council consented, as a preliminary step, to ascertain how
far British electric manufacturers were likely to become exhibitors,
and accordingly appointed a Committee for that purpose, it being
understood that upon the result of their inquiries must depend
whether the Society would be justified in undertaking the func-
tions which the Director-General desired them to fulfil.

The response to the Committee's circular, which was widely
distributed, was so unsatisfactory that the Council were compelled
to decline M. Berger's request.

The President of the Society and several members of your
Council have, however, been nominated on a Committee called
" The Mansion House Committee of the 1889 Paris Exhibition,"
of which the Lord Mayor is the President, formed to watch the
interests of British exhibitors generally; and such of these who
purpose to exhibit in the Electrical Section are therefore sure to
be cared for.

The Council are glad to report that the class of Students con-
tinues to increase in numbers, and that at the Students' meetings
the proceedings have not this year been so largely confined as last
year to the discussion of papers read and already discussed at the

general meetings, but have included the reading of several original papers of considerable merit.

The necessity for further library accommodation has been felt for some years, and has at length compelled the Council to rent an additional room on the same floor as those already occupied by the Society. Two new book-cases have been purchased, and two others have been most kindly presented by our member Sir David Salomons. The shelf space thus provided will, it is hoped, meet the increasing requirements for some years to come.

The financial position of the Society continues sound ; but the additional expenditure involved by the increased number of meetings, the corresponding increase in the amount of printed matter in the *Journal,* and by its publication and issue at more frequent intervals, together with the rent of the new room and purchase of book-cases and furniture, will, it is expected, be found to have absorbed the increase of revenue arising from the large number of new members elected during the year.

Since the last Annual General Meeting two Conversaziones have taken place—one given by our lamented late President, Sir Charles Bright, on the 10th of December last, and one by our present President, Mr. Edward Graves, on the 15th of July last. Both of these réunions were held in the galleries of the Royal Institute of Painters in Piccadilly, and were largely attended.

The replies received to the circular issued by the Council in February last for the purpose of gauging the opinions of members of all classes as to the proposal to change the name of the Society, fully bore out the expectation of the Council, having practically amounted to a unanimous approval. The necessary legal steps have therefore been taken to enable the Society to assume on the 1st of next month the new title of "The Institution of "Electrical Engineers."

THE LIBRARY.

REPORT OF THE SECRETARY.

I beg to report that the number of accessions to the Library during the year amount to 76, the large majority of which have

been presented, my applications to authors and publishers having again been most kindly and liberally responded to.

The total number of patents applied for this year, up to the 6th inst., is 17,870, of which 965, or 5·40 per cent., were in respect of electrical inventions. The Society continues to receive, by the liberality of H.M. Commissioners of Patents, the specifications of all electrical patents.

The number of periodicals and Transactions of other Societies received by the Society is somewhat increased since last year, as will be seen by the list appended hereto.

The acquirement of an additional room, and the new book-cases purchased by the Society, together with those presented by Sir David Salomons, have met a want which has been felt for some considerable time, and constitute a material improvement in the condition of the Library.

The number of visitors to the Library during the year has been 345, of whom 74 were non-members.

<div align="right">F. H. WEBB,</div>

12th *December*, 1888. <div align="right">*Secretary.*</div>

APPENDIX TO LIBRARIAN'S REPORT.

TRANSACTIONS, PROCEEDINGS, &c., RECEIVED BY THE SOCIETY.

ENGLISH.

Asiatic Society of Bengal, Journal and Proceedings.
Cambridge Philosophical Society, Proceedings.
Greenwich Magnetical and Meteorological Observations.
Institute of Patent Agents, Transactions.
Institution of Civil Engineers, Proceedings.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
Liverpool Engineering Society, Proceedings.
Physical Society, Proceedings.
Royal Dublin Society, Transactions and Proceedings.
Royal Engineers Institute, Proceedings.
Royal Institution, Proceedings.
Royal Meteorological Society, Proceedings.
* Royal Society, Philosophical Transactions of.

* Presented by Professor D. E. Hughes, F.R.S. (Past-President).

Royal United Service Institution, Proceedings.
Society of Arts, Journal.
Society of Chemical Industry, Journal.
Society of Engineers, Proceedings.
University College Calendar.

AMERICAN.

American Academy of Science and Arts, Proceedings.
American Institute of Electrical Engineers, Transactions.
Canadian Institution of Civil Engineers, Proceedings.
Franklin Institute, Journal of.
John Hopkins University Circulars.
Library Bulletin of Cornell University.
Ordnance Department of the United States, Notes.
Smithsonian Institution Reports.

FRENCH.

L'Académie des Sciences, Comptes Rendus Hebdomadaires des Séances de.
Société Belge d'Électriciens, Bulletin de la.
Société Française de Physique, Séances de la.
Société des Ingénieurs Civils, Mémoires.
Société Internationale des Électriciens, Bulletin de la.
Société Scientifique Industrielle de Marseille, Bulletin de la.

LIST OF PERIODICALS RECEIVED BY THE SOCIETY.

ENGLISH.

Electrical Engineer.
Electrical Plant.
Electrician.
Engineer.
Engineering.
English Mechanic and World of Science.
Illustrated Journal of Patented Inventions.
Indian Engineer.
Industries.
Invention.
Machinery Market.
Mechanical Progress.
Military Telegraph Bulletin.
Nature.
Patent Office, Official Journal of.
Philosophical Magazine.
Railway Press.
Scientific News.
Telegraphie Journal and Electrical Review.

AMERICAN.

Electrical Engineer.
Electrical Review.
Electrical World.
Journal of the Telegraph.

Science.
Scientific American.
United States Patent Office, Official Gazette of.

<center>**FRENCH.**</center>

Annales Télégraphiques.
Cosmos les Mondes.
L'Électricité.
Journal de Physique.
Journal Télégraphique.
· La Lumière Électrique.
L'Électricien.
Revue Internationale de l'Électricité et de ses Applications.

<center>**GERMAN.**</center>

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Centralblatt für Elektrotechnik.
Electrotechnischer Anzeiger.
Electrotechnische Zeitschrift.
Verhandlungen des Vereins zur Beförderung des Gewerbfleisses.
Zeitschrift für Elektrotechnik.
Zeitschrift für Instrumentkunde.

<center>**ITALIAN.**</center>

Giornale del Genio Civile.
Il Telegrafista.

<center>**SPANISH.**</center>

Ingeniero y Ferretero Español y sud Americano.

Sir DAVID SALOMONS: With the exception of that portion of the Report alluding to the sad losses we have sustained by death, I think we may congratulate ourselves most heartily on its contents, and I have therefore very great pleasure in moving—" That the Report of " the Council, just now read, be received and adopted, and that it " be printed in the Journal of the Society." I am sure that this motion will be carried without a single dissentient; and of all the paragraphs of the Report the most interesting one to the members of the Society is that dealing with the change of name. You will no doubt have observed that our members have very largely increased during the past year, and no doubt that increase will continue to go on, for we all feel that electricity has now a much wider field than in former years; and probably those connected with electric lighting will increase in the future more than those in any other branch, for as electric lighting grows, electrical engineers will become more numerous, and gas engineers will go down, their places being filled by members of our body.

The motion, having been duly seconded, was carried unanimously.

Mr. W. T. GLOVER :· I beg to propose—" That the cordial " thanks of the Society be presented to the President, Council, " and Members of the Institution of Civil Engineers for their " great kindness and liberality in continuing to allow the Society " to hold its meetings at the Institution." We ought to be very grateful to that Institution for giving us the privilege of meeting here.

The motion, having been seconded, was carried by acclamation.

Mr. A. STROH : I wish to move the following resolution :— " That the thanks of the Society are due to those members who " so ably represent it abroad as Local Honorary Secretaries and " Treasurers for their kind attention to its interests."

The motion, seconded by Mr. SPAGNOLETTI, was carried unanimously.

Professor G. FORBES : I propose—" That the thanks of the " Society are due to Mr. Edward Graves, our President, for the " continued watchfulness exercised by him, in his capacity of " Honorary Treasurer, over the financial interests of the Society." We all feel that this is a pre-eminent resolution among those put forward this evening. Mr. Graves is marked through all his career as Treasurer of this Society for his remarkable precision, punctuality, and attention to business ; and just as during the whole of his Presidential career he has attended every single meeting of the Society, so also he has attended to his duties as Treasurer.

The motion, having been duly seconded, was heartily carried.

The PRESIDENT : I do not think I need say more than that I thank you, gentlemen.

Mr. GISBERT KAPP : I beg to propose the following resolution :— " That the thanks of the Society be presented to Mr. J. Wagstaff " Blundell and to Mr. Fred. C. Danvers for their kind services as " Honorary Auditors."

The motion, seconded by Mr. PREECE, was carried unanimously.

Mr. W. M. MORDEY: I beg to move—" That the thanks of " the Society are due to Messrs. Wilson, Bristows, & Carpmael " for the kind and valuable services rendered to the Society " through Mr. G. L. Bristow, especially in reference to the " change of the Society's title." This is a motion made annually, and which, although always heartily responded to, sometimes appears to be rather formal, because the services performed have been of a nominal character. But a considerable amount of business has, I believe, been transacted this year by our Solicitors in connection with the change of title, which business has been carried through successfully, and the vote therefore should be more heartily given than usual, if possible.

The motion, seconded by Mr. CHARLES BRIGHT, was heartily carried.

The PRESIDENT: We now have to resume the adjourned discussion on Mr. Edmunds's paper, " On a System of Electrical " Distribution." Perhaps there are some members present who were absent from the last meeting who would like to make some remarks before Mr. Edmunds gives his reply. If so, we shall be glad to hear them.

Mr. W. M. MORDEY: The meter which Mr. Edmunds proposes to use appears, from the description, simply to add up the number of charges received by the cells in a given time. This counter does not, so far as I understood the author, take note of any loss that may occur on the line or in the cells; and in the event of the people at the station supplying, say, 50 or 60 amperes instead of 70, it appears that the meter would still charge the customer for 70 amperes. What safeguard has the consumer that the meter really measures anything? No doubt some check is provided.

Mr. H. EDMUNDS, in reply, said : Since reading my paper on Electrical Distribution, I have carefully considered the discussion thereon, and also noted the various comments in the journals and other quarters on the subject. It has occurred to me that I did not give sufficient data or details in order to make the system fully understood in all its bearings. I think that Mr. Mordey's question is one, among others, that will show that, and I may at

Mr.
Edmunds.
once reply to that question and say that the arrangement we use
for metering is a solenoid through which the current passes when
it comes from the charging main into the batteries. This solenoid
is so weighted that if anything under a 70-ampere current comes
in, it does not respond : the consequence is, that if the station
sends out 60 amperes they do not have any record at all ; if they
send out more than 70 amperes, it is their loss ; and if they send
out exactly 70 amperes, then we get the number of revolutions
recording the time each distributor has been under charge.   We
know the potential required in each distributor, and multiplying
this by the 70-ampere current gives the units passing into the
cells.   Mr. Mordey very properly put it that it is hardly fair to
charge what goes into the cell, but we ought rather to charge for
what comes out.   I think that this can be got over by charging a
fairly low rate per unit.   It has been suggested to charge the
consumers 6d. per unit, as by this means we can satisfy them
as to cost.

I claim for the system something more than a mere set of
complicated mechanical devices for attaining certain ends which
might be attained by much simpler means, or might be shown
to be unnecessary ; therefore, if you will allow me, I will not
merely reply to the several questions raised, but will add some-
thing to my previous remarks, in order to make you more
intimately acquainted with the subject, for one is sometimes apt
to think it superfluous to inform an audience about small details
and points which really are necessary to be recognised in order
to understand the subject as a whole.   If, therefore, in this reply
I should either repeat myself or give you obvious or well-known
information, I must beg you to excuse this on the ground that
I wish to be fully understood.

I have decided to reply to the several speakers as a whole
rather than in detail, and have analysed and arranged the
various points raised in the discussion and comments accord-
ingly.   First let me express my thanks for the recognition
accorded by the members to the ingenuity of the scheme, for
such recognition is most encouraging, and is one of the most
valuable recompenses one can obtain.   I also appreciate the

various suggestions made as to the desirability of simplifying Mr.
Edmunds. the details and removing unnecessary parts. I think there was a feeling at the last meeting that the machine was " fearfully and " wonderfully made ; " that the general principle might be all right, but that the means for carrying out the details was very questionable. I am pleased to say that since I was here I have simplified the apparatus, so that, instead of measuring 30 in. × 18 in. × 9 in., it only measures 12 in. × 12 in. × 6 in. over all. Instead of thirty-two contacts, there are only eight ; and instead of costing £25 to make, they ought to be produced at less than half that sum. In fact, I have really modified the thing in such a way that now, instead of it being an experimental model, it is a practical working instrument. What is still more important, the new form, being free from all springs and cams, can never, under any circumstances, open the circuit and cause the lights to go out. This, I think, deals with Mr. Preece's and Mr. Reckenzaun's objections as to the apparatus being too complicated.

The absence of sparking also enables me to depend on the several contacts working freely, the reason for non-sparking and non-fluctuation being that we never break or open the circuit (for all the changes are made either by giving an alternative path to the current through the cells or through a suitable resistance). There is also an absence of fumes to tarnish the working parts. Mr. Reckenzaun remarked that he thought the fumes of the gases evolved from the cells being charged were rather hurtful to an arrangement of that kind. The cells evolve gas but slightly, and the bubbles, when liberated, are trapped by a cover of solid paraffin, which prevents evaporation and the escape of acid spherules. The hydrogen gas evolved discharges against the solid paraffin cover in bubbles, and there condenses, and the hydrogen coming off is practically harmless, and does not seem to affect instruments working in the vicinity of the batteries. It is also interesting to note that although we are charging with 70 amperes, carrying on the charge for twenty-four hours, and bringing up the batteries to a high point of charge, we have very little gas liberated.

Mr.
Edmunds.

Now comes the question as to the necessary charging potential. I said in my paper that we could charge cells *intermittently* to their full charge with a *mean* voltage per cell of 2·25, instead of a constant 2·5 per cell, as is required where cells are charged continuously; and I wish to explain this, and to show why this differs from ordinary practice alluded to by Mr. Reckenzaun. In the first place, I said a *mean*, or average, voltage, and not a constant potential. We have a constant current of 70 amperes, and that is our only constant; the charging potential varies with the change in counter-E.M.F. in the cells, due, as Mr. Reckenzaun points out, to the hydrogen gas in a nearly *fully* charged cell on the negative plate giving a fictitious E.M.F. But these are the very points of difference between ordinary practice, such as Mr. Reckenzaun refers to, and the system employed here. Let us try and disabuse our minds about a fully charged cell as ordinarily understood, which would mean a condition where, after many hours' continuous—not intermittent—charging, we get in a set of cells a counter-E.M.F., while being charged, of, say, 2·5 or more (this being the point when it is usual to consider a cell as fully charged); and when we cease charging——if we refer to the diagram I can explain what I mean. I believe that the practice generally is to commence charging a cell, and to continue charging for a given number of hours. We will say that we are charging, as shown, with a 60-ampere current. You charge for one hour, when you find that a rise has taken place in the potential at the terminals of the cell from about 2 volts to 2·15 volts. As you go on charging it continues practically constant up to 2·2 volts—that is, when a five-hours' charge, or 300 ampere-hours, has been put in. Since I read my paper I have tried an experiment as follows:—I took cells and charged them intermittently, giving a cell a two-minutes' charge. I took the potential with a voltmeter before placing the cell in the charging circuit, and found it to be about 2 volts. Then charging with a 70-ampere current—which was indicated, and was quite constant—there was an immediate rise from 2 volts to 2·1 volts; then after another two minutes I got from 2·1 to about 2·2, or two-tenths. I am speaking now of a cell that had perhaps

about 300 amperes in it at the time.   Now, if I continued the
charge beyond two minutes, I found that the cells had a tendency
to gas, and at that point the fictitious E.M.F. that Mr. Reckenzaun
alluded to came in.   But if I discontinued charging at the end of
two minutes, then I had got 70 amperes into the cell with a
charging potential of 2·2 volts; the cell then went through the
cycle, as shown on the diagram (Fig. 4), into parallel with another
cell next to be charged, and the voltage at the moment of leaving
is about 2·1.   When such cell is placed in parallel with another
of 2 volts there is a tendency for the two to equalise one another,
but it is only momentary, for the next cell comes in and receives
its 70-ampere charge for two minutes without having raised its
potential more than 2·2.   So I find that I can charge the 70
amperes into the cells as a whole in twenty-four hours, in the
way which we desire, and yet never have had a charging potential
higher than 2·25 volts in order to do it.   That, I think, will
probably answer Mr. Reckenzaun's question.

Now I would ask another question, and it is this, Why do we
cease charging?   What is understood by a full or an empty cell?
For under some circumstances it would answer our purpose to
continue charging many more hours, if we were sure we could
economically do so—that is, if we could get back again the 98
per cent. of the energy put in which Mr. Preece told us about.
We cease charging because the energy put into, or rather
expended on, a cell after a certain period is not returned to us as
current, but is practically wasted in overcoming the fictitious
counter-E.M.F., and in doing work in decomposing water and
producing heat without useful effect.   Now note the difference
in practice.   We only charge a cell whose E.M.F. is, say, two volts,
for two minutes.   What happens then?   There has not been
time for the hydrogen to appear and develop the fictitious
E.M.F. Mr. Reckenzaun refers to before the cell is removed
from the charging circuit, isolated, and then placed in parallel
momentarily with another cell, into which it would then discharge
were there any difference of potential; and then it takes its place
in the local group, either resting or discharging, according to the
then requirements of the local circuit.   The same conditions

Mr.
Edmunds.

apply to each group of cells in turn, and that is why I maintain we can charge cells with a constant 70-ampere current and a *mean* potential of 2·25 volts per cell. But now comes the question, To what point do we charge them? How much energy can we get into them, and how much can we get back again? What are the dimensions of the charging station plant, and what does it cost to operate this system? At the last meeting several members wished to know this, and since then I have been much questioned about these matters, and also why we are justified in adopting a system with complex details in lieu of proceeding on more simple lines. I will try and explain.

With regard to the charge in the cells Mr. Bate remarked: " It seems that not only will the system of secondary battery " distribution not reduce the cost, but that it is liable to increase " it, not only over transformer, but also over other battery " systems. Here, if the batteries are discharged at more than " an average rate of one-third of the charging current, we shall " not only have no accumulation, but we shall have a loss, and " therefore, taking 70 amperes as being put in, the battery must " not discharge at more than 20 odd amperes. The cells would " therefore be very much larger than if, as is done in the " Colchester system, the batteries were allowed to discharge at " their full rate and then replaced by others of equal capacity; " by which means it would seem that we could get twelve " hours' charge and discharge (Mr. Preece, however, says we can " get 98 per cent. efficiency at a low discharge), whereas here " we could get only six hours' charge out of twenty-four."

If Mr. Bate's conclusions were correct, I certainly should not feel justified in taking up your time in bringing such a system to your notice; and here again I feel that fuller explanation is necessary, and I will therefore take an example of what we would do in actual practice. Suppose we determined on only having 2,000 volts potential at our station, and a 70-ampere current. This would represent 140,000 watts. We can use for this at our central station, say, six dynamos, each giving 400 volts by 70 amperes, and each driven by a separate engine. This would provide an ample margin of power, allowing us to use a ten-mile circuit in

addition to the batteries. This circuit would demand an addi- Mr.
tional 300 volts, if we had a circuit of 19/14 stranded con- Edmunds.
ductors, such as we are now using, bringing the total voltage up
to 2,300 volts. In addition to this plant we would have two spare
engines and two spare dynamos, making in all eight dynamos and
eight engines. We would also provide four boilers, of which three
would at any time be more than sufficient for the work required,
leaving one over, with its engines and dynamos, as a reserve.

Now let us consider what we can do with such an amount of
plant in the central station, and see how many batteries we can
charge on circuit. This will give us some indications of the
value of the system on a large scale. We can divide our 3,600
cells so that they can supply 112 centres. Each centre would be
capable of supplying current locally of 48 volts by 70 amperes
for eight hours, provided the station were run for twenty-four
hours. This would practically enable us to maintain at each of
those centres 70 16-C.P. lamps, or a total of 7,840—say 8,000—
16-C.P. lamps on an extended circuit of ten miles at 112 centres.

Now what would such a plant cost? and what would be the
running expenses? Also, what would be the scale of profit?
This, I think, is the most important feature for consideration in
such a paper as this, and I am glad to say I am prepared with
figures from actual experience on this point. From first-class
contractors I learn that we can have our eight dynamos, eight
50-H.P. engines, four 100-H.P. boilers, the preparation of the
building, instruments, and ten miles of circuit with the fitting
wiring, and sundries included in the station and on the circuit, for
£12,000. In addition to this, our 3,600 batteries, at £6 apiece,
would come to the large item of £21,600; our 112 distributors, at,
say, £10 each, £1,120. The erection of batteries would cost, say,
another £1,000; but the consumers would in most cases be asked
to supply a room, and, at any rate, pay for the fitting up of the
batteries, so that these need not be charged wholly to the supply
company. This would give us a grand total of £35,720. Now
let us see what it would cost to run this station at twenty-four
hours a day for 365 days in the year, with these engines and
these dynamos and the three boilers running continuously day

Mr.
Edmunds.

and night, so that practically we should be taking the outside and maximum cost that this plant would represent. We will allow per H.P. 4 lbs. of coal, which, at 240 H.P. for twenty-four hours a day, and 20s. a ton, gives us a total of £3,756 for fuel. We will then take oil and waste at 6s. 6d. per night, which would give us £119. Take the water at 1s. per 1,000 gallons: that would give a total of £300. We will allow for wages £1,565. We will take rent and taxes at, say, £500. Those items will give us an annual expense of £6,240—that is to say, if we run this station without any regard to the demand for twenty-four hours a day for the whole year.

Coals, oil, and water...	...	...	£4,175
Wages ...	...	...	1,565
Rent and taxes ...	...	...	500
			£6,240

Now we will say that we get a revenue of 6d. per unit of the current passing into the batteries, which by the system of metering proposed we could ask. This would give us, on the output of such a station, a gross revenue of £84 per day, or £30,660 per annum. But such a revenue would only be obtained if the total amount of energy in the station were consumed eight hours per day throughout the year. Therefore I have made a calculation as to the revenue accruing from a less consumption than this. We will take, for example, an average of one hour per day—£3,832 10s. per annum; two hours per day—£7,665 per annum; three hours per day—£11,497 10s. per annum; four hours per day—£15,330 per annum; and this latter is probably about the average throughout the year that we could depend upon. This would only necessitate the station working upon an average twelve hours a day, instead of twenty-four, for which we have reckoned; in which case we should cut down our fuel charge one-half, reducing that to £2,000, and our wages we might reduce by one-third, leaving that item at £1,000; the rent remaining the same would give us a gross cost of £3,500 per annum, with a gross revenue of £15,330, leaving £12,000 to the good. This, however, would be by no means all

profit, as we must now allow for depreciation of the machinery Mr.
Edmunds. and batteries. It is usual to write off 10 per cent. for machinery, which, for our engines, dynamos, and boilers, would be £600. The depreciation of the batteries is more of an unknown quantity. My own idea is that 10 per cent. per annum ought to cover these; but we will put it at 15 per cent.—£3,240—which is a liberal depreciation. This would leave us, therefore, a gross profit of £7,990 on the supply of current for an average of four hours per day. Placing the capital at £40,000, this would give us a profit of 20 per cent. This I think a most attractive feature in the system.

Of course it is often said that figures can be made to prove anything, but I think I have dealt very liberally with all the cost of the plant and the depreciation; and should we increase the demand from an average of four hours per day, you will understand that the profits mount up very rapidly. This, I think, is the best reply I could make to the questions raised by Mr. Bate, and to similar remarks made by others.

Mr. Preece and Mr. Bate thought that the proposition to run an engine without a governor was a novel one, but I can assure them that there is no difficulty in achieving this in practice. Nor do we pay too heavily in our coal bill, as Mr. Kapp feared, for I find that, in our high-speed engines actually in use, instead of the coal being 4 lbs. per H.P., it is about 3 lbs. to evaporate 30 lbs. of water. We get over the difficulty of slow speed by running a suitable number of small engines; and the E.M.F. required to charge the ten-mile line we contemplate here being about 300 volts, we could always ensure our engines, as we arrange them, running at least at 150 revolutions a minute. We have not found in practice that any difficulties have occurred in this direction.

Professor Forbes raised a question about lamps having $2\frac{1}{2}$ watts efficiency. I remarked *about* $2\frac{1}{2}$ watts, and did not state it to be actually that. I find, on investigation, that with an actual $2\frac{1}{4}$ watts they blacken too soon; and, though the filaments last 1,000 hours, the lamps are not so useful, owing to the blackening and higher resistance for more than 400 or 500 hours;

Mr.
Edmunds. but at about 2·7 or 2·75, with a perfectly constant potential, so
that we do not overstep the mark even for a short time, we can
safely rely on the lamps coming up to their 1,000 hours. I find
that a very slight difference in temperature when working at
this high potential is enough to cause all the difference between
length or shortness of life in the lamp, and therefore, with such a
system as the one contemplated, it is important to know that we
can depend on a certain potential which is practically constant at
all times.

Several allusions were made to the undesirability of intro-
ducing motor-generators into this system. I do not think that
they are essential, but I think they form valuable auxiliaries, and
I think it is well to look to the absorption of more current from
the charging station by means of motor-generators and electro-
motors than would be taken by the demand on the batteries, in
order to increase our revenues. Further, the cost of batteries is
the most heavy one in this system ; and, although we have
enormous advantages when they are to be used to their full
capacity, yet, should we have to meet a large demand for a very
short time, it would probably be better to have a small number
of batteries with the auxiliary of a motor-generator, than to have
the larger number with their interest and depreciation going
on, which interest and depreciation could not be equal to the
revenue accruing from them. That is why I deemed it desirable
to have an auxiliary of this kind, but it is only an auxiliary, and
not a necessary part of the system.

The important features of this system are the reduction and
simplification of the plant in the charging station, not necessi-
tating that the engines should be running all the time at an
absolutely constant speed, and requiring the particular attention
such conditions demand ; the reduction of the potential at the
charging station to a point not exceeding 2,000 volts, which, as
we have seen, is sufficient with a 70-ampere current to supply an
eight hours' demand of 8,000 16-C.P. lamps on a ten-mile circuit.
Or, by increasing the batteries, and running on a double line (or
in the alternative method I spoke of last week, where the
charging station to supply four hours' discharge, which is the

amount we have based our revenue upon), would allow of double the amount of work in a twenty-four hours' run at the station, thus giving us 16,000 lamps. All these points are very desirable in the interests of economy and safety. The absence, also, of any high potential on the local lines of the system is a feature of great importance, getting over the vexed question Mr. Preece referred to, of highly insulated wire. The assurance that, even should there be an accident at the station, or an interruption of the line, there is always a reserve throughout the entire system; the bringing of the local batteries near to their work, considerably reducing the cost of mains—all point to the commercial, as well as to the electrical and mechanical, advantages of this system.

I think that what I have said has covered the several questions that I was asked, but if not I should be glad to answer any further remarks.

The PRESIDENT : As there are no further remarks, it is incum- bent upon me to propose that we accord to Mr. Edmunds a most sincere and hearty vote of thanks for the interesting paper and the novel points that he has introduced to us. I think they are the more especially noticeable because in the discussions upon the papers read before this Society for the last twelve or fifteen years I have never before heard the original speaker converted.

> You may discuss and express what opinions you will,
> This only is certain : he retains his own still.

Mr. Edmunds—thanks partly, perhaps, to the interval that has occurred between the close of the discussion and his reply—has been able to test the value of the criticisms expressed by his hearers, and has used them to such good purpose that he can reduce the cost of his apparatus one-half. I think, therefore, there is some benefit in adjourning the reply to a discussion, and perhaps in the future we may take advantage of it to a large extent.

The motion was heartily carried.

The PRESIDENT : As the Scrutineers will take some time to carry out their duty, I will ask Mr. Preece to make some remarks upon a subject about which I feel sure he is burning to speak.

Mr. W. H. PREECE: It is certainly news to me, Sir, to know that I am burning to do anything; but if I ever do burn or get excited on any question at all, it generally is on some question affecting the interests of this Society. As you are all aware, I am one of the original members of this Society. For many years now—I forget how many—I have taken a most active part in its proceedings. During the time that the Society has existed there have been several changes at different times proposed and carried out, not always, in my opinion, for the good of the Society, but the great majority certainly have been for the good of the Society. But there is one change that has been made that I want to call your particular attention to, and I think I shall show you—for I certainly feel—that it has been detrimental to the interests of the Society. It is a matter that affects the selection of the Council itself. Now it will be obvious to all, that in bringing this subject before the Society I can have no personal interest to serve, for I have passed through every honour that the Society can possibly shower on me, and if I do bring this before you it is in the interests of those who are to form the future Councils rather than of those who are in the present Council or who have passed through it.

It was our custom at one time to submit to the ordeal of the votes of the Society a full and complete list of those who should constitute the Council, nominating only a sufficient number of new names to fill the vacancies. The Council itself displays the very greatest interest, consideration, and care in the selection of those whom they consider to be worthy to conduct the proceedings of the Society and to look after its interests, and for many years the Council, year after year, selected and submitted for election the names of a sufficient number of Members and Associates to fill the vacancies occasioned by the compulsory retirement of a certain number of the old Council. That practice went on very well, and it is a practice that is followed by most of our principal Societies—the Royal Society, the Society of Arts, the Physical Society, and nearly every Society, with one exception, and that one exception was our own parent Society, the Society to whose hospitality we are indebted for the privilege of holding our meet-

ings in this hall—the Institution of Civil Engineers. The Mr. Preece. practice of the Institution of Civil Engineers has been invariably to submit to the members of the corporation a long " house list " —that is, not only the names of the existing Council who are eligible for election, but also the names of many other eminent men whom the Council thinks sufficiently eminent to place upon the Council, and who may be left to the members of the Institution to elect. That Council, and its two children—the " Mechanical " Engineers " and the Society almost immediately to be called " The Institution of Electrical Engineers "—followed suit. In our Society, owing to a little agitation that originated in this room at an Annual Meeting, it was suggested that the members themselves had not sufficient choice or sufficient voice in the selection of those who were to represent them on the Council. I can speak for the Council of the past that there is nothing that affords them more gratification than to have their proceedings criticised in any way, and to have suggestions submitted to them for their consideration ; and if such suggestions are good, they have always been, as you have had experience, properly and thoroughly carried out. The suggestion was acted upon, and by a majority, though not so large as the usual majority, the Council agreed to submit, and have for two or three years submitted, to the members a list containing a good many more names than the number that constitute the Council itself. This may, perhaps, afford a little more interest and excitement in our Annual Meeting ; but I am sorry to tell you that the new system has worked very badly indeed.

In the first place, men, and men of eminence and position, will not submit to a process of competition, and when they are asked to allow their names to appear on the list to be submitted to the members for election, they decline to be put into competitition with members of their own profession, and with men of equal eminence to themselves ; and the result has been that the last two or three years we have met with refusals from those whom you would all, I feel sure, be proud to see on your Council. Again, there are men who have agreed to allow their names to appear on this house list, who have been through the process of

Mr. Preece. competition, and who have been rejected. I am sorry to tell you that it has been perfectly evident to some of us that there are two or three men (whom it would be invidious to mention), formerly prominent in their attendance at our meetings, who were accustomed to take part in our discussions, who showed themselves to be anxious in our interests, and who were therefore selected by the Council as men worthy to represent you on the Council, whose names accordingly have appeared on previous lists, but who, having been rejected, have, in consequence, apparently lost all interest in the Society, for they have not come amongst us again. That I take to be one of the most serious consequences of the new process that we have adopted. Another very serious objection to it is this: our Society is increasing rapidly in numbers; the various sections that are represented here, the various fields in which electricity is being applied, are gradually extending—we have various sections growing amongst us—and it is quite evident that if our Council is to properly represent the interests of the Society it ought as far as possible to represent fairly and properly each section of the different practical applications of electricity. That has always been the desire of the Council, and it is that principle which has always actuated them when selecting names for the balloting list; but this object has been made difficult, and indeed I may say has been defeated, by our present system, which, as I have already stated, deters some of our most representative men from allowing their names to be put forward. We have a very striking instance of that in the correspondence that many of you may have noticed in the technical journals this week. There was a letter signed by "A Telegraphist," pointing out that Telegraphy, the origin of the Society — Telegraphy, whose members are found scattered all over the face of the earth, the great majority of whom are away from England and cannot be here to record their votes, and who, if it were simply a question of votes, would naturally be outvoted by those who are interested in the industries in London, and who generally attend here and record their votes--Telegraphy is not represented among the new names on the balloting list. Well, it so happens that two gentlemen who have held extremely high positions in that very

important branch of electrical engineering, who would have been an honour to the Society, and who certainly would have looked after our interests, were invited by the Council, but declined, under the present system, to appear on the balloting list to be subjected to this evening's competition. There are other reasons, which I will not now give you; but the principal reason which I would urge for reverting to our old practice is not only that every other Society carries it out, with the two exceptions I named, but that I can mention as a fact that even one of those exceptions—the Institution of Civil Engineers itself, our parent Institution—has had under its consideration the very motion I am going to propose, viz., that instead of submitting to the Society for selection as Members of Council a list of names more numerous than the number constituting the Council, a sufficient number only of new names be submitted to fill the vacancies.

I therefore propose that in future the Council revert to the old practice of submitting for election a complete list of the Council.

The PRESIDENT: By a "complete list of the Council" I presume you mean a list containing no more new names than there are vacancies?

Mr. W. H. PREECE: Yes, exactly.

Professor G. FORBES: I rise, Sir, to second this proposal, and I shall do so very shortly. Mr. Preece has put forward the arguments very forcibly indeed, and I feel perfectly sure that everyone must appreciate them. I should like, however, to mention— I think I am at liberty to do so—the reason why I take upon myself to support his suggestion. The subject was brought before the Council at a recent meeting, and the cogency of Mr. Preece's arguments was generally seen to be very strong indeed; but we had moved in the other direction a few years ago, in consideration of a general expression of opinion at one of the Annual Meetings of the Society, and I felt that our action, if the Council determined to revert to the ancient system, would not be appreciated by the general body of members unless the reasons were fully explained; and I suggested that if the members of the Society knew

really what was the actual position of affairs, and what were the reasons that led many of us to think that the old system was best, the sense of the general body would be in the same direction, and that they would feel the great undesirability either of preventing eminent men from allowing their names to appear on the nomination list, or of subjecting them to an unpleasant feeling owing to their having been put forward and rejected. I have great pleasure in seconding the motion of Mr. Preece.

Mr. Donovan.   Mr. H. C. DONOVAN suggested that the balloting papers should state some particulars as to the position and qualifications of those gentlemen recommended for election on the Council, as many of them might not be known to the majority of those who had to vote. He thought, also, that it would assist the Council in preparing the list if some time previous to the Annual General Meeting the members generally were invited by circular to propose names.

Mr. Newman Lawrence.   Mr. H. NEWMAN LAWRENCE was inclined to agree with the views expressed by Mr. Donovan, but he recognised the difficulties which the Council had to deal with in the matter, not the least of which arose from the apathy evinced by a large section of the members, who, while quite ready to criticise the action of others, avoided the responsibility of voting themselves. He had learnt with surprise and regret from Mr. Preece's remarks that eminent members of the Society were so very sensitive as to decline being put into competition with others for election on the Council.

Mr. Lant Carpenter.   Mr. LANT CARPENTER would be glad of a more precise definition of the terms of Mr. Preece's motion. Was it proposed that members should still have the right of substituting other names for those put forward by the Council?

Mr. Preece.   Mr. W. H. PREECE: I might answer that at once, Sir, and say that, by the suggestion of the Secretary, I propose to word my proposal in this way: " That a list of the Members and Associates " proposed by the Council for election on the Council be in " future limited to the number of vacancies to be filled." Also, in answer to Mr. Lant Carpenter, I would say that I do not propose to make the slightest change in the form or in the mode

in which the election is made.   At the present time it is at the Mr. Preece.
liberty of anybody to erase any of the new names and to
substitute any others, and I think it is necessary to maintain
that freedom.

With reference to the suggestion thrown out by Mr. Newman
Lawrence, I consider, speaking for myself, that it is a very good
one indeed, and I think that the Council generally would be
also glad to receive suggestions from the members of the Society
a month or two before the election. If that were carried out, it
would to a great extent relieve us from the difficulty. It would
be a very easy thing for the Secretary, six weeks or so before the
Annual Meeting, to issue a circular asking members to submit
names, and from those names the Council could make their
selection; but that practice is not in vogue in other Societies.

I should like to see our Council varied even more frequently
than it is, because I think there is nothing so good for a
Society as to pass its members through the Council, so that they
may see how the work of the Society is done, and see the interest
and zeal which is always shown for its benefit by the Council.
But although we sometimes hear of the Council being a closed
body—that we are a clique among ourselves—I do not suppose
there is any Society in London, or elsewhere, of the proportions
of this Society, where the changes in the Council are more
numerous than in yours. We have always been a shifting body,
and shall always continue to be so, for every year six at least of
the old Council retire and six new members are elected, and
generally the changes are seven in number.

As to being "thin-skinned," I think it is human nature to
be thin-skinned and annoyed on defeat. It does not matter
what, or how trifling, the contest may be, we all, in our own
hearts, feel chagrin and annoyance at not being successful.

A MEMBER thought the last suggestion of Mr. Donovan would A Member.
only make the task of the Council more invidious than at present.

Mr. W. T. GLOVER asked if this were a question that could be Mr. Glover.
properly decided without notice having been given. It was one
upon which he believed many members besides those present felt

Mr. Glover. strongly, and upon which they would be glad to offer an opinion and record their votes if opportunity were given them.

The President. The PRESIDENT remarked that there was no special urgency for the matter being decided that evening, although it would be necessary that it should be settled a considerable time before the next Annual General Meeting. If Mr. Glover thought it desirable to postpone it, perhaps he would make a motion to that effect.

Mr. Glover. Mr. GLOVER moved—" That notice be given of the proposed " change, and that the members have the opportunity of voting " upon it at, say, two months hence."

Professor Forbes. Professor FORBES suggested that Mr. Glover's object would perhaps be best attained if a circular were issued to all members, placing the matter before them and inviting their opinion thereon. They would thus have the opportunity of making their views known to the Council, even if they could not attend to vote personally.

Mr. Preece. Mr. PREECE quite approved of Professor Forbes's suggestion, and would ask leave to modify his motion accordingly. He assured Mr. Glover that he had no desire to precipitate a decision, but as the matter had occupied his thoughts a great deal during the last two years, he had been glad of the opportunity afforded that evening of ventilating his views. The question was a very important one, and therefore required " simmering." He hoped the statement he had made would reach the members, and thought the proposed memorandum should be accompanied by a form of reply for expressing assent or dissent, as was done in respect of the proposal to change the title of the Society.

Mr. Glover. Mr. GLOVER expressed himself quite satisfied with that proposal.

Mr. Preece. Mr. PREECE : The Secretary inquires whether the issue of the memorandum should not be confined to those Members and Associates residing in the United Kingdom, they alone being entitled under the Articles of Association to vote at the Annual General Meeting ; and it is evident that if we could obtain the opinions of all such Members and Associates, we should be able to come to a decision much sooner than if we waited for replies from Australia, Japan, and other distant countries.

Professor FORBES thought that such limitation would be
scarcely fair to the large class whose claims Mr. Preece advocated in his opening remarks.

Mr. C. E. PITMAN (of the Indian Government Telegraph
Department) said that, as one of the absentee class, he thought his brother officers and he should have an opportunity of expressing their opinion on the subject.

Mr. HOWARD SWAN said that it appeared to him that the
question involved two separate and distinct points, which had become rather mixed up in the discussion, viz., the sensitiveness of those who shrank from a competitive election, and the fair representation of the two principal classes of electrical engineers. Might not the latter point be met by following the example of the Electrical Section of the London Chamber of Commerce, and selecting a certain number of telegraph engineers, and a certain number of electric light engineers, &c. ?

Mr. W. H. PREECE : I beg to say that it is absurd to suppose
that the Society simply consists of telegraph engineers and . electric light engineers. There are manufacturers of all classes, the makers of motors, the telephone world, the cable world, those who represent the application of electricity to various other useful purposes, the medical profession, and heaps of industries originating and growing every day, each of which classes has claims for representation.

The PRESIDENT : Perhaps you will cast your resolution into
its final form ?

Mr. MORDEY hoped that the members who had not given the
subject the same attention that Mr. Preece had, would not be invited to give their opinion upon it on the statement of the case made by him, which, however able and fair, only expressed one view. If notice had been given that the matter was to be raised that evening, there would probably have been many present to advocate other views.

Mr. PREECE quite agreed with Mr. Mordey that the proposed
memorandum should place the matter as fairly as possible before the members, and he begged therefore, in place of his previous proposal, to move—"That a circular be issued by the Council to

Mr. Preece. "all Members and Associates at 'home and abroad, putting the "facts of the case fairly before them and inviting their opinion "thereon."

Mr. Glover.   Mr. W. T. GLOVER had much pleasure in seconding the motion.

Professor Forbes.   Professor FORBES expressed his approval of the motion, as he had all along been desirous that no change should be made until the general body of members had had an opportunity of learning what the objections really were to the present system, and which he believed would have great weight with them in forming an opinion upon the matter.

The motion was then put, and carried unanimously.

The Scrutineers handed in their report of the result of the ballot for Council and Officers for the year 1889, which the PRESIDENT announced to be as follows :—

President :

Sir WILLIAM THOMSON, D.C.L., LL.D., F.R.SS. (L. & E.).

Vice-Presidents :

Dr. J. HOPKINSON, M.A., F.R.S.   Professor W. E. AYRTON, F.R.S.
WILLIAM CROOKES, F.R.S., Pres. C.S.   ALEXANDER SIEMENS.

Ordinary Members of Council :

Captain PHILIP CARDEW, R.E.   GISBERT KAPP, Assoc.M.Inst.C.E.
W. LANT CARPENTER, B.A., B.Sc.   Professor JOHN PERRY, M.E., D.Sc., F.R.S.
R. E. CROMPTON.
Sir JAMES DOUGLASS, F.R.S.   Sir DAVID SALOMONS, Bart., M.A.
J. A. FLEMING, M.A., D.Sc.
Professor GEORGE FORBES, M.A., F.R.SS. (L. & E.).   AUGUSTUS STROH.
Professor SILVANUS P. THOMPSON, B.A., D.Sc., F.R.A.S.
Captain Sir DOUGLAS GALTON, K.C.B., D.C.L., LL.D., F.R.S.

Associate Members of Council :

SYDNEY EVERSHED. | GUY CAREY FRICKER.
Captain A. E. WROTTESLEY, R.E.

Honorary Auditors :

J. WAGSTAFF BLUNDELL (Wagstaff Blundell, Biggs, & Co., Chartered Accountants), 12, Delahay Street, Westminster, S.W.

FREDERICK C. DANVERS, India Office, S.W.

Honorary Treasurer :

EDWARD GRAVES, Past-President.

Honorary Solicitors :

Messrs. WILSON, BRISTOWS, & CARPMAEL, 1, Copthall Buildings, E.C.

The PRESIDENT: I propose that a hearty vote of thanks be given to the Scrutineers for their labour in the examination of the ballot lists.

The motion was carried unanimously.

Mr. NEWMAN LAWRENCE: If I am in order, I would ask whether there is any reason why we should not be informed of the numbers who have voted. I think it would be some indication to us as to the working of the present system of election if we knew the numbers who vote compared with the total number of members of the Society.

Mr. J. HOOKEY: I have only an approximate idea, but I should not say there were more than two hundred, and thirty of the papers were invalid.

A ballot took place, at which the following candidate was elected :—

Student:

Horace C. Hollingsworth.

At a Special General Meeting of Members, held at the offices of the Society, 4, The Sanctuary, Westminster, on Thursday, December 20th, 1888—Mr. EDWARD GRAVES, President, in the Chair—

The SECRETARY read the notice convening the meeting.

The PRESIDENT explained that, in consequence of the technical objection having been raised that a clear interval of fourteen days had not elapsed between the Special Meetings of the 8th and 22nd November, it would be necessary to take the resolutions which were confirmed on the 22nd, as having been passed only on that day, and he now moved that the said Special Resolutions be now confirmed, viz. :—

    1. " That the name of the Society be changed to ' The " ' Institution of Electrical Engineers.' "

    2. " That the office of Honorary Secretary be abolished, and " that the Articles of Association be altered by omit- " ting all reference to the Honorary Secretary in " Articles 36, 38, 40, and 43."

The motion, having been seconded by Mr. ALEXANDER SIEMENS, was carried unanimously.

The PRESIDENT then moved the following special resolution, viz. :—

    " That the Regulations of the Society of Telegraph-Engineers " and Electricians, as contained in their Memorandum " and Articles of Association, be altered by substi- " tuting the name ' The Institution of Electrical " ' Engineers' for 'The Society of Telegraph-Engineers " ' and Electricians,' and also by substituting the word " ' Institution' for the word 'Society' wherever the " same respectively occur in the Regulations."

The resolution, having been seconded by Mr. C. T. FLEETWOOD, was carried unanimously.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY FROM JULY 1 TO DECEMBER, 31, 1888.

(Works marked thus (*) have been purchased. Of those not purchased or received in exchange, where the donors' names are not given, the works have been presented by the authors.)

IT IS PARTICULARLY DESIRABLE THAT MEMBERS SHOULD PRESENT COPIES OF THEIR WORKS TO THE LIBRARY AS SOON AS POSSIBLE AFTER PUBLICATION.

Aldrich [Capt. Pelham]. [*Vide* Hydrographic Department, Admiralty.]

Bolton [Sir Francis]. [*Vide* Scratchley.]

Carruthers [Rev. G. T.] The Planets upon Cardioides. 4to. 4 pp.
<div style="text-align:right">*Subathu, India*, 1888</div>

———— The Cause of Light. 8vo. 29 pp.   *Subathu, India*, 1888

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———— Ink-marking Telegraph Instruments. A few Practical Hints. 12mo. 6 pp. [Read before the Victorian Railways Electrical Society, 13th April, 1887.]   *Melbourne*, 1887

Ellis [William]. Address delivered at the Annual General Meeting of the Royal Meteorological Society on Jan. 18, 1888, including a Discussion of the Greenwich Observations of Cloud during the Seventy Years ending 1887. 8vo. 13 pp.   *London*, 1888

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<div style="text-align:right">*Berlin*, 1886</div>
<div style="text-align:right">[Presented by Professor Silvanus Thompson.]</div>

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<div style="text-align:right">[Presented by Messrs. Macmillan & Co. (Publishers).]</div>

Hedges [Killingworth]. Central-Station Electric Lighting; with Notes on the Methods used for the Distribution of Electricity. 8vo. 128 pp.
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* **Hering** [Carl]. Principles of Dynamo-electric Machines, and Practical Directions for Designing and Constructing Dynamos. 8vo. 279 pp.
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[Presented by the Executors of the late Sir William Siemens.]

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Western Australia. [*Vide* Knight.]

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JUNE to DECEMBER, 1888.

(*Philosophical Magazine, Vol. 25, No. 157, June, 1888.*)

W. E. SUMPNER—Variation of the Coefficients of Induction. W. E. AYRTON and J. PERRY—Efficiency of Incandescent Lamps with Direct and Alternating Currents. E. C. RIMINGTON—Measurement of Power supplied to Primary Coil of a Transformer. W. E. AYRTON and J. PERRY—Magnetic Circuit of Dynamo Machines.

(*No. 158, July, 1888.*)

H. TOMLINSON—Sudden Loss of Magnetic Properties by Iron and Nickel at certain Temperatures. T. H. BLAKESLEY—Magnetic Lag. C. V. BURTON—Electro-motive Forces of Contact. W. E. AYRTON and J. PERRY—Governing of Electro-Motors. S. P. THOMPSON—Formulæ of Bernoulli and Haecker for the Lifting Power of Magnets.

(*No. 159, August, 1888.*)

H. A. ROWLAND and L. BELL—Action of a Magnet on Chemical Action. C. TOMLINSON—Some Effects of Lightning. T. E. THORPE and A. W. RÜCKER—Additions to the Kew Magnetometer. W. W. H. GEE and H. HOLDEN — Experiments on Electrolysis. S. P. THOMPSON — Continuous-Current Transformers. O. J. LODGE—Theory of Lightning Conductors.

(*No. 160, September, 1888.*)

J. BORGMANN—Influence of Light on Electric Discharge. J. A. EWING and W. LOW—Influence of a Plane of Transverse Section on the Magnetic Permeability of an Iron Bar. E. H. COOK—Existence of an Undulatory Movement accompanying the Electric Spark.

(*No. 161, October, 1888.*)

O. HEAVISIDE—Electro-magnetic Waves, and the Forced Vibrations of Electro-magnetic Systems (*continued*).

(*No. 162, November, 1888.*)

O. HEAVISIDE—*Vide supra.* A. TANAKADATÉ—Mean Intensity of Magnetisation of Soft Iron Bars.

(*No. 163, December, 1888.*)

S. P. THOMPSON—Conditions of Self-Excitation in a Dynamo Machine. O. HEAVISIDE — *Vide supra.* J. BROWN—Figures produced by Electric Action on Photographic Dry Plates.

PROCEEDINGS OF THE ROYAL SOCIETY.
(*Nature, 7th June,* 1888.)

J. BURDON-SANDERSON—Electro-motive Properties of the Leaf of Dionœa.

(*Nature, 12th July,* 1888.)

T. ANDREWS—Electro-chemical Effects on Magnetising Iron.

(*Nature, 19th July,* 1888.)

Dr. G. GORE—Minimum Point of Change of Potential of a Voltaic Couple. **Dr. G. GORE**—Change of Potential of a Voltaic Couple by Variation of Strength of its Liquid. **Dr. G. GORE**—Influence of Chemical Energy of Electrolytes upon the Minimum Point and Change of Potential of a Voltaic Couple in Water.

(*Nature, 26th July,* 1888.)

J. C. EWART—Electric Organ of the Skate.

(*Nature, 2nd August,* 1888).

S. H. BURBURY—Induction of Electric Currents in Conducting Shells of Small Thickness. **Dr. G. GORE**—Effects of different Positive Metals upon the Changes of Potential of Voltaic Couples. **Dr. G. GORE**—The Voltaic Balance. **J. A. EWING**—Magnetic Qualities of Nickel.

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J. MONCKMAN—Effect of Occluded Gases on the Thermo-electric Properties of Bodies, and on their Resistances.

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J. A. EWING and W. LOW—Magnetisation of Iron and other Magnetic Metals in Very Strong Fields.

(*Bulletin de la Société Internationale des Electriciens, Vol. 5, No. 49, June,* 1888.)

E. HOSPITALIER — Electric Energy Meters. **TROUVÉ** — Electric Auscanoscope. **TROUVÉ**—New Switch. **H. BRIVET**—Aluminium Bronze and Ferro-Aluminium.

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E. DESROZIERS—New Type of Dynamo Machine. **E. DUCRETET**—Improvements in Electro-magnetic Fuse-Exploders. **E. ESTAUNIÉ and E. BRYLINSKI**—Researches on Telephonic Currents.

(*No. 51, August—October,* 1888.)

ESTAUNIÉ and BRYLINSKI—Telephonic Currents (*continued*). **C. REIGNIER and P. BARY**—Electro-magnetic Induction.

(*No. 52, November,* 1888.)

G. CHAPERON and E. MERCADIER — Electro-chemical Radiophony. **R. V. PICOU**—Insulation of Electric Light Installations. **ESTAUNIÉ and BRYLINSKI**—*Vide supra.*

(Comptes Rendus, Vol. 106, No. 23, 4th June, 1888.)

H. WUILLEUMIER—Determination of the Ohm by Lippmann's Electro-dynamic Method.   A. STOLETOW—Actino-electric Currents through Air.   G. CHAPERON and E. MERCADIER—The Electro-chemical Radiophone.   FABINGI and PARKAS—Constant-Current Battery with a Carbon Negative Electrode.

(No. 25, 18th June, 1888.)

G. MANŒUVRIER and J. CHAPPUIS—Electrolysis by means of Alter-nate Currents.   A. D'ARSONVAL—Connection between Animal Elec-tricity and Surface Tension.

(No. 26, 25th June, 1888.)

G. BERSON and A. DESTREM—Electrolysis of Solutions of Potash.

(Vol. 107, No. 1, 2nd July, 1888.)

E. BICHAT and R. BLONDLOT—Combined Action of Inflation and Illumination of the Electric Shells enveloping Conductors.   G. MANŒUVRIER and J. CHAPPUIS—Electrolysis by means of Alter-nate Currents.

(No. 2, 9th July, 1888.)

E. BOUTY and L. POINCARÉ—New Method of Measuring the Resist-ance of Molten Salts.   A. STOLETOW—Actino-electric Currents.   G. MANŒUVRIER and J. CHAPPUIS—Spontaneous Explosions Pro-duced by the Electrolysis of Water by Alternate Currents.

(No. 4, 23rd July, 1888.)

T. MOUREAUX—Magnetic Determinations in the Western Mediterranean.

(No. 5, 30th July, 1888.)

GOUY—Conservation of Electricity.   E. BOUTY and L. POINCARÉ—Conductivity of Mixtures of Molten Salts.   BICHAT and GUNTZ—Production of Ozone by Electric Discharges.

(No. 9, 27th August, 1888.)

A. BANARE—Submarine Telephony.

(No. 14, 1st October, 1888.)

E BICHAT—Actino-electric Phenomena.   A. RIGHI—New Electric Pheno-mena produced by Radiations.

(No. 18, 29th October, 1888.)

E. L. TROUVELOT—Application of Photography to the Study of Electric Discharges.

(No. 20, 12th November, 1888.)

VASCHY—Means of Diminishing the Harmful Effects of the Extra-Current in Electro-Magnets.   F. GODFROY—New Method of Increasing the Output of Long-Line Telegraphs.   E. L. TROUVELOT—Effects pro-duced by Electric Discharges on Eastman's Paper.

(Journal de Physique, Vol. 8, June, 1888.)

A. CORNU —Synchronisation of Standard Clocks.   NEYRENEUF—Action of Induced Currents on an Aluminium Voltmeter.   P. A. SILJESTRÖM—Electric Currents Produced by Pressure.

(*August*, 1888.)

A. RIGHI—Experiments on the Reflection of Light from the Equatorial Surface of a Magnet. **P. DUHEM**—Relation between the Peltier Effect and the Potential Difference of Two Metals in Contact.

(*September*, 1888.)

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(*October*, 1888.)

GOUY—Regulator for Electric Currents. **A. ERIMOFF**—Magnetism of Gases.

(*November*, 1888.)

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(*Journal Télégraphique, Vol. 12, No. 5, 25th May*, 1888.)

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(*No. 6, 25th June*, 1888.)

Dr. ROTHEN—Telephony (*continued*).

(*No. 7, 25th July*, 1888.)

Dr. ROTHEN—Telephony (*continued*). **Dr. JEFE DE LA ESCUELA**—Storms in Spain in 1886.

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Dr. ROTHEN — Telephony (*continued*). **M. P. SANTANO** — Duplex Telegraphy.

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Dr. ROTHEN—Telephony (*continued*). **H. DISCHER**—Prevention of the Interception of Telegrams in Time of War.

(*No. 9, 25th October*, 1888.)

Dr. ROTHEN—Telephony (*continued*).

(*No. 10, 25th November*, 1888.)

Dr. ROTHEN—Telephony (*continued*).

(*La Lumière Electrique, Vol. 28, No. 22, 2nd June*, 1888.)

K. SOSNOWSKI—Testing Primary Batteries. **C. E. GUILLAUME**—Electrical Measurement of Temperatures. **Dr. A. TOBLER**—Baudot's New Printing Telegraph. **A. LEDUC**—Measurement of Magnetic Fields. **E. ZETSCHE**—Railway Block Signal Apparatus. **N. VON KLOBUKOW**—New Apparatus for Electro-chemical Experiments.

(*No. 23, 29th June*, 1888.)

C. E. GUILLAUME—Electrical Measurement of Temperatures (*continued*). **X. SOSNOWSKI**—Testing Primary Batteries (*continued*). **Dr. A. TOBLER**—Baudot's New Printing Telegraph (*continued*). **C. REIGNIER**—Study of a Disc Machine. **E. MEYLAN**—Thury's Arc Lamp. **X. S.**—Hassner's Cell.

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M. COSSMANN—Electric Block System. **E. GIMÉ**— New Temperature Transmitting Apparatus. **A. LEDUC**—Magnetic Field of a Faraday Electro-Magnet. **C. REIGNIER and P. BARY**—Coefficients of Induction of Transformers.

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(*No. 26, 30th June*, 1888.)

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(*Annalen der Physik und Chemie, Vol.* 34, *Pt.* 3, *No.* 7, 1888.)

G. QUINCKE—Electrical Researches. H. HERTZ—Rate of Propagation of Electro-dynamic Action. A. J. VON OETTINGEN—Interference of Oscillatory Electric Discharges. C. L. WEBER—Changes of Resistance of Alloys on Fusion. W. KOHLRAUSCH—Accumulators for Use in Physical Laboratories.

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